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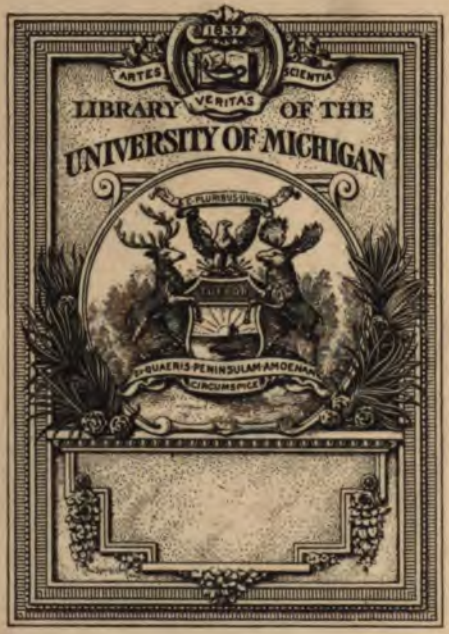
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THE

RETROSPECT

OF

PRACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M. D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.,

AND

JAMES BRAITHWAITE, M. D., Lond.

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PRACTICAL MEDICINE,

&c., &c.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON THE ETIOLOGY OF SCARLET FEVER.

By Dr. CHARLES MURCHISON, Physician to the London Fever Hospital, &c.

[It is, of course, an indisputable fact that scarlet fever depends upon a peculiar poison capable of being communicated by the sick to persons in health. It is, in fact, the very type of what has always been, and still is, regarded as a contagious and infectious malady. The words contagion and contagious are here used in their modern sense, to signify a disease capable of self-propagation, and not to imply the necessity of actual contact. Strictly, a disease which propagates itself *through the atmosphere*, and not by contact, is *infectious*. Scarlet fever is both contagious and infectious.]

The proofs of the contagious character of scarlet fever may be classified as follows:—

1. *It constantly happens that several cases of scarlet fever follow one another in succession in the same house.* Illustrations of this statement must be familiar to every reader, and until scarlet fever can be shown to depend upon some local cause, such facts afford a strong presumption that the disease is propagated from the individuals first infected. The evidence, however, is not crucial; for on the supposition that the disease may have a local origin, the circumstance of a number of individuals being successively attacked in the same house admits of another explanation than that of contagion.

2. *The attendants on the sick, or persons in close communication with them, are more liable to contract the disease than persons equally predisposed residing in the same house, who are not in communication with the sick.* For example, Rilliet and Barthez state that out of 53 children who contracted scarlet fever in the Children's Hospital at Paris, 34 were in wards into which scarlet fever cases were admitted, 14 were in wards in close communication with the preceding, and only 5 were in wards having but slight communication with those first mentioned. Facts of this nature are of almost daily occurrence. They would be even more common but for two reasons: 1st, That a large proportion of the attendants are protected by a previous attack; and 2nd, that they have usually arrived at an age in which the predisposition to scarlet fever is comparatively small. For these and other reasons to be mentioned hereafter, unprotected children residing in the same house, but having no personal communication with the sick, may be attacked, while the adult attendants escape. But instances of this sort do not invalidate the general law, that the danger is great in proportion to the exposure to the poison.

3. *Scarlet fever is often imported by infected persons into localities previously free from it.* For example, it is often observed that when scarlet fever breaks out in a large school, and when the school in consequence is broken up, the disease is simultaneously imported into localities widely distant, and where the disease was before unknown. Facts of this sort are of constant occurrence, and afford unquestionable proof of the contagious character of the disease. They admit of no other explanation.

4. *Certain localities, having little or no communication with others in the*

same part of the world where scarlet fever prevails, are known to be remarkably exempt from it. A return has just been published, compiled from the records in the Registrar-General's office, showing the causes of death in the decennial period 1851–60 in each of the 623 registration districts of England and Wales. The Scilly Isles, containing a population of about 2500, constitute the only district out of the 623 in which there has not been a single death from scarlet fever (measles, or small-pox) during the ten years in question. This fact requires no comment; of itself it speaks volumes.

5. *The contagious nature of scarlet fever is indicated by the success of measures taken to prevent its propagation.* Patients very frequently contracted scarlet fever in the London Fever Hospital when it was the practice to treat cases of that disease in the same wards with other patients; but during the last two years, although the number of scarlet fever admissions has been far greater than in any previous years, such occurrences have been almost unknown, because the scarlet fever cases have been isolated in distinct wards from the other patients. I have often had occasion to observe that when scarlet fever has broken out in a school, or in a large family of unprotected children, its spread has been arrested by the early isolation of the sick. Occasionally the disease continues to spread, in consequence of the isolation not having been sufficiently prompt, or because others than the individual first attacked have been exposed to the original source of infection.

6, and lastly. *The contagious character of scarlet fever is demonstrated by the fact that inoculation with the poison taken from the sick is capable of producing the disease.* The evidence on this head is sufficient to leave little doubt about the matter, but the subject requires further investigation. Scarlet fever is said to have been successfully inoculated in Germany by Stoll, but I have been unable to find any details of his experiments. Speaking of the vesicles occasionally observed in the eruption of scarlet fever, Dr. Robert Williams observes: "This serum has, by Sir Busick Harwood and other physicians, been used to inoculate healthy children, in the hope that by this method a milder disease, as in the small-pox, might be produced. Scarlet fever has in many instances resulted from this experiment, but the disease has been as formidable as that which occurs spontaneously, and consequently this practice has of late years not been repeated." Although this statement and opinion have been adopted by many subsequent writers, the only account of Dr. Harwood's observations which I have been able to discover is contained in Dr. Erasmus Darwin's "Zoonomia," and has reference to a patient suffering from scarlet fever with a vesicular eruption, discharging a quantity of serous fluid. This patient on a Wednesday evening became very delirious, and could with difficulty be kept in bed. He was attended by a poor man who had cut his hand with a broken bottle. The wounded hand was repeatedly brought in contact with the patient's skin in the efforts to restrain his movements. The next day the man's hand was much inflamed and his arm swollen. On the following Monday he was seized with scarlet fever, of which he died on the Wednesday morning. It is to be observed, however, that this man had also been exposed to the poison in the ordinary way. Thirty years ago, M. Miquel d'Amboise, in a communication addressed to the French Academy of Medicine, stated that he had inoculated a number of unprotected children with fluid taken from the vesicles of scarlet fever. At the end of thirty hours a red areola, resembling exactly the eruption of scarlatina, appeared around the punctures. For three days this redness went on increasing, but it disappeared on the fifth day. The inflammation was not traumatic, for a second inoculation in the same individual failed to take effect. The operation was said to confer complete immunity from subsequent attacks of scarlet fever; two children, aged one year and a half and nine years respectively, who never before had the disease in a regular form, were not attacked, although placed in the same bed as a patient suffering from the disease. Rostan refers to cases where the eruption of scarlet fever appeared seven days after inoculation; and Copland had a patient with a severe attack of scarlet fever "produced by the contact of a small portion of the discharge *from the throat of* a person with malignant anginous scarlatina."

In the face of such evidence, it must be admitted that scarlet fever can be propagated from the sick to persons in health by means of a specific poison. The laws which appear to regulate the action of this poison are worthy of study.

1. *Mode of transmission of the poison.*—The experiments and observations mentioned above leave little doubt that the poison is contained in the secretions of the skin and throat. It is highly probable that the other secretions are also impregnated with it, and particularly the breath and the glairy fluid which in severe cases is discharged from the nostrils; but this is an opinion founded on no positive data. Opposite statements are made by different writers as to the presence of the poison in the blood. On the one hand, scarlet fever is said to result from inoculation with the blood of infected persons; on the other, it is believed that the results of such inoculation are negative. The opinion that the poison circulates in the blood is supported by the fact that children have been born suffering from scarlet fever. The poison is conveyed through the atmosphere or by means of clothing or other articles, and enters the body of the recipient through the lungs or skin, or, after being swallowed with the saliva, through the mucous membrane of the digestive canal. The possibility of the poison entering by the skin admits, as we have seen, of demonstration; and when a healthy person suddenly sickens with scarlet fever, after inhaling the breath of an infected individual, the conclusion seems inevitable that the poison has entered by the respiratory mucous membrane.

2. *The infecting distance.*—It is difficult to speculate on the distance to which the poison of scarlet fever may be transmitted through the atmosphere; but the most careful observers agree in making it extremely short. According to the late Dr. Haygarth, of Chester, who was the first to investigate this subject, the infecting distance of small-pox does not exceed half a yard. That of scarlet fever is probably not greater. Dr. Darwin was, I believe, right in maintaining that the poison of scarlet fever is much less volatile than that of measles. From the supposed shortness of the infecting distance of scarlet fever, it would be reasonable to infer that when the disease appeared in a large family or establishment of children, its spread might be at once arrested by the prompt isolation in separate apartments of the persons first attacked. But, unfortunately, a contrary result has too often been observed. Dr. R. Williams tells us that, in his day, the London Foundling Hospital presented excellent opportunities for isolating patients attacked with scarlet fever, but that the spread of the disease was not arrested when once it had broken out amongst the children. Again, at Ackworth School, the spread of the disease could not be prevented, although the infirmary was 250 yards distant from the school: 184 out of 216 scholars were attacked. A similar observation was made at Donaldson's Hospital, Edinburgh, in 1852; and, in fact, there are few practitioners who have not met with instances of a like nature. The spread of the disease, however, under such circumstances, is not due to the distance interposed between the sick and the healthy children being insufficient. It is more probably owing to the healthy children imbibing the poison before the removal of those who are actually sick; and still more to the circumstance that the poison can be transmitted by means of fomites.

3. *Fomites.*—The opinion has long existed that the poison of scarlet fever can attach itself to clothing, bedding, and furniture; and that the disease can be transmitted by a third person who is not himself attacked. The evidence in support of this view appears to be insurmountable. Dr. Willan's great experience led him to state "that a nurse having received on her clothes or pocket-handkerchief the vapour from the lungs, the phlegm from the throat, or the discharge from the nostrils of a patient labouring under scarlet fever, would infect any child predisposed to the disease whom she attended or caressed." Dr. Sims relates several instances of midwives, in Aberdeenshire who, after nursing patients in scarlet fever, carried the disease to women whom they attended in labour. Many medical men have narrated to me circumstances which made it clear to my mind that they

have been the means of transmitting the disease to their puerperal patients; and, indeed, the belief in the possibility of such an occurrence is so universal that not a few practitioners of midwifery decline to attend cases of scarlet fever. Scarlet fever appeared twice within three years in the family of a medical friend of mine. On both occasions its origin could only be accounted for by the circumstance that the father was in attendance on cases of scarlet fever, although he himself exhibited no symptoms of the disease. I have met with several well-authenticated instances in which the poison of scarlet fever appeared to be transmitted to a long distance in a letter or a lock of hair. Dr. Mason Good records a case where the poison was imported from England into a family in Dublin by means of a box of toys. A remarkable instance is related by Dr. Richardson, where the poison was transmitted from an infected family to a house many miles distant by means of a letter. The letter on arrival was seized by a child, and conveyed by her to her mother. Both were susceptible, both contracted the disease, and both died of it.

Not only may the poison attach itself to surrounding objects, but its activity under such circumstances exhibits a durability truly surprising. In the course of practice I have repeatedly found scarlet fever reappear in a family immediately after their return to their infected home, although several months had elapsed since the first outbreak, and the ordinary cleansing and disinfecting measures had been resorted to. In one instance—that of a medical man's family—the disease recurred this way twice in succession, the family having absented themselves on both occasions for upwards of two months after the recovery of the sick children. Benedict cites a case where several children were seized with the disease immediately after their return to a room in which a death from scarlet fever had occurred two months before, and which, in the interval, had been cleaned with all the care imaginable. Dr. Elliotson mentions an instance where a patient with scarlet fever was admitted into a ward of St. Thomas's Hospital, and for nearly two years afterwards children and young men placed in the same ward continually took the fever, though the ward was thoroughly whitewashed and cleaned once a year. An impressive instance of the same nature is narrated by Dr. Richardson. A man and his wife, with four children, inhabited a country cottage. One of the children was seized with a malignant scarlet fever, and died. The three remaining children were removed at once to a village several miles distant; but after some weeks one of them was permitted to return home. Within twenty-four hours this child was seized with the disorder, and died with equal rapidity to the first. The cottage was now thoroughly cleansed, the walls limewashed, and every article of clothing washed or destroyed. After four months a third child was brought home, who also sickened with scarlet fever within twenty-four hours and died. It was believed that the poison was retained in the thatch, which extended down close to the children's beds. Lastly, Hildenbrand, no mean authority on the etiology of fevers, states that after travelling from Vienna into Podolia, he put on a black coat which he had last worn while attending upon a scarlet fever patient a year and a half before. He was at once seized with scarlet fever, which spread from him through the province, where it was before unknown. A consideration of such facts readily explains why it was that in a recent outbreak of scarlet fever at a fashionable watering-place, the disease was confined to the visitors residing in lodging-houses, while the residents for the most part escaped.

4.—*Latent period.*—It is a point of no small importance to fix with accuracy the latent period of the several infectious diseases. That of small-pox has been most studied, and is found to be subject to but little variation. Advantage is taken of this knowledge in the practice of vaccination. The development of the areola, which is necessary for the protective effect of vaccination, occupies nine days; but as the latent period of small-pox is twelve or fourteen days, it follows that, if a person exposed to the poison of small-pox be successfully vaccinated *at once*, he has time to escape.

The latent period of scarlet fever appears to be shorter than that of the *ther infectious diseases. Most* observers have agreed in fixing it at between

two and seven days, as follows: Dr. Binns, two days; Withering, three or four days; Bateman, three to five days; Heberden, Gendron, and Frank, four days; Cazenave and Schedel, three to six days; Dr. Watson, four to six days; Willan, never more than six days; Guersant and Blache, two to seven days; Dr. Copland, two to eight days. The writers now cited, however, do not state the facts on which their opinion is founded. In the cases alluded to by Rostan, where the poison was introduced by inoculation, seven days are said to have elapsed before the appearance of the eruption. It must be remembered, however, that when small-pox is inoculated, the latent period (seven days) is not the same as under ordinary circumstances (twelve days). A few cases have been recorded in which the latent period has been said to extend to three or four weeks, but it may be doubted if some of the cases referred to were really examples of scarlet fever, and in others, if the poison had not been lurking in the clothes rather than in the bodies of the individuals who were subsequently attacked.

In some of the cases where I have succeeded in ascertaining the latent period of scarlet fever, its duration has been shorter than might be inferred from any of the authors cited above. Considering the difficulty of collecting information on the subject, and the fact that cases suitable for the investigation rarely present themselves, the details of my observations appear to me worthy of being recorded.

In the only case where Professor Trousseau has been able to trace the period of incubation of scarlet fever, it did not exceed twenty-four hours. The case is remarkable. An English gentleman with his daughter was returning from Pau to London, and was joined at Paris by another daughter who came direct from London. Scarlet fever was prevalent in London, but there was not a case of it at Pau. The second daughter was seized with scarlet fever in crossing the Channel, and joined her relatives in Paris seven or eight hours later. She occupied the same room in the hotel as her sister, who was also attacked within twenty-four hours. Dr. Richardson relates that once when he was attacked, the effect of the poison was instantaneous. He had applied his ear to the chest of a patient suffering from scarlet fever, and was conscious of a peculiar odour emitted by the patient. He immediately felt nauseated and chilly, and from that moment he was able to date the commencement of his illness.

It follows from the evidence now adduced, that the latent period of scarlet fever varies from a few minutes to five days, and that it rarely, if ever, exceeds six days.—*Lancet, Aug.* 13, 1864, *p.* 175.

2.—THE INFLUENCE OF AGE ON THE LIABILITY TO SCARLET FEVER.

By Dr. Charles Murchison, Physician to the London Fever Hospital, &c.

Scarlet fever is most common in the second, third, fourth and fifth years of life, and after ten years its frequency rapidly declines. From an analysis of the returns of the Registrar-General, extending over ten years, it appears that for every 100,000 children under five years of age, in England and Wales, 419 die annually of scarlet fever. It has been alleged that childhood does not predispose to scarlet fever, but that adults merely escape in consequence of being protected by a previous attack; but the mere fact of such large numbers of infants being attacked shows a remarkable aptitude for the disease in early life (not observed in small-pox or typhus), while if we estimate the mortality of scarlet fever at only six per cent., the number of persons annually attacked in England and Wales is considerably less than one-half of the number of births, so that a large proportion of the adult population must be unprotected. Daily experience shows that unprotected adults escape in families where all the children are attacked.

The above results show how erroneous is the opinion originally expressed by Withering, and still by many believed to be true, that scarlet fever does not often *attack infants at the breast* or under two years of age. No fewer

than 30,974 (or more than one-fifth of the entire number) deaths from scarlatina are noted as occurring under two years of age. The number of deaths, however, in the first year of life is less than one-half of that in the second, and constitutes less than one-ninetieth of the entire mortality, whereas the deaths from scarlatina in the second year of life amount to one-fifteenth of the entire mortality.

Examples of scarlet fever in the fœtus have been recorded on good authority, although it might be sometimes difficult to distinguish a scarlet rash from the redness natural to newly-born infants. On the 28th of April, 1839, the youngest son of the late Dr. George Gregory was born, "evidently suffering under some form of fever. The throat was affected on the following day, obviously from angina maligna. Eruption was never developed. The child drooped and died on the 1st of May." On two occasions I have known females during an attack of scarlet fever, delivered of living and healthy children.

Although scarlet fever is a rare disease after forty years of age, instances are not wanting of its occurrence at an advanced age. Out of 2402 cases admitted into the London Fever Hospital during twenty years, four were above fifty, and one above sixty. Four fatal cases are reported as having occurred in persons above eighty-five years.—*Lancet, June* 18, 1864, *p.* 696.

3.—ON THE CONDITION OF THE STOMACH AND INTESTINES IN SCARLATINA.

By Dr. Samuel Fenwick, late Lecturer on Pathological Anatomy at the Newcastle-on-Tyne College of Medicine, in connexion with the University of Durham.

The object of this paper is to prove the following propositions:—

1st. That the mucous membrane of the œsophagus, stomach, and intestines is inflamed in scarlatina.

2d. That desquamation of the epithelium of these parts takes place.

3d. That notwithstanding the anatomical changes in the mucous membrane of the stomach, the formation of pepsine is not prevented.

4th. That the condition of the skin is similar to the condition of the mucous membrane in scarlatina.

In support of the first proposition, the microscopic examinations of the mucous membranes of the œsophagus, stomach, and intestines were detailed in ten cases of death from scarlatina during the first week of illness, and in six cases who died in the second and third week of the fever. The first effects of the scarlatina poison upon the mucous membrane of the stomach were shown to be the congestion of the bloodvessels and the stripping of the epithelium from the tubes and the surface of the organ, and also the softening of the tissues. The tubes are greatly distended by granular and fatty matters, or by small cells intermixed with granules, and in some cases they are lined by a newly-formed membrane. Sometimes no normal cells can be distinguished; in other cases they are present, but are scattered irregularly. After the second or third week the tubes are found less distended than at an earlier period, and whilst their closed ends are still loaded with granular matters, which greatly obscure the gastric cells. These become more evident towards the surface of the mucous membrane. The cells at this period are sometimes very large, sometimes loaded with fat or coated with granules, and seem to have but little adhesion to their basement membrane, as they readily separate from the tubes, but adhere closely to each other. The effects of the inflammation upon the intestines seem, in slighter cases, to consist in the effusion of granular and fatty matters into the mucous membrane; but in more severe cases the tubes of Lieberkühn are obstructed by epithelial cells, whilst extravasations of blood takes place in the villi, and these, with the rest of the mucous membrane, are loaded with small cells and *granules.* In one case the mucous membrane was entirely stripped of villi, *excepting a few fragments which* still remained, and the enlarged and promi-

nent opening of the follicles of Lieberkühn gave its surface the appearance of a sieve. In some instances in which the pancreas has been examined, evidences of disease presented themselves.

The second proposition was stated to be more difficult of proof, inasmuch as vomiting usually occurs only in the first stage, and the author had no opportunity of examining the vomited matters at this period of the disease. In one case, in which vomiting took place in the third week, fibrinous casts of the stomach tubes were discovered, and inflammation of the mucous membrane was proved to have existed by post mortem examination. The chief reason upon which the opinion that desquamation of the epithelium occurs was founded, was from the microscopic examination of the contents of the stomachs of those who had died of this disease. The contents in recent cases consisted of pieces of fine membrane, of cells, and of granules and shreds of membrane. The membranes were of the shape and size of the tubes of the stomach, and were covered with granules and fat. The cells varied from 1-1200th to 1-2200th of an inch, and were usually fringed with fine pieces of membrane. In cases of longer duration the membranes were covered with cells, and were also of the size and shape of the stomach tubes. In order to ascertain if these appearances were trustworthy as evidences of inflammation, the contents of the stomachs of forty-five subjects were examined at the Middlesex Hospital, the condition of the mucous membrane being at the same time noted. In only one were there any fibrinous casts, and it was in a case of acute gastritis. In eighteen there were only separate cells, chiefly of the columnar form, and in none of these was there any inflammatory action. In eight cases casts of the upper parts of the tubes were plentiful, composed only of healthy conical cells, and in all the mucous membrane was in a natural condition. In eighteen there were either plugs formed of cells and granules from the secreting parts of the tubes, or the casts of conical cells were overlaid with granular matters, and in all of these the stomach was more or less inflamed. Two cases of gastritis, unconnected with scarlatina, were also quoted as examples of the forms in which casts of the stomach tubes appeared in vomited matters during life, and the author stated he had detected casts of the stomach tubes in matters vomited by persons affected with gastritis connected with diseased kidneys, with inflammatory dyspepsia, and other forms of inflammation of the gastric mucous membrane. It was urged that if casts of the gastric tubes can be discovered during life in cases of gastritis, and if in scarlatina this condition exists, and casts have been found in the stomach after death, there is every probability that desquamation of the epithelium takes place in this organ, as it does in the skin and the kidneys.

In support of the third proposition, the results of the following experiments were given in the three cases of scarlatina:—Ten grains of hard boiled white of egg were digested at a temperature of 90° for twelve hours in an infusion of the mucous membrane, to which three per cent. of hydrochloric acid had been previously added. The average loss of albumen was three grains and two-thirds. Similar experiments performed with the stomachs of eleven males who died of various diseases at the same hospital gave an average loss of four grains; so that there had been scarcely any diminution of pepsine produced by the fever. As a contrast to this were the results of similar experiments upon four cases who died of typhus fever. In two of these the albumen had gained three grains of weight by imbibition, and was not at all softened; whilst in the other two it was softened, and one had lost only half a grain, the other one grain and a half in weight. But as the activity of the digestion must depend not only upon the relative amount of pepsine, but also upon the bulk of the mucous membrane, this was also attempted to be estimated. The average weight of the mucous membrane of the stomachs of ten males dying of various diseases at the Middlesex Hospital was eighteen drachms, the weight of two recent cases of scarlatina was eighteen and sixteen drachms (the latter being in a boy), whilst it only amounted to fifteen drachms in one who died in the third week of illness. In four cases of typhoid *fever the average weight of* the mucous membrane only reached eleven *drachms.*

Under the fourth proposition it was stated that the skin had only been examined microscopically in three cases. In the first, in which the patient died after a few days' illness, the only morbid appearance in the cutis was an occasional minute extravasation of blood in the neighbourhood of the sudoriferous ducts. The rete mucosum was greatly thickened, and numerous round cells with large nuclei were everywhere visible, intermixed with the natural cells. The basement membranes of the sweat-glands were thickened, and the epithelium lining them was so much increased that in most cases it obstructed their channels. In some of the sweat-glands the coils of which they were composed were loaded with coagulated blood, and were greatly and irregularly distended. In the other recent case the appearances were similar, excepting that the external layers of the cuticle were stained with blood in minute patches, and the sweat-ducts were also reddened; but there were no extravasations of blood either in the glands or cutis. In some of the ducts the epithelium was detached from the basement membranes. In the case of a man who died during the third week, the sudoriferous tubes were still choked up, but in the glands the epithelium seemed in many places to be torn away, leaving the basement membranes bare, or only covered by ragged particles. The cutis was in a natural condition.

The author stated that although he had, in accordance with the usual custom, described the appearances of the skin and mucous membranes as the results of inflammation, yet that certain considerations suggested the idea that the term when so used was perhaps misapplied. In scarlatina, we find that in each part the morbid condition is mostly confined, in the first instance, to the basement membranes, and consists in the formation of layers of new cells, which, in the skin, are transformed into cuticle of natural appearance, and in the stomach contain pepsine. If future researches should prove that a similar condition occurs in the kidneys and other parts, it will be necessary to look upon the structural changes produced as resulting from increased physiological, rather than from pathological action; and that the primary effect of the scarlatina poison is suddenly and violently to stimulate the natural cell-growth of the various secreting organs.—*Lancet, July* 23, 1864, *p.* 93.

4.—ON THE TREATMENT OF FEVER.

By Dr. Gull, Physician to Guy's Hospital.

[The following short extract from some clinical remarks made by Dr. Gull, is contained in the "Reports of Hospital Practice" in the Medical Times and Gazette. When the pulse in the early stage of typhus or typhoid is very frequent, the patient requires to be well supported, in order to carry him through the later stages of the fever.]

Dr. Gull proceeded to remark: I do not believe in giving enormous doses of brandy in fever. My experience is against that system. I am sure of this, that if you have a severe case of typhus, and the patient is likely to die, no amount of brandy will save him.

Another part of the treatment that requires your attention is this: when I am called in to see a case of fever, I am not unfrequently told that the patient has been given something every half hour. In one case they said every five minutes. The plan of continually feeding Dr. Gull very strongly condemned; further saying, it is a very doubtful question in my mind whether much, or if any, of the nourishment given in severe cases is assimilated. But there is another way to support the patient; keep him cool, sponge his skin over three or four times a-day, give him plenty of liquids to support the renal respiration, and plenty of fresh air to support the pulmonary respiration.—*Med. Times and Gazette, Aug.* 20, 1864, *p.* 196.

5.—ON THE PREVENTION OF ZYMOTIC AND CONSTITUTIONAL DISEASES: FEVERS; CHOLERA; CONSUMPTION, &c.

By Dr. E. D. Mapother, Professor of Hygiene, and Medical Officer of Health for the City of Dublin.

The poison of scarlatina seems the most contagious of all its class, having the greatest infecting distance, and fixing with the greatest tenacity in dwellings, furniture, or clothes—fomites they are technically called. For this reason, when the disease breaks out in schools, it is generally politic to close them up entirely for some time. The heat of boiling water destroys the poison, and rooms or clothes in which the poison may be lurking should be thoroughly steamed or fumigated with sulphurous acid emitted from burning sulphur. The disease is inoculable, but the malady so produced being as severe as the ordinary kind, such an operation is not advisable. The most malignant epidemics of this disease from which Ireland has suffered were in 1801–2–3, and 1834.

Measles poison would seem to be nearly as contagious, for instances have occurred of the disease being carried by children's clothes packed in a box and sent from schools where the epidemic had broken out. In America this disease has assumed a serious aspect, for 22,000 soldiers were prostrated by it during the first year's campaign. I may mention that Dr. Salisbury, of Ohio, has endeavoured to prove that the measles poison owes its origin to a fungus which grows upon rotten straw. He inoculated himself, his wife, and twenty-seven other persons with this fungus, and in all an eruption similar to measles followed, and none of these individuals so protected caught the natural measles, which just at the time was strongly epidemic.

In connexion with this subject, I may remark that Dr. H. Kennedy has published an interesting case where a measles-like eruption followed the application of some mouldy flaxseed meal. Several hundred children in France are said to have been inoculated with the disease by inserting a tear from a patient into a puncture.

In my Introductory Lecture, I endeavoured to impress on you that typhoid fever was about the most preventible of diseases, yet 140.000 cases occur, and 20,000 at least die of it every year in England, and as the average age of its victims is 21, and all ranks are obnoxious to it, the very flower of the people is included. I will not dwell on the subject further here, save to remark, that in having pustules on the intestinal mucous surface, it resembles the diseases —small-pox, for example—which are characterized by peculiar eruptions on the skin. It is supposed that the seeds of this disease may dry up, and yet, like those of many plants, may germinate when submitted to favourable conditions. There is much greater risk of contagion from the decomposition of the poison in faulty sewers than from the atmosphere about the patients, and its progress is much more virulent when introduced by water drank than by air inspired. I enumerated many chemical destroyers or disinfectants of acknowledged efficacy, but I wish now to add to the list McDougal's fluid (crude carbolic acid), and McDougal's powder (carbolate of lime), which are cheap, convenient, and thoroughly reliable in eradicating this pest "born of putrescence."

Typhus has always been Ireland's epidemic enemy, and is still five times as frequent in Dublin as London in proportion to the population. You all must have read of the lamentable way in which this fever decimated the army in past centuries, and the contagion was usually introduced in the following way: Commissions were formerly given to those who collected a certain number of recruits, and a promiscuous rabble, reeking with the seeds of typhus, was often obtained by ransacking the lowest haunts, and even the jails. In these latter places so virulent was the poison, that the judges, witnesses, and others who were engaged in court rarely escaped, as I could relate to you if time permitted, from the writings of Lord Bacon. The intensity of the disease would seem now to be much less, for persons in a house with a typhus patient suffer little risk, and those in neighbouring houses none. I have *investigated* some cases in which the inhabitants of a *house were reputed to have caught* typhus from patients in a neighbouring

one; but I have always found that the attack was due to an original focus usually promoted by similar unsanitary conditions. If there be suitable arrangements for the exit of the infected air and the entrance of the fresh, I have never thought it necessary to banish relatives from the patient's house, for their affectionate solicitude is often more valuable than any hired services. I will now recapitulate the circumstances which recent pathologists assert to be promotive, if not productive, of typhus, and you will acknowledge they are all preventible. 1, over-crowding and defective ventilation; 2, personal squalor, especially the wearing of clothes soaked in cutaneous exhalations, and dark coloured woollen stuffs have by far the greatest power of absorbing the poison; 3, a low state of system due to scanty or bad food; 4, a medium temperature.

As regards the most plainly malarious of diseases—ague—it used to prevail in Dublin and its vicinity; but owing to improved surface-drainage it has almost wholly disappeared.

The next group of the zymotic diseases is that of which the breathing organs are the seats, and among them influenza is the most clearly epidemic, the most rapidly diffusible, and the most largely fatal, at least 4000 persons in Dublin having succumbed to it in four months of the year 1837. Few old people escape if they are attacked; but Dr. Graves mentions a notable exception. He attended Judge Day, the contemporary of Goldsmith, at the age of 93, and he recovered perfectly. There are reasons for supposing that influenza is due to some specific poison floating in the air, and acting like the emanations from the sweet-smelling vernal grass which gives rise to that remarkable disease, "hay-fever."

Whooping-cough, another zymotic, seems to be excited by some poison which acts exclusively on the branches of that widely-distributed nerve, the pneumogastric. That the poison is not solely aërial would appear from a child just born having had the disease, which must have been carried to it in the mother's womb. Quinsy, croup, and diphtheria, are other examples of this group of diseases, and in the prevention of all of them, ventilation, drainage of excreta, and removal of dampness, are hygienic measures of tried and positive efficacy.

Cholera, the most dreaded of the zymotic diseases, which choose as their seat the digestive canal, is, as I endeavoured to explain on two previous occasions, communicated by a specific poison emitted from a patient already attacked, and carried to others through the water or air. It, therefore, follows the lines of human intercourse for the most part, but outbreaks do undoubtedly occur which allow of no such explanation. It first appeared in Dublin on 22nd March, 1832, and arose in Cork on 12th April, probably from some new source of contagion (perhaps by the steamer which then plied between these cities), for Naas, which lies along the direct road, was not attacked till April 13th. That learned physician, Prof. Aitken, to whose comprehensive treatise I am indebted for many facts, relates the following exceptional case:—"In one of the Western Islands, the most remote from the mainland, the disease suddenly appeared, where so little intercourse existed with the place that the clergyman of the island continued regularly every Sunday for eighteen months to pray for King William the Fourth, as if he had been alive, after our gracious Queen Victoria had ascended the throne."

The cholera, which is endemic in these countries, prevails, for reasons I explained before, during hot dry weather, and is in London contemporaneous with the putrefaction and stench of the contents of the Thames, and a death in this city from the same disease has just been registered by one of our dispensary medical officers. When our water supply will be available and our sewerage efficient, we may set at defiance even its awfully aggravated and epidemic form.

I will conclude what I have to say upon those zymotic diseases depending upon the introduction of a specific poison into the blood by mentioning that Prof. Polli of Milan has energetically advocated the use of chemical antidotes to them—such as the sulphites—which are known to possess the power of *checking catalytic action. If* their curative efficacy be proved, they will

become still more reliable in preventing the specific zymotic process consequent on the introduction of the poison.

On another occasion I dwelt at perhaps sufficient length upon the influence of want of pure air in the production of consumption, which is so potent that the deaths among in-door artisans, such as tailors, shoemakers, weavers, and printers, from this disease are at least twice as frequent as among those who labor in the open air. Much may be done in the way of prevention, both by public and private hygienic measures. Under the former head I include the opening of public parks and grounds, a close supervision of the establishments where people work in numbers, and the application of the most scientific preventives to the special injuries which many noxious trades inflict, and in such arrangements our French neighbours far excel us. Among means which lie in each individual's power are the fit ventilation of bed-rooms, to which sunlight should have free access by a proper aspect being chosen, and free exercise in the open air with the habit of filling the lungs occasionally to the utmost, for by ordinary breathing their upper parts are not inflated, and remember a disused organ always suffers.

I must mention to you some remarkable facts with regard to the habits of those who have afterwards become consumptive—namely, that they have suffered from that kind of dyspepsia in which acid is too freely produced in the stomach, and that they have exhibited a constant dislike for fatty foods. The peculiarity of their digestion would then be that while albuminous food would be assimilated, the alkalinity of the saliva and pancreatic juice would be neutralized, and their respective functions—namely, the conversion of starch into sugar and the taking up of fat, interfered with. The remedial powers of cod-liver oil, which insinuates itself so readily into the absorbents, may depend upon their supplying this want. The preventives of consumption are urged so enthusiastically by Dr. McCormac that I shall sum up by giving you his list—the respiration of a pure untainted atmosphere by day and night—improvement of the artisan's *locale* and habits, including his bent and sedentary posture—the substitution of steam machinery for dry grinding and stone-chiselling—attention to the digestive and cutaneous functions—improvements in the aspects of houses and sleeping-rooms—increase of out-door pursuits—and lastly, full and free respirations in the open air. Dr. Aitken, Professor of Pathology in the Army Medical School, most justly remarks:—

"Experience has now adequately demonstrated, that the tuberculous cachexia springs from causes over which the public, rather than the medical profession, have control. The physician must be at once impressed with the belief, and encouraged with the hope, that when he acquires the confidence of the public in the practice of his profession, he may exercise a powerful influence for good in teaching how much they may themselves control the ravages of consumption by prudent marriages and sanitary attention to offspring. There are several circumstances which show the great influence of public sanitary measures in controlling the ravages of consumption when these measures are scientifically directed to the preservation of the general health, and especially when men are associated together in great communities—an influence much greater than the best directed efforts of the medical profession can establish through their *materia medica*. It is by the mode of life as citizens of the world, in the social relations of husbands and wives, parents and children, and in the public relation of masters and workmen, that the extent and ravages of consumption are to be controlled. It is by a strict attention to the rearing of offspring, and in the subsequent regulation of food, clothing, cleanliness, occupation, the choice of a profession, and by many other circumstances which have an obvious influence (perhaps at first sight inappreciable) on the maintenance of the general health, that our hopes of success as practitioners of medicine must rest in the prevention of that bad habit of body which developes and propagates the tubercular diseases in civilized society.

Upon dietetic diseases I made a few remarks on a former occasion, and now, concerning one of them—namely, gout—I can tell you in a very few words *all the measures which* are preventive by quoting Abernethy's reply

when asked, What was a cure for the gout? "Live on sixpence a day and earn it," for temperance, exercise, and a freely acting skin, are among the hygienic requisites comprised in the aphorism.

As I have endeavoured to elucidate, our tissues do not remain permanently ours, but are in health being constantly removed and renewed. Now, when renewal fails and substances lower in the scale of human chemistry become deposited instead, we give the name of "degeneration" to the condition. The fatty form in which a tissue—the muscular, for example—is either chemically converted into fat, or is removed to give place to that substance, is the most frequent, and chooses such vital organs as the heart, brain (yellow softening), and the kidney (chronic Bright's disease). We are warned that sedentary habits are strongly promotive of this affection, for it was fatal to such prolific writers as Abercrombie, Pereira, and Thackeray, and such industrious lawyers as Cresswell and Slade. The influence of the ingestion of too much heat-producing food to the exclusion of tissue-material we have already discussed; but it remains for me to explain the physiological effect of alcohol, which we shall find tends powerfully to the production of fatty degeneration. It checks the changes which our tissues are undergoing, interferes with the due oxidation of the blood, or the combustion of its waste material, and, according to many authorities, supplies material itself for the formation of fat, as more certainly does the sugar, which most of its preparations plentifully contain. Distilled spirits are for this last reason less promotive of fatty degeneration than other alcholic liquors. I shall not now dwell on other evils of intoxication, lamentable though they are, save to say that it is not on the individual alone they work destructive changes, for his offspring, in addition to moral injury, undergo most manifest physical deterioration. In this way alcohol is one of the most powerful causes of that remarkable character of disease in the nineteenth century—namely, that while the mean lifetime of adults is becoming prolonged, that of children is being most perceptibly shortened.—*Dublin Medical Press, Sept.* 14, 1864, *p.* 261.

6.—ON INTERMITTENT FEVER.

By Dr. S. H. Ward, Physician to the Seamen's Hospital, "Dreadnought," &c.

[Dr. Ward possesses tolerably ample notes of between two and three hundred cases of ague, which have been under his care during the last five or six years. He gives the places where the disease was contracted, and continues:]

An ague may appear to be cured, but it has a tendency to return; and when it does so after a short interval, we have what is called relapsing ague. Dr. Graves—to whose admirable clinical lectures I am always pleased to make reference—was the first, I believe, to establish, from a long series of observations, the law that the periodicity of agues applies, not only to the succession of paroxysms, but to the free intervals. My own experience leads me to confirm this view, the period of relapse, in the few cases in which I could determine it, having occurred on the day on which the fit would have taken place had the disease not been arrested. Thus I find, in one case of tertian, the fit occurring on the eighteenth day after the last, and, in other cases, on the twelfth day.

About a fifth of recent cases of ague were ushered in by rigors, pain in the head and back, and by more or less gastric derangement, indicated by furred tongue, pain in the abdomen, bilious vomiting, and diarrhœa. In the other cases they were merely the phenomena of the recurring paroxysms, upon which I need not dwell more than to recall the checked action of skin, the cold surface, the pinched, wrinkled aspect, and arrested secretions of the cold stage; the then powerful determination of blood to the internal organs; the reaction characterizing the second stage in the effort of the heart to throw off the blood from the centre to the circumference, the intense heat of *surface, and still congested state of* viscera; and the resolution, marked by

re-established secretions, especially by action of skin and the relief of internal organs, which is distinctive of the third stage. The paroxysm over, the patient generally remains well until the next accession; but not always so. In about a fifth of recent cases, as recorded here, there is more or less gastric irritation continuing in the intervals of the paroxysms. In a not very small proportion of cases, the liver and spleen, especially the latter, are enlarged during the paroxysms, as determined by percussion and palpation; and sometimes they remain so during the intervals. There is also more or less tenderness over these viscera, and indications of congestion and functional disorder of liver in the shape of bilious vomiting and diarrhœa. But during the cold and hot stages, congestion is not limited to the abdominal viscera. Delirium not unfrequently shows that the brain is temporarily implicated. Cough, hurried breathing, and a sense of tightness over the chest, tell of a congested state of lungs. In one case of quotidian that was under my care, the patient had hæmoptysis every morning during the cold stage. In two or three cases, pain over the loins and albuminous urine showed that the kidneys were congested.

It has been our custom here, when a patient has been admitted suffering from intermittent fever, to defer treatment until after the occurrence of a paroxysm, in order to satisfy ourselves—first as to the existence of the malady; secondly, as to its type. In cases where there is much gastric derangement, one or two doses of calomel, followed by a purgative, have preceded the special treatment, and the patients have been put upon slop diet. Generally the liver and stomach have readily responded to this treatment; the tongue has become clean, and vomiting arrested. But this is not always so, and there has been, especially in recent cases occurring along shore, some obstinacy in the gastric derangement. I have not cared usually to give quinine until the tongue is tolerably clean and the stomach free from irritability. When the gastric irritation is relieved, or where it does not exist, on having ascertained the time of access of paroxysm, we have given a full dose of quinine about three hours before its expected occurrence. I have, after some pains to ascertain it, come to the conclusion that the quinine acts better when given about three hours than at a longer or shorter period before the expected attack. I have also endeavoured to determine the minimum dose of quinine that is required, and this I have fixed at ten grains. Where the paroxysms are irregular in their character, I have found five grains of quinine, given every four hours, answer thoroughly. Out of 109 recent cases, 102 yielded at once either to ten grains or five grains every four hours, and in but 7 was a repetition or continuance of large doses required. Two or three cases complicated with severe gastric derangement proved more obstinate. For the removal of cachexia, and to guard against a relapse, quinine was generally given in two grains three times a day for a week or two, and the patients usually had a bottle of quinine mixture to take out with them. The quinine occasionally produced ringing in the ears and deafness, but only in one case did I observe any more serious effects. It was in a man who was admitted with tertian on Oct. 29th, who had had ague for a month, but had not any indications of head affection during the attacks. He was ordered ten grains of quinine before the expected paroxysm, and three grains three times a day. When seen on Nov. 1st, he had impaired motion of the right side of the face and right arm, with mouth somewhat drawn to left side, delirium, pupils contracted, pulse weak, surface moist; had passed his urine unconsciously, and stools twice in bed. Ordered a blister to nape of neck, and the omission of the quinine. He was much relieved on the following day, and by the end of a week all the cerebral symptoms had disappeared. Although these symptoms appeared during the administration of quinine, and disappeared on its discontinuance, yet it would not be quite logical to regard the quinine and the attack as cause and effect; for the cerebral affection occurred on the day on which the ague fit was due, and may have been a masked form of it.

I have fairly tried the liquor potassæ arsenitis against the quinine, in doses as full and frequently repeated as I dared to venture upon, and have

continued it until irritation of the intestinal mucous coat rendered desistence imperative; but have not succeeded in checking the paroxysms as with quinine. Some physicians, admitting the greater efficacy of quinine in arresting the paroxysms, have claimed for arsenic, and also for strychnia, greater power in preventing a relapse. I believe, however, that quinine is quite effective for this purpose if continued in small doses for a considerable time, and also that it is the best prophylactic against the disease. A captain who consulted me a year or two back said he was on the point of sailing for Jamaica, and that he had never been there without contracting severe ague. I recommended him to take quinine, in small doses, two or three times a day, all the way out. He did so; and on his return to England reported that for the first time he had enjoyed immunity from the disease.—*Lancet, Sept.* 24, 1864, *p.* 347.

7.—ON THE TREATMENT OF RHEUMATIC FEVER.

By John Roberton, Esq., Manchester.

[After some remarks upon heart affection in rheumatic fever, the author passes on to the treatment which he considers the best fitted to prevent this, and gives the following cases.]

The first is that of a vigorous adult male. After fatigue and unusual exposure to the weather, my patient became feverish, and complained of headache, with acute pain in the right knee. By the evening of the same day, the pain had extended to the ankle, as well as to the other knee; and the following morning the joints of the lower extremities were generally affected, with the addition of pain in the loins and one shoulder. In a few hours, nearly all the joints were the seat of acute inflammation. The nature of the case being obvious, a pill, consisting of a grain of opium and two and a half grains of calomel, was administered every three hours till six grains of opium and fifteen of calomel had been taken. The effects were some relief of the pains and a moist skin. I then ordered a purgative mixture, composed of an ounce and a half of Epsom salts and two drachms of the wine of colchicum, in eight ounces of cinnamon water; the dose, a wine-glassful, was repeated every four hours until there was free, not profuse purging. This being accomplished, I prescribed Peruvian bark with quinine, compounded thus: one ounce of powdered cinchona bark, with twenty-four grains of the sulphate of quinine, were shaken up in eight ounces of water, with the addition of a little dilute sulphuric acid to secure the solution of the quinine. Of this a tablespoonful was given every third hour, and also during the night, when the patient was awake. Lemon-juice in water was taken freely as a beverage; and dry cotton-wool, made warm, was wrapped round the joints that were most painful. This course, being steadily maintained—I refer especially to the use of cinchona and quinine—allayed in a few days the articular inflammation, and speedily effected a cure. Indeed, where this treatment is pursued, I rarely have a patient who is obliged to remain within doors longer than three weeks. Of course, the remedies mentioned have to be varied as to doses, &c., according to the age of the patient and other circumstances. For instance, when the muscles are the chief seat of the disease—a most painful variety of rheumatism—opium may have to be given nightly throughout the illness.

The other case was that of a tall vigorous girl, twelve years old. When I first saw her she was in bed, suffering from what appeared to be a sharp feverish cold. The following morning, there was pain in the joints of one limb; and by the evening, painful swellings in the joints of both the upper and lower extremities, such as left no doubt as to the nature of the complaint. Indeed, late in the evening of this second day, the joints of the fingers and toes were all affected. I immediately ordered for her eight pills—one to be taken every three hours—each containing a grain and a half of calomel, with a quarter of a grain of opium. In about twenty-four hours from this, an aperient mixture was prescribed, consisting of Epsom salts half an ounce, wine of

colchicum a drachm, an ounce and a half of compound tincture of senna, with cinnamon water to six ounces; of which an ounce was given every fourth hour to cause free purging. When this had been accomplished, and the fever and pain much reduced, the following was ordered.

Quinine, xxiv. grains; dilute sulphuric acid, xx. minims;
red cinchona bark (in powder) ʒvj.; and water to ℥vj.

Of this, well shaken up, was given, *perseveringly*, a teaspoonful every three hours by day, and also in the night, when she was awake. Before the end of the eighth day, the pain had left; and in a fortnight she might be said to be quite well. This case occurred about two years ago, and the health has ever since been sound and vigorous. Last autumn, I treated a strong girl of eleven, living in the country, in the same manner as the above; and she had to keep her bed only for nine or ten days, and has since been in excellent health.

It is assumed, when I am speaking of the treatment of acute rheumatism, that the patient is seen by me *at the commencement*. When a case has been going on without treatment, it may be, for two or three weeks, the remedies I have mentioned might not be altogether applicable.

Certain important particulars, with reference to the general management of this disease, I give in the following order.

1. Be the patient ever so robust, and the fever and pain severe, I never bleed, either by the lancet or with leeches.

2. I avoid diaphoretics; that is, doses of the Dover's powder to cause and keep up profuse sweating; which was, I may say, the universal practice of my early days.

3. I forbid the use of fomentations to the inflamed joints; indeed, I forbid every kind of application to the joints, excepting *dry warm cotton*, or a dry warm silk handkerchief. The use of friction with some anodyne liniment (apt to be had recourse to by the friends of the patient, unless guarded against) I also prohibit.

4. In patients under the age of twenty, I do not allow even the hands or face to be sponged or washed during the fever; indeed, I dread every kind of liquid application. I well remember, early in my residence here, a fine young man, about the age of sixteen, brought home from a boarding-school with acute rheumatism, his knees enveloped in wetted bandages. The consequence was death in three days, from cardiac inflammation.

5. The quinine and bark should not be given until the feverish symptoms have been allayed by the use of calomel and opium, followed by a purgative.

I will just add that, when the patient has been seen early, and treated as here recommended, I have never, during the last thirty years, known heart affection to follow in a single instance.

I may state, in conclusion, that I am quite aware there are several eminent authors who recommend, in rheumatic fever, other remedies and modes of treatment than what I have mentioned; but I have thought it best, for the sake of brevity, to give an outline merely of my views and experience.—*British Medical Journal, June* 18, 1864, *p.* 659.

8.—ON THE THERAPEUTIC VALUE OF ALKALINE AND EARTHY SULPHITES IN THE TREATMENT OF CATALYTIC DISEASES.

By Dr. H. R. De Ricci.

[The discovery, if indeed it may be so called, of the action of sulphites in certain forms of catalytic (zymotic) diseases, was made by Dr. Polli of Milan. Since the publication of Dr. Polli's papers on the subject, Dr. De Ricci has devoted much time and attention to its investigation.]

For the benefit of those who may not have read my former publications on the action of the alkaline and earthy sulphites, nor the original writings of Professor Polli, I shall now, as briefly as possible, recount the origin and result of his discoveries.

For several years past a theory has been almost universally gaining ground that a great number of diseases depend essentially on the presence of an organic poison circulating in the system, where, acting as a ferment in the blood, it multiplies itself, vitiating the animal fluids and giving rise to divers diseases, according to the special poison in circulation; thus in one case producing small-pox, in another scarlatina, in another puerperal fever, &c. These diseases have been termed *zymotic*, on the supposition that the circulating poison acted as a *ferment* in the blood; but, as the precise mode of action of these animal poisons is not as yet well understood (though it is no doubt a chemico-vital action), I shall throughout this paper employ the term *catalytic* instead of *zymotic*, as perhaps better calculated to convey the idea of the function of these poisons, without absolutely involving a principle, which the term zymotic might do.

Several years ago Professor Polli, being fully convinced of the truth of the catalytic theory of many diseases, devoted his time and talents to search out some means by which these poisons, even after they were absorbed and developed in the animal economy, might be neutralized, and eventually eliminated, without, at the same time, imperilling the integrity of the blood. The task was an arduous one, for the great physiologist, C. Bernard, had dogmatically asserted that any substance capable of destroying a catalytic poison in the blood would, at the same time, so alter that fluid itself that it would be rendered incapable of performing its vital functions. Professor Polli, however, had long observed the action of sulphurous acid in arresting the process of fermentation. He was convinced in his own mind that in it would be found the anti-catalytic substance he was seeking for. He persisted in his researches, and eventually succeeded in establishing that not only sulphurous acid possesses the property of arresting fermentation and neutralizing catalytic action, but that also its alkaline and earthy compounds have the same power, with this difference, that while sulphurous acid cannot in any way be introduced into the circulation with safety, the alkaline and earthy sulphites, hyposulphites and bisulphites can be administered with the greatest impunity, even in large doses; and their presence can always be detected when administered, not only in the blood but in the tissues and evacuations. Having satisfactorily established this important fact, he began a series of experiments on animals, which proved, most successfully, the correctness of his theory and the accuracy of his deductions. Nothing could give me greater pleasure than to recount the several interesting experiments which he performed, but it would lengthen this paper too much, and otherwise occupy too much of the space of the Journal; and, for those who are interested in this matter, I beg to refer them to a communication read by me, about a year ago, at the Obstetrical Society in this city, in which will be found a short epitome of Professor Polli's experiments.

From the moment I first read the account of Professor Polli's researches I came to the conclusion that his discovery was either valueless or of immense value. In such a case there could be no medium; it was either utterly worthless, or it was a discovery second to none—a discovery even greater than that of vaccination, and one which would constitute one of the greatest epochs in the annals of medicine; for vaccine could, at most, only ensure the vaccinated against one single disease, while the sulphites—if they truly could neutralize the action of catalytic poisons—of such poisons as *glanders* (see Polli's experiments)—would be able to arrest the course of small-pox, hydrophobia, syphilis, infection from dissecting wounds, erysipelas, puerperal fever, measles, scarlatina, whooping-cough, dysentery, diarrhœa, cholera, influenza, typhus, dothinenteria, plague, diphtheria, &c., &c., all of which are classed by Dr. Farre as zymotic diseases. The question seemed to me of such vast magnitude and importance that I solicited in every direction for co-operation in testing the real value of these sulphites. I first tried them on myself, and finding them decidedly harmless, even in doses of one scruple, five or six times a day, I began to administer them in every case where I thought that a catalytic poison might be the cause of disease. In no case did I meet with *any mishap, and in most cases* I think I can fairly say I was successful. In no

case did the exhibition of the sulphites produce sickness of stomach, diarrhœa, or any other inconvenience; and in every case their administration seemed to be of decided advantage.

The first case I shall describe is one of great interest and value in a clinical point of view; it was undoubtedly a case of infection from an animal poison. The patient was almost given over as lost, so severe were the symptoms of the disorder; yet the patient recovered, and the treatment consisted solely in the administration of the bisulphite of soda, in full, repeated, and continued doses.

A lady of about forty-five years of age, of sound constitution, and in the enjoyment of excellent health, was suddenly called, about a year ago, to the death-bed of one who was very dear to her. That death-bed was fearfully sudden and unexpected, and that poor lady could not be persuaded, long after death had indubitably taken place, that the spirit of the beloved one had really fled. She would not leave the corpse; she threw herself on it, and kissed it over and over again, and could not be induced to leave it, even when the discolouration of the skin and the offensive smell of rapidly-advancing decomposition gave ample testimony of the reality of death. The burial was performed two days after death, owing to the rapid decomposition of the body; and, soon after the funeral, I was hastily summoned to the bedside of this lady, whom I found in the following condition. It was about five in the morning when I entered the bedroom. She had gone to bed the night before quite calm and resigned, and on the previous day she had partaken fairly of food, but had not eaten any thing which could in any way account for the state in which I found her, of which the following is a fair sketch:—The windows of the bedroom being open, the morning light streamed freely into the room, and as I approached the foot of the bed I had a full view of the patient's face. To those of my readers who, like myself, have been familiar with the victims of cholera I shall simply say that the patient looked like one in cholera, in the stage of collapse. To those who have not yet had the melancholy opportunity of witnessing a case of that terrible disease, I shall say that I hardly recognized my friend, so altered, and pinched, and ghastly were her features. Her eyes were sunk, and surrounded by a lead-coloured zone; her cheeks, which, but a day before, were plump and ruddy, were now hollow and sunk; her eyes were glassy; her pulse scarcely to be felt; the surface of the body cold; her breath cold; her tongue cold; her voice low and husky; her faculties perfectly clear. She now lay quite prostrate on her back; but for some hours previously had suffered greatly from vomiting, cramps, and diarrhœa, which was of the characteristic rice-water appearance always observed in cholera. I at once administered a full supply of hot brandy and water, ordered turpentine stupes to the abdomen and limbs, treating the case exactly as one of Asiatic cholera; and, being fully alive to the grave nature of the attack, requested for further advice, and at once sent off for my valued friend, the late Dr. Mayne. At first I felt singularly at a loss how to account for so sudden and severe an attack of what seemed to be a genuine case of Asiatic cholera. So true was it to symptoms that, when Dr. Mayne looked at the patient, he whispered to me: "If cholera were in the country one would not hesitate to give this case a name." The clue to the disease was, however, given to me, in a few words, by the patient herself. As soon as she had taken the brandy she said to me:—"The smell of the body was dreadful; I cannot get rid of it in any way;" and immediately she began to retch. She had given me the key, and the mystery of her case was solved. It was no doubt a case of putrid infection—of septicemia; and if there was any truth in the "sulphite" theory it should prove of value in such a case as this. I at once ordered a strong solution of bisulphite of soda in infusion of quassia, with tincture of bitter orange peel and Battley's sedative—two drachms of the bisulphite to the ounce—and gave it in large teaspoonful doses, every half hour at first, and then every hour, each dose containing nearly twenty grains of the bisulphite. I watched this case incessantly myself, day and night, and the result was most satisfactory. All the symptoms by degrees abated, and in a *very few days* the patient was fully convalescent. Dr.

Mayne, who had anxiously watched this case with myself, was so impressed with the results obtained that he told me he would give the remedy a full trial in every case of scarlatina that should come under his care; and, in addition, promised (at my suggestion) that he would prescribe a dose of the sulphites daily to every healthy member of the family in which there was a case of scarlatina, to test its value as a prophylactic; for if a sulphite could destroy a catalytic principle, even when developed in the system, it should also have the power of preventing the development of that principle from the commencement.

As I stated above, the lady recovered, and, apparently, was restored to absolute health; she, however, complained to me occasionally of wandering pain, and general *mal-aise* at times, which she could not account for. About five months after her recovery she hurt her leg; it was a very trifling abrasion, but it assumed an angry look, and seemed determined not to heal, when, at the end of two months, she broke out all over with an extraordinary eruption, more like erythema-nodosum than anything else, when I at once placed her again on bisulphite of soda, in the supposition that some of the poison was still lurking in her blood, when the sore in her leg rapidly healed and the eruption disappeared, leaving the patient perfectly well.

The next two cases in my list were well-marked types of measles. I shall not take up the time of my readers by giving the details of them. I shall merely state that they were severe cases; that they were treated solely with bisulphite of soda, in scruple doses, every second or third hour, and that both cases grew rapidly well.

The fourth case is one of poisoned wound. A. B., aged about thirty-five, a gardener, was grafting a cactus, and in carelessly handling such a thorny plant, got the back of his left hand severely stung in different places. He plucked out as many of the spines as he could discover, and thought no more about the matter; but in the course of twelve hours or so the hand began to swell and be painful. He at once wrapped it up in a poultice; but the pain and swelling not abating, his employer sent him to me, three days after he had been stung. I found his hand enormously swelled, of a dusky purple colour, with large bullæ over the dorsum; the fore-arm also swelled, though not discoloured, but presenting several longitudinal red lines running up to the elbow. The man complained of great pain locally, of intense thirst, headache, loss of appetite, shivering, and general feeling of sickness. His tongue was furred and brown. He had, of his own accord, taken a dose of senna and salts that morning. I at once made an incision in the dorsum of the hand, and some pus came out. It was not, however, like cutting into an abscess—no gushing of matter took place, only an oozing, like as if I had cut into a sponge saturated with pus. I therefore made a second incision, parallel to the first, from which some more pus came out; ordered him to wrap up the hand in a large linseed-meal poultice, and prescribed the bisulphite of soda, in scruple doses, every second hour. I desired the man to keep quiet at home—without, however, ordering him to keep his bed; directed him to take light nutritious food, and to drink two pints of XX porter in the day. The following day the hand looked better; it was less swollen and purple, and the pus was in larger quantity and better looking. The man felt also better; he had less thirst, and had only had two slight shiverings. The bowels not having been freed, I ordered him some sulphur, with magnesia and scammony, as an aperient, and to persist with the bisulphite, taking it now every third hour. The following day the man was so much improved that he called on me. The hand looked still better; the incisions which I had made had ulcerated somewhat round their edges, but the suppuration was free, and the pus seemed quite healthy; there was no pain in the arm, and the red lines were very much paler. Matters looking so very much better, I desired the man to take the sulphite only three times in the twenty-four hours; to take a good allowance of food; to stay much in the open air; but to still keep the arm in a sling, and the hand wrapped in a poultice. Two days later he again came; when, finding that all swelling had subsided, all pain gone, all red lines *disappeared, the sore* inclined to granulate, I stopped the poultice, and desired

him to dress the hand with some warm dressing, containing a little balsam of Peru, and to take one scruple of the bisulphite twice a day. Two days after the man returned to me, not looking as well as at the previous visit. He had a yellowish tinge in his skin; his tongue was foul; he complained of chilliness, almost amounting to shivering, and the sore at the back of the hand looked unhealthy and angry. On stripping the arm, the red lines were again visible, but of a very pale red; while above the elbow, from its bend to the axilla, a hard, knotty, and extremely painful cord could be felt and seen running up parallel to the brachial artery. I at once returned to the primary doses of bisulphite, wrapped up the hand in a poultice, and watched to see what the result would be. The man immediately began to mend, and in four days not a trace of hardness was to be felt. Fearing, however, a relapse, I continued administering four scruples of bisulphite daily for ten days longer, by which time the hand was perfectly healed, and the man returned to his work.

Two better cases for testing the effects of sulphites could not possibly have been selected; in both cases the disease clearly resulted from the working of a poisonous element in the blood, evidently introduced from without in the first case, whilst it may in the second case have originated within the system subsequent to the stinging with the thorns, if one does not feel justified in considering cactus thorns as poisonous of themselves; be this, however, as it may, the second case was as clearly one of purulent absorption as the first was of putrid infection—the red lines up the arm marking the course of the inflamed lymphatics as the hard knotty condition of the veins denoting the phlebitic inflammation. Both were treated solely with sulphites, and both completely recovered. It would be great presumption, no doubt, to say that in both cases the patients would have lost their lives except for the saving properties of the bisulphite of soda administered; but I still cannot avoid believing that the sulphurous acid did prevent the spreading of the catalytic principle by rendering it incapable of reproducing itself; and, while keeping it in abeyance, allowed time for its elimination by the ordinary powers of nature; and I think we have a confirmation of this view in the occurrence of a relapse in both cases, where it would appear as if, when the bisulphite was stopped, all the poison had not yet been eliminated, the blood disease breaking out again the moment that the poison was freed of its antagonist; and a cure being effected by administering more of the anti-catalytic remedy until every trace of the animal poison was eliminated.—*Dublin Quarterly Journal, August*, 1864, *p.* 28.

9.—ON THE COMPARATIVE VALUE OF SULPHURIC ETHER AND AMMONIA, AND CHLORIC ETHER AND AMMONIA AS ANTI-SPASMODICS IN TYPHOID FEVER.

By Dr. Henry Osborn, Consulting Physician to the Southampton Dispensary.

[The case was one of typhoid fever in a gentleman twenty-two years of age. The lips, teeth, and tongue were thickly incrusted with black sordes; pulse 120; skin hot and dry.]

Being informed that wine had caused very great excitement, and observing a tendency to subsultus tendinum, we agreed to give chloric ether in combination with carbonate of ammonia.

Towards evening the unfavourable symptoms were more pronounced, and I was again requested to see the patient; but my colleague in the case could not make it convenient to attend. Wishing to know the effect produced by the mixture ordered in the morning, I administered a dose of it myself; and, finding no mitigation of the symptoms, resolved to try the effect of wine, which, though cautiously administered, produced so much excitement that it could not be continued with safety. I then gave ammonia and chloroform, omitting the spirit contained in the so-called chloric ether, but without effect; and the tetanic spasms had reached their acme. Involuntary evacuations took place, and *the pulse was too* rapid to be counted; there was total in-

sensibility to those around him; violent convulsive movements, especially of the right arm, which was contracted upon the chest, and one leg was similarly affected; while the jaws were spasmodically closed. I then ordered a mixture of sulphuric ether and carbonate of ammonia, in the same proportion as the chloric ether and ammonia; and the moment the muscles of the jaw had relaxed sufficiently to admit a teaspoon, the mixture was administered in divided doses, and immediate effect was produced, as shown by the general tranquillity throughout the system. The convulsive movements returned, however, at short intervals; and some time was lost by the locking of the teeth, but this was ultimately overcome by passing a bottle containing chloroform under the nose of the patient, and when the spasm relaxed the mixture was introduced, care being taken not to overstimulate. Cold applications to the shaved head served to regulate the stimulating effects of the medicine; while ice, beef-tea, and beef-essence, were given between each dose of the medicine.

In such a case it was necessary to stand by the bedside during the night, and to administer the remedies chiefly with my own hands; but early in the morning I had the satisfaction of seeing the patient tranquil.

Not being convinced that so great a difference could exist between sulphuric ether and chloric ether or chloroform in arresting tetanic convulsions, I made one or two more trials of the chloric ether and ammonia, but no effect was produced by it.

It may be requisite to state that I found the addition of ipecacuanha wine, in the dose of three to five drops, materially assist in removing the sordes and promoting perspiration. We cannot, I believe, assign any chemical action to ammonia, sulphuric ether, and ipecacuanha, in removing the morbid matter of typhoid from the blood; the action must therefore be physiological.—*Lancet, July* 2, 1864, *p.* 7.

10.—ON THE FEBRIFUGE PROPERTIES OF THE COMMON WHITE WILLOW.

By Dr. Cazin.

It has long been known that the white willow and its alkoloïd, salicine, possess febrifuge properties, and the fortunate coincidence has often been remarked of the growth of the willow in those very localities where intermittent fevers prevail—namely, in low marshy situations. Dr. Cazin remarks, that if the willow has failed to exert a febrifuge action in all cases, the circumstance is due to the smallness of the dose employed, and he suggests that the remedy should be associated with other bitter and aromatic vegetable extracts. M. Cazin has been in the habit of treating in this manner for the last twenty years the intermittent fever which prevails among the inhabitants of the marshy grounds about Calais; and he has found the willow an efficient substitute for cinchona and quinine. In the early stage he recommends an emetic or aperient when gastric disturbance is present, and then he proscribes large doses of the bark of the white willow, alone or in combination with camomile, wormwood, and some other indigenous tonics, alteratives, and aromatics. He considers that the combinations of vegetable bitters with astringent and aromatic substances are far more efficient than the indigenous febrifuges administered alone. He also thinks it desirable to persevere for a week or ten days in the treatment after the removal of the paroxysms, and to prescribe a large dose every week for a month or longer, if any symptoms indicate the danger of a relapse. M. Cazin recommends the willow bark to be employed in a decoction (℥ ss to ℥ j in a pint of water) or in powder in the dose of ʒ ij to ℥ j, in wine or beer, or in the form of a tincture or extract.—*British and Foreign Medico-Chirurgical Review, July* 1864, *p.* 248.

DISEASES OF THE NERVOUS SYSTEM.

11.—ON THE PATHOLOGY OF TETANUS.

By J. Lockhart Clarke, Esq., F.R.S., London.

[There is an able article by Dr. Lockhart Clarke in the Lancet for Sept. 3, on the microscopic appearances presented by the spinal cord in cases of death from tetanus. It seems that during the progress of the disease, there is a fluid exudation thrown out into the interior of the cord whereby the natural tissue of the part becomes broken down and ultimately dissolved. Every region—the cervical, dorsal, and lumbar, is more or less affected. In some sections the injury was limited to the grey substance; in others it involved the white columns, particularly the posterior and lateral. Hence the primary morbid action is more probably vascular, not nervous. Mr. Clarke at the conclusion of the paper briefly gives the *treatment* he would himself pursue in cases of tetanus.]

Division of the wounded nerve as early and as high up as possible; cupping along the course of the spine; frequent doses of calomel combined with opium; and potassio-tartrate of antimony, repeated, during the severer paroxysms, at short intervals and in sufficient quantity to produce nausea, or perhaps vomiting. The chief object of the tartrate of antimony is to subdue the spasms; but it might also assist in arresting the morbid action of the bloodvessels. Some years back, in the columns of this journal, I recommended the use of tartrate of antimony in those violent and prolonged paroxysms of hysteria which are so intractable and distressing, and I have never known it to fail. As soon as nausea supervenes, the spasms, however violent, begin to relax; and if the paroxysm be excited or prolonged by the presence of undigested food in the stomach, the vomiting will prevent its recurrence. In tetanus, any depression that might be caused by the antimony would be much less than the exhaustion of the nervous system resulting from the violence of the spasms.—*Lancet, Sept.* 3, 1864, *p.* 261.

12.—ON THE TREATMENT OF TETANUS BY THE LOCAL APPLICATION OF TOBACCO.

By Henry J. Tyrrell, Esq., one of the Surgeons to Jervis-street Hospital, Dublin.

On February 19 I laid before the Surgical Society of Dublin (see *Braithwaite's Retrospect*, Vol. 49, p. 54) the details of a case of traumatic tetanus treated by the application of tobacco stupes to the wounded part, and then I stated my reasons for preferring the topical use of tobacco to the internal administration of nicotine, as recommended by Professor Haughton, in 1862, in a paper which he read before the College of Physicians.

As another and better marked case of acute tetanus has lately been treated and cured by me by the use of tobacco locally applied, I think it advisable to let the Profession know, through the columns of the *Medical Times and Gazette*, the manner in which I have acted and my theory on the subject. It is that as tobacco or its active principle, nicotine, is of service when given internally, *à priori* it should be more so in peripheral or eccentric tetanus if applied locally by paralysing the nerves of the affected part from which the irritation proceeds, and thus removing the cause of tetanic spasms; and that when tetanus arises from a small wound, when a decided impression should be made quickly, a solution of nicotine of known strength or of tobacco should be applied to the wound and surrounding parts. And in the idiopathic form of the disease, if the cuticle along the back was raised by a blister, the endermic use of the remedy would more quickly bring the cord under its influence and with *less vital depression* than when given internally.

In the first case which I treated the wound was large, but in the subject of the present communication it was very small and insignificant, not calling for surgical assistance. The following is the history of the case:—

Michael K., aged thirty-five years, by trade a painter, of temperate habits, was admitted into Jervis-street Hospital on June 22, 1864, under my care. He was carried to the Hospital by four men in a state of great prostration, his teeth clenched, the muscles of expression and mastication rigidly contracted, the dorsal and cervical muscles also rigid. He was conscious and able to speak a little, but could not open his teeth to the slightest extent; he was bathed in a cold perspiration, his pulse 95, very weak and intermittent. His wife stated that on the 10th of June he cut his nose by a slight fall against a wall, but so trivial was the injury received that he thought nothing about it, went on with his work as if nothing had happened. However, on the 19th he felt some stiffness in the jaws, for which a medical man recommended him to apply a mustard poultice, and to take some medicine which he gave him. He rapidly got worse, and she had him taken to the Hospital. On examination I found a slight lacerated wound about half an inch in length on the right ala of the nose: it appeared indolent, without disposition to heal. There was no difficulty in the diagnosis; it was evidently one of acute traumatic tetanus with extreme depression.

If I gave nicotine I was afraid the depressing action of the drug would prove fatal, and recollecting the success which attended the local application of tobacco stupes in the case which I published, I determined to give another trial to the plan. As the wound was very small, I had the cuticle from the entire nose and for about twelve inches square of the back of the neck removed by boiling water and vesicating collodion, and covered the denuded surfaces with spongio piline saturated with a strong solution of Cavendish tobacco. The infusion was made by infusing one ounce of the unrolled leaves in twenty ounces of boiling water for an hour. The infusion was not strained, and both water and leaves were applied. I likewise determined to support his failing strength, and for this purpose directed an enema of sixteen ounces of strong beef-tea to be thrown up the rectum to unload the intestine, and after that was accomplished an injection to be administered every two hours, containing one ounce of brandy, ℥j. of sulphuric ether, four grains of sulphate of quinine, and six ounces of strong beef-tea. My residents, Mr. O'Grady and Mr. Fegan, remained up during the night, and attended most carefully to the patient.

June 23, 9 a. m.—He passed a very restless night; pulse 88, weak and intermittent; respirations 30; did not sleep; the muscles of the neck slightly relaxed; the abdominal and dorsal muscles more tense if possible; he cannot swallow his saliva, any attempt to do so gives exquisite pain in the fauces and brings on severe spasms, throwing the body into an arch with the heels and head resting on the bed. I ordered the treatment to be continued, but with an interval of four hours between each enema; a fresh infusion of tobacco to be made. I saw him again at 2 p. m.—Spasms somewhat relaxed, and he can separate his teeth so far as to allow a cedar pencil between them; pulse 86, stronger, not intermittent. 4 o'clock.—The rectum rejected the enema, and at 6 the pulse rose to 108, and became excessively weak. 9 p. m.—Ordered the enema to be repeated, having added to it twenty grains of tinct. opii.

24th, 1 a. m.—As he could swallow by sucking through a sponge, I ordered him a draught containing one ounce of brandy, 20 gt. tinct. opii in a glass of water; this he took through a sponge. However, it was with great difficulty he managed to swallow it, as now and again a drop would go wrong and cause the most distressing spasms. He took forty minutes to get it all down. After the draught the pulse fell to 92, and became stronger. 3 a. m. He swallowed through a No. 8 catheter ℥ss. of brandy in a glass of water with gr. x. tinct. opii. He is afraid to sleep, when he dozes he starts and moans and roars most fearfully. However, towards morning he fell into a refreshing slumber, and at 7 o'clock he drank with ease a pint of beef-tea *through the catheter.* 9 a. m.—The tension of the neck very much less than

it has been since his admission, but the abdomen if possible more tense. During the day he drank a quart of beef-tea, having in it twelve eggs and twelve ounces of whisky. The stupes were frequently renewed. Slept four hours during the night.

26th —Neck quite pliant; back and abdomen same as on yesterday; towards evening he became delirious, and his hearing was morbidly acute. Pulse at 10 p. m., 102.

27th, 9 a.m.—Pulse 100. Delirious. Suspects every one is attempting to poison him. During the day he took twelve eggs and one quart of beef-tea without the catheter. Slept at intervals.

28th.—Delirium much increased; he will not allow any one to attend him, not even his wife. Ordered the following mixture:—

℞. Quinæ sulphatis, gr. xxiv.; morphia sulphatis, gr. ij; aqua distil. ℥viij. M.; capiat ℥i. ter. horis, and the following draught to be given at 10 p. m.

℞. Chloroform, gt. xx.; liquor opii. sed. M. xxv.; syrupi croci, ℥j.; aq. distill., ℥vij.—M. ft. haustus.

During the night he got no sleep, but continued very restless.

29th, 9 a.m.—Pulse 108, weak; he fell asleep at 10 a.m., and slept sound until 5 p.m.; he got restless again during the night, and got at intervals ♏ 50 of tinct. opii., which calmed him towards morning, when he again slept.

30th.—He awoke at 8 a.m.; at 9 I saw him, delirium quite gone, recollects nothing about it. The neck and back quite relaxed, but complaining of the tension of his abdomen and of a severe shooting pain from his xiphoid cartilage to his spine, and of cramps in the thighs. I directed tobacco stupes to the belly and thighs, but did not blister the surface beforehand. The beef-tea, &c., to be continued.

July 1, 2, 3.—Getting gradually well, on the 4th he could open his mouth to the natural extent; the back and abdomen quite flaccid, in fact, I pronounced him cured, applied simple dressing to the blistered surfaces, and gave up the use of tobacco altogether.

In two days after he took a shivering fit, and afterwards went through a severe attack of pneumonia; however, he got quite well, and was discharged fit for work on the 12th of this month.—*Medical Times and Gazette, Sept.* 24, 1864, *p.* 326.

13.—TRAUMATIC TETANUS TREATED BY ICE TO THE SPINE.

Case under the care of Mr. ADAMS, at the London Hospital.

[The patient was a labourer twenty-eight years of age, suffering from gradually increasing tetanic symptoms following a wound from a nail under the left great toe.]

On admission, the jaws were firmly closed, so that he could only insert the extreme tip of the tongue between his teeth. He had a well-marked risus sardonicus, with pain and stiffness in the neck and back, and a tense, drum-like state of the abdominal muscles. The right lower extremity was perfectly rigid and extended. He was unable to flex the thigh or leg in the least. The ankle joint and toes were unaffected. On the left side the muscular rigidity was not quite so well marked, otherwise it was in the same condition as the other. He complained of frequent pains shooting down the legs, and also pain in the epigastrium on coughing. The wound did not suppurate, but was surrounded by a slightly inflamed areola. The muscles of respiration and those of the upper extremities were apparently unaffected. He had no dyspnœa; his pulse was quiet; appetite good; tongue clean; and action of bowels regular. Mr. Heckford, the house-surgeon, ordered him a drop of croton oil, and ice to be continually applied along the whole course of the spine. Two hours afterwards he could protrude the tongue to the extent of an inch. During the night he did not sleep well; but a still more decided improvement *took place as regards* the closure of the jaws.

On the following day (April 8th) he was able to bend the previously rigid right knee, and separate the teeth at least an inch. The croton oil was repeated, as the first dose had not purged him thoroughly. Took half a grain of morphia at night, after which he slept well.

9th.—So far improved that he was ordered a chop and two pints of ale (he did not have this until the following day). The paroxysmal pains in the lower limbs had, however, increased in frequency and severity, causing him to cry out; but it was only when these occurred that he had any marked spasm of the muscles of the legs. Morphia repeated. Slept well.

[The rest of the report up to the 5th of May, is one of gradual improvement.]—*Lancet, July* 16, 1864, *p.* 67.

14.—ON BELLADONNA IN EPILEPSY.

By Dr. JOHN C. THOROWGOOD, Assistant-Physician City of London Hospital for Diseases of the Chest, Victoria Park.

John F., aged twenty, of florid aspect, well grown, but thin, and poorly nourished, has been obliged to leave service in consequence of frequent fits. Now (March, 1860) he has three or four of these fits in a week; when seized, he feels giddy, with what he describes as a "hot rising into his head;" he then loses consciousness and falls down, and his parents tell him that his limbs are for a time convulsed, though not more on one side than the other, and then they become stiff; he does not bite his tongue nor foam at the mouth. The fit passes off in ten or fifteen minutes, and he remains giddy and with a dull headache for several hours after.

Some years ago he was troubled with ascarides, but these seem to have quite disappeared, and no connection can be traced between this affection and the fits.

His appetite is very variable; the tongue large and clean; bowels open; at times he is sick and retches much; when he is fatigued his limbs are apt to twitch; he often has headache and feeling of fulness, made worse by beer or spirits; at times his nose bleeds; he sleeps pretty well, but at times is very prone to dream; the fit sometimes takes him at night, when he cries out, and his limbs are convulsed: pulse 100, feeble; heart and lungs seem healthy; urinary organs present nothing unusual. Of late he has lived rather poorly, being ill off for means; and though he has by his own account had a variety of medicines, he is getting worse every week. The treatment consisted at first of a few pills of ext. hyoscy. and hyd. c. creta at night, with nourishing diet and a purgative when necessary. Whatever good this might have done in improving the stomach, it had no effect over the fits, and these seemed greatly to exhaust him. As he had already taken much medicine, I gave him now (about March 25) ext. belladonnæ, gr. $\frac{1}{6}$; aquæ, $\frac{3}{3}$ss.; M. ter. die.; and he is to leave off the pills and all other medicine. He took the belladonna for ten days, during which time he had no fit. He, therefore, was ordered to continue the same for another ten days, bringing him to April 15, 1860, without any fit.

At this period a very careful examination of the cervical spine detected a thickening over the upper vertebræ on one side, and this spot was rather tender. An emplastro iodinii was applied here. He says that he feels stronger and better, and now has no retching or sickness, still he has a wild excited look, and is evidently in a very nervous state. He got in place of the belladonna tinct. hyoscy. ℥xv., aquæ $\frac{3}{3}$ss., ter. die, for a week, with some relief, and then I lost sight of him for a time. By the middle of May it seems he was much better, and went one Sunday for a very long walk with his father; on his return feeling very tired, his father treated him to some gin, after which he had a fit, the first since he has taken the belladonna. On my seeing him a short time after, I ordered him the belladonna again for a fortnight in increased dose (gr. $\frac{1}{4}$, I believe, ter. die). The extract I knew to *be very good and* powerful; and as the patient was very uncertain in his *visits to me, I feared to* put him on a large dose. I did not see him again

till March, 1861, when I hardly knew him, he looked so stout and well; he told me the fits had left him, and though at times he felt nervous, still he was now married, and able to earn a good living, and believed himself perfectly cured; his neck was now to all appearance sound and well.

This case cannot be called a severe case of epilepsy, still the fits were so severe and so frequent that the patient had to leave a good situation, and these fits remained unamenable to apparently judicious treatment. Once the patient was told he had a tumour in his brain, but what led to this observation he did not inform me, and with the exception of the slight thickening over the cervical spine, which the iodine plaister soon dispersed, I could not detect anything indicative of actual organic disease in any part of the brain or system generally. The patient attributed this thickening to a fall from a tree some years before; at first he scarcely heeded it, but during the last twelvemonth, while the fits have been increasing, this swelling has been unusually tender. No special sensation was ever felt in it immediately before the invasion of a fit.—*Medical Times and Gazette, July* 23, 1864, *p.* 86.

15.—ON ACUPUNCTURE IN FACIAL NEURALGIA.*

By William Craig, Esq., Ayr.

[A few years ago Mr. Craig published a paper, in which he endeavoured to establish the identity between the nervous and electric fluids. The treatment he recommends in facial neuralgia he considers an illustration of the truth of this position.]

I have long believed that the essential cause of facial neuralgia is a superabundance of nervous fluid in the nerve affected, and that the proximate cause, in the great majority of cases, is a congestion of the nerve, bearing some analogy to capillary congestion in inflammation. I came to the conclusion that the treatment for the one form of disease should bear some analogy to that employed for the other; that as depletion is requisite in capillary congestion, so depletion ought to be beneficial in nervous congestion. I consider that, as nervous power and electricity are identical, when a conductor is presented or brought nearly into contact with a nerve, the overcharge ought to escape, and with it the pain, just as a charged Leyden-jar is discharged by a conductor brought near it. Full of this idea, when on my way to a case of facial neuralgia, about twelve years ago, I considered it a good opportunity to test my principles. The patient had been suffering from a severe throbbing pain shooting in the direction of the branches of the supra-orbital nerve. He had slept very little for some nights previously. I thereupon procured sewing needles, and inserted two or three of them near the place where the nerve issues above the eye. The pain went away immediately, and the patient was asleep shortly after. The pain has not since returned (August 28, 1863).

Since the above period I have treated a very large number of cases of this affection by the same means, and with the same success. I have taken notes of a few of them only, as there is little variety in the character or development of this affection, and none in the treatment.

Case 1, May 6, 1852.—J. R., aged forty-two, states that he has been subject to attacks of severe pain over and under the right eye for several years. The attacks generally continued for a month or six weeks in spring. He has now been ill for eight days, during which the pain has continued with very little intermission, and he has obtained scarcely any sleep. I applied the conductors above and below the eye, and the relief was immediate. He fell asleep in ten minutes afterwards. He told me he could not express the agony experienced at the onset of a paroxysm. In June, 1860, the pain had not returned.

Case 2, September 4, 1856.—Miss A. has been for four years subject to attacks of neuralgia *during each winter*. Each attack continued about six

weeks. She now complains of a severe pain of a burning and shooting character over the right eye and right cheek. "She has been obliged to go up and down stairs all night, unable to find a resting-place." She has had various internal remedies without effect. The conductors were inserted, and the pain left immediately. June, 1864.—It has not returned up to the present time.

Case 3, June 28, 1857.—Miss McJ. complains of a severe darting pain on the right side of the back of the neck and head. She has been suffering from it for a fortnight. The pain extends from a point two inches behind the ear to the crown of the head, at the situation whence issues the occipitalis major nerve. She has had no sleep for some nights. The conductors were inserted, and the pain went off immediately. June, 1864.—Miss McJ. states that she has not been troubled with pain since.

Case 4, March 12, 1858.—W. H. has during each winter for the last ten years been affected with neuralgia of the left side of the face of such severity as to prevent him from working. He has tried very many medicines and has had the nerves above and below the eye divided, the cicatrices being quite distinct. He has found no benefit from these means. When first seen his head was enveloped in several layers of flannel, and he was surrounded with awnings to prevent the slightest current of cold air from reaching him, as this immediately induced an intense paroxysm. It was with great difficulty that the treatment could be adopted, on account of the extreme sensitiveness of the skin of the face, but as soon as the needles were applied he was partially relieved. March 13.—He is considerably better, but very weak. He gradually got stronger, and was soon at his work again. May 23, 1860.—He has remained quite well for the last two years, and has been exposed to all kinds of weather.

It must be admitted that every case of facial neuralgia which I have met has not been cured by this means. But this admits of easy explanation; for in diseases of the nerves, as in those of other structures of the animal economy, there is sometimes inflammatory action and change of structure, producing neuralgic pain, in which case little benefit can be expected from acupuncture. In a case in which the mental nerve had been the seat of neuralgia, which had continued for ten years, the treatment by conduction was nearly fruitless. For twelve hours after the first insertion of the needles, the pain had completely gone, but afterwards it returned as before. In case 4, however, which was very much of this character, the cure was complete. Cases which have little or no intermission, and which have existed for years, seem to be amenable to no form of treatment; at the end of so long a time there may be a change of nervous structure. It is necessary to allow the needles to remain applied for ten or twelve hours. In a case from the country about a month ago, the patient required to leave town in three hours; in two days the pain returned, but on the reinsertion of the conductors it was removed. After twelve hours he left, and did not return. It seems to be necessary not only to draw off the quantity of nervous fluid which produces the paroxysm, but also to take off for a time its normal supply, in order to break off the tendency to a return of the paroxysm. That this is the case may be concluded from the fact that sometimes the textures for a small space around the situation occupied by the conductors are benumbed, as if the cutaneous nerves had been for the time deprived of their proper supply of nervous fluid. It has been found that electricity passes from the free ends of the needles during acupuncture. Dr. Dantu states that a patient, immediately on the insertion of a needle, felt a gentle electric spark affect the tissues in the vicinity of the needles. M. Schwerger has observed the same phenomenon attend his operation as electricity was made appreciable by the deflection of the electric multiplicator. These electric phenomena, though not sought after to support preconceived opinions, but related as mere matters of fact, are exactly what might be expected from the principles here advocated. According to this fact—viz., that electricity escapes from the needle in acupuncture, thereby relieving the nervous con*gestion which causes tic*—the induction of an electric current into a nerve

subject to neuralgia would be followed by an aggravation of the pain. This happened in cases reported by Dr. Althaus (*Medical Times and Gazette*, August, 1858). He states that Duchenne recommended to cause a strong revulsion by producing Faradisation with the metallic brushes, conveying a very powerful electro-magnetic current through the part affected. But the pain by this proceeding was, according to Duchenne himself, "so atrocious," that after applying it to several cases he was obliged to desist. This result of the experiments of Duchenne is exactly what might have been expected from the principle here advocated. The atrocious pain was just the consequence of sending into the already congested nerve a stream of the same element, an over-supply of which was agonising the patient. Electricity in the form of current has been frequently applied in neuralgic affection, but, so far as I know, with no favourable result. These French observers state that the electric phenomenon immediately consequent on the insertion of the needles is the result of the oxidation of the metal; but the effect is so instantaneous, the relief so speedy, that oxidation could not have begun before the cure was completed: the result is too great and too speedily effected to be accomplished by such a trifling amount of action, or, more correctly, no action at all.

For a considerable time after their insertion there is no impression of oxidation on that portion of the needle which is imbedded in the tissues, and long before that time the patient is cured.

It is no exaggeration that by this treatment the cure is immediate, as any one may satisfy himself by inserting a conductor of proper size in the proper place during a paroxysm in a pure case of facial neuralgia.—*Medical Times and Gazette, Sept.* 10, 1864, *p.* 277.

16.—ON NEURALGIA AND ITS CONNEXION WITH PAIN OVER THE SPINES OF CERTAIN VERTEBRÆ.

By M. Trousseau.

In the course of a clinical lecture on neuralgia, M. Trousseau makes the following observations: "If neuralgia occupied the branches of the trifacial, it was always at the point of emergence of the ophthalmic branches, and of the superior maxillary and the inferior maxillary, that the pain was felt most acutely; then came the frontal point where pain rarely failed, then the parietal point where it was more frequently wanting; lastly, the occipital nerve, although having no relation of origin with the trigeminal, is almost always affected. An extraordinary thing—inexplicable, but invariable in all the cases which we have carefully observed and noted—is that whether the trifacial were alone attacked, or the occipital nerve simultaneously affected, pressure on the spinous apophyses of the first two cervical vertebræ was *always* very painful, and in a certain number of cases immediately awakened the pain in the affected nerves. If the nerves of the brachial plexus were attacked, invariably pressure over the spinous apophyses of the last cervical vertebræ produced pain, and it was the same when we explored the vertebral column in the case of intercostal, lumbar, and sciatic neuralgia." M. Trousseau lays it down as a rule, that in the various neuralgias the spinous apophyses are painful at a point nearly corresponding to that at which the nerve emerges, and not unfrequently the pain extends a little higher up the vertebral column.—*British and Foreign Medico-Chirurgical Review, July*, 1864, *p.* 255.

17.—ON THE LOCAL APPLICATION OF CHLOROFORM IN NEURALGIA AND MUSCULAR RHEUMATISM.

By Dr. Dupuy de Frenelle.

Dr. Dupuy maintains that idiopathic neuralgia and muscular rheumatism are two varieties of the same disease, the nerves of common sensation being equally affected in both, and the causes of both being the same. The treatment by chloroform is not new; but Dr. Dupuy contends that no one has previously employed it in the same manner as himself, except the late Dr. Aran. Dr. Dupuy states that he has discovered the means of inducing every variety of local irritation by the contact of chloroform, from simple rubefaction to vesication, the revulsive action being necessary to the success of the plan, which is described as follows: The middle of a piece of fine and well-worn linen is introduced as a stopper into a phial of pure chloroform, which is inverted so as to impregnate with the fluid a more or less considerable portion of the compress, according to the extent of the skin to be acted on; the linen is then laid over the seat of pain, and with the palm of the hand is kept in close contact with the skin; but when the pain is limited to one spot, as in some cases of intercostal, facial, supra-orbital, or auricular neuralgia, the pressure of the thumb or forefinger is sufficient. Several successive applications should be made at once when the pain occupies more than one region, or when it exists along the entire course of a nerve, as in sciatica. In the latter case, the chloroform should be applied over the ischiatic notch, the head of the fibula, or the external malleolus, from the origin of the nerve to its termination. Dr. Dupuy brings forward in support of his practice a hundred and fifty cases, in which the largest number of applications of chloroform was twelve, and this number he has never exceeded.—*British and Foreign Medico-Chirurgical Review, July* 1864, *p.* 246.

18.—ON THE EMPLOYMENT OF LOCAL INJECTIONS IN NEURALGIA, PARALYSIS, AND OTHER AFFECTIONS.

Professor Courty, of Montpellier, has published a note on the efficacy of local injections of strychnia in the treatment of paralysis of the facial nerve. He injected a few drops of a solution of this alkaloïd along the course of the facial nerve, between its exit by the stylo-mastoïd foramen and its passage to the neck of the condyle of the lower jaw. The injection was repeated every two or three days, and three injections at the least, and six at the most, sufficed to remove entirely, in the space of from ten to fifteen days, every trace of paralysis in all the muscles of the face. The patients were a man aged fifty-six, a lady of twenty-five, and a young woman of twenty-two. In all three cases the cure was complete. M. Courty has also recorded a case of paralysis which lasted for a year, and had been ineffectually treated by various remedies, but which was cured by a few injections of strychnia, performed over the inferior extremity of the spinal cord. M. Luton, of Rhiems, has also called attention to the use of local injections in various maladies. He has successively employed a more or less concentrated solution of nitrate of silver in 12 cases of sciatic neuralgia, 2 cases of intercostal neuralgia, 3 of coxal neuralgia, &c. M. Luton has also mentioned a curious case of suborbital neuralgia removed by three injections of salt water. He has three times employed injections of tincture of iodine in parenchymatous goitres; one case was cured, the other two were still under treatment at the date of the report. The applications of which this new plan is susceptible are very numerous, and may include the use of bichloride of mercury, arsenious acid, sulphate of copper, sulphate of zinc, and any other irritating substance which acts in the interior of the tissues in the same manner as one which is applied on their surface.—*British and Foreign Medico-Chirurgical Review, July 1864, p. 241.*

19.—MUSCULAR PAIN: A LOCAL ANÆSTHETIC.

The Bulletin Médical du Dauphiné supplies us with the following formula, which would appear to have been often found useful for the relief of the severe pain coincident with muscular rheumatism.

℞. Tinct. aconiti, ʒj.; adipis, ʒij.; chloroformi, ʒj.; morphiæ muriatis, gr. xv.

Mix the tincture with the lard, and add the chloroform and morphia in a glass mortar; keep in a wide-necked and well-stopped phial.

The ointment is applied with a feather over the seat of pain, and the parts are then covered with cotton wool and a water-proof cloth. This dressing should be repeated every hour until relief has been obtained.—*Journal of Practical Medicine and Surgery, July* 1864, *p.* 300.

20.—PARALYSIS OF THE HAND AND FOREARM FROM SMOKING.

By Dr. A. J. H. Banks, Evercreech, Somersetshire.

[Dr. Banks considers that impaired nervous energy and even actual paralysis produced by the excessive use of tobacco are much more common than is usually supposed. All or most will probably endorse the assertion of impaired nervous energy resulting from excessive smoking, but cases of actual paralysis resulting from this cause must be very rare. We never remember seeing one which we could without any doubt assign to this cause.]

In February last I was consulted by P. S——. He was forty-eight years of age, tall, well made, and of rather spare figure. I found him suffering from what he called "dropped hand." It had lasted for some considerable time, and he had been under treatment for it; but finding it get worse, had discontinued for some weeks before applying to me. Recently, owing to a good deal of numbness and uncomfortable sensation down the right side and right lower extremity, he began to fear he should be unable to pursue his work (which was that of gate-keeper on the Somerset and Dorset line), and this induced him to apply to me. I ascertained that he was an inveterate smoker, and, supposing this to be the cause of his ailment, I advised him to refrain from the habit altogether. I then gave him an ordinary purgative, placed blisters on the back and front of the arm and forearm, dressed these with an ointment composed of one grain of strychnine and one ounce of spermaceti ointment, and subsequently gave him one-sixteenth of a grain of strychnine in a pill three times daily. In ten days he had perfect use of the limb, lost all his disagreeable symptoms, and has been at work regularly since that time without any signs of it returning. He tells me that he thought, and that the surgeon who was attending him was of the same opinion, that the loss of power arose from local external injury; and that it had been treated by cold lotions and wet bandages, assiduously applied, under which it became gradually worse. I think this mistake is not unfrequently made.—*Lancet, Sept.* 3, 1864, *p.* 280.

DISEASES OF THE ORGANS OF CIRCULATION.

21.—ON FATTY DISEASE OF THE HEART.

By Dr. Henry Kennedy, A. B., Physician to Sir Patrick Dun's Hospital.

Of the several symptoms of fatty heart few have been more noticed than the state of the pulse. When I spoke on this point myself some years back, I thought well of using caution in what was then advanced. But I believe I may now say that of all the symptoms, it, as a single one, affords us the most assistance in the diagnosis, and for the sake of description may be

spoken of under three aspects: the slow pulse, the natural one, as regards the number of beats, and the rapid one. I do not say anything of the irregular or intermitting pulse, as in my own observation it has been rare, and is common, I believe, to all states of the pulse, whether the heart be fatty or not.

More has been written about the slow pulse of fatty heart than any other variety, and it is unquestionably a very valuable sign, and always sure to catch attention. In this city it has been brought under notice by Drs. Stokes and Fleming, and our able Secretary, Dr. Richardson, has detailed some interesting cases. I would ask, however, is it usually present? I believe not. Within my own experience it has been very exceptional; but I am speaking now of a pulse under 40. When it does occur, however, there appear to me to be very good grounds for supposing that the true fatty degeneration exists, and the greater the degree of the latter the slower the pulse becomes. On a former occasion I expressed my doubts as to what state of the organ gave rise to the very slow pulse, but a more enlarged observation has enabled me to arrive at the conclusion stated. The comparative rarity, however, of this very slow pulse lessens, I must repeat, its value as a diagnostic sign.

But the pulse of fatty heart may be the very opposite to slow, being permanently quickened, and of course above the natural standard. On this point I can find only the slightest allusion in the works on diseases of the heart, the bare statement being all that is said on the matter, yet I am satisfied it is one of very great consequence, and still affords a wide field for accurate observation. It is some years now since this particular part of the subject was gradually forced on my notice by the fact, which has indeed been long known to physicians, that fat people are, *cæteris paribus*, bad subjects for fevers. Whilst seeking some cause for this, I found that in such the heart had always more fat on it than was natural, and this state, coupled with what we know now of the effects of the fever poison, explains clearly the results. Two such instances I saw some time back, both ending fatally. One of these, a case of scarlatina in a gentleman close to forty years of age, was seen by Drs. Hutton and Nowlan; and the other a gentleman somewhat older, was attacked with typhus, and was seen by Drs. Corrigan and Lyons. In both cases the pulse became very rapid, and early, it is to be observed, in the attack. Dr. Stokes has in his able work called attention to the great importance of recognizing this state of the heart in connexion with acute diseases. But it is the great and early occurrence of a rapid pulse, especially in men to which I wish to draw particular attention. When this state of the heart exists, any disturbance of the system may set the pulse going at this rapid rate, and in Latham's elegant work will be found some marked instances of this; at the same time he avows there were no symptoms present to lead him to suppose the heart was fatty, though proved to be so after death. In fact, it does not seem to have struck him that there was any connexion between the rapid pulse and the state of the heart. But this, as it appears to me, is the point of importance to notice. It is now several years since a case got into the public papers and so acquired but too much notoriety. Some whom I address may recollect it, as being the case of a gentleman who, for an attack of gall-stones, got laudanum, necessarily in very full doses, which acted so powerfully as a narcotic that it required some hours' persevering efforts to recover him. When recovered, he came up to Dublin a few days later, where he was seen by different medical men, who could discover nothing wrong but a quickened pulse. In this state he lived more than a week, and then died. On post-mortem examination the heart was found fatty—a state which had not been suspected previously.

But it is not in acute disease alone that a great rise in the pulse, and in connexion with fatty heart, may be found. There appear to me very good grounds for supposing that in the progress of some cases the pulse becomes quickened *and remains permanently so*. On this point, however, I would speak with caution; not because I doubt the fact, but because its complete *elucidation* demands a kind of experience which it is hard to acquire—I

mean an experience which extends over years. One such case which has been seen by Dr. Stokes has, however, occurred to me. A gentleman, now above forty, and of a full habit of body, has been under my observation for eight years. Till about four years ago the pulse remained at the healthy standard, beating 70 in the minute. At that time, and just after a severe cold, I observed that it did not fall, as was to have been expected, for the other symptoms had disappeared, and from that time to the present the pulse has never fallen below 90, and I suspect never will. There is not the slightest reason to suspect any lurking disease in the lung which might keep up the pulse, and there is no morbid sound to be heard over the heart. But there are grounds for believing that gout is in his system, and when older he will very probably have the disease.

In another case, a woman of 50, whom I saw with Mr. McCarthy of Capel-street, I suspect the same thing has occurred. When I saw her she laboured under pleuritic effusion, from which she entirely recovered, but the pulse has never fallen to the natural beat, and is now capable of being raised from the slightest cause. Neither is there in this case any ground whatever for suspecting that the lungs are affected, whilst there are good reasons for supposing that the poison of gout is in the system. This part of the subject is still very open for investigation.

I have reserved for the last place some remarks on the kind of pulse in connexion with fatty heart, which I believe to be of far the greatest consequence to recognize, and for the simple reason that it is by much the most common—I mean when the pulse beats neither too slow nor too quick, but at the healthy standard. When speaking on this point in 1859, I did so with some reserve. Since that period, having become connected with Sir Patrick Dun's Hospital, my opportunities for observation have much increased, and I am now able to state that the diagnosis can very generally, if not always, be made. It will be recollected that the great probability is you will have no morbid sound to guide you at all, whilst the pulse will be beating at 65 or 70. Under such circumstances, what then can be a guide? The answer is the kind of pulse which would be caused exactly by the state of the heart described already this evening, as being much the most common to meet. The organ being enlarged, if I may so speak, the pulse is so likewise, at the same time that the beat to the finger does not convey the idea of strength, nor when we call to mind the state of the heart could we expect it. The pulse in fact is large and compressible, passing sedately as it were under the finger, and to it may be well applied the words of John Hunter, that it is action without power. When, then, such a pulse is found, we are now, I believe, justified in concluding that fatty heart exists. In saying this it is not to be supposed that we are to exclude other symptoms if present, for every point is to be seized in order to render our diagnosis the most accurate, and the question to be solved is avowedly a very difficult one. All I would convey is this: that in these cases, and mainly from the absence of valvular disease, other symptoms are very apt to be so obscure as to be entirely overlooked, but that the pulse and its characters are just as likely to be present, and so afford us the most valuable aid.

To enter further here into the diagnosis of fatty heart would be quite foreign to the purpose of the present paper. Neither, with two exceptions, is it my intention to speak of the causes of the disease; but I must notice one which has year after year been gradually forcing itself on my attention till it has now reached the strongest conviction in my mind—I mean the habit of smoking, which, I believe, I have traced in many instances to have been the predisposing cause of the disease. No one is more aware than myself of the difficulties which beset a question of this sort, nor the great opposition which, for obvious reasons it is likely to meet. Still the opinion has not been taken up hastily, nor, as I think, without such proof as the subject admits of. All will recollect that within a very few years a great paper war was carried on in the pages of the Lancet on the effects of tobacco, and the opinions expressed were sufficiently contradictory. Amongst them all, however, I did not observe one point noticed which seems to my mind of great

importance in this question. It is the fact that if any one, no matter what his temperament may be, gets out of health, so that the powers of his system are lowered, he must then either lessen his smoking or give it up entirely. I have met no exception to this statement, which every one may test for themselves—as, for instance, in cases of paralysis, no matter how slight they may be. From the fact, however, I conclude that tobacco, besides other effects, is a depressor of the nervous system, and that there is a constant antagonism going on between it and the healthy state of the constitution, and when used too freely it ultimately engenders a state of health which is very apt to be followed by a fatty heart. At any rate, whatever the explanation be, the fact is as stated above, and I have seen now too many cases of fatty heart, in what are called heavy smokers, to have any doubt on the matter. Though more might be said on this point I must hasten on.

Want of proper exercise and sedentary habits, are mentioned by all writers as leading to the disease. There is one way, however, which, though very obvious when mentioned, seems to myself particularly injurious—I mean men allowing themselves to be constantly carried about in their carriages—passive exercise as it is called—which is a kind of contradiction in terms, at least it appears so to me. To what else, I would ask, can we more fairly attribute the fact which must have struck all present?—I mean the way in which the leaders of our own profession have been cut off within the last few years; and what is still more remarkable, is the fact that out of six or seven of them, three actually suffered from that very rare disease—angina pectoris—and they were men in or about sixty years of age. To us, as physiologists, and therefore knowing something of the laws which regulate health. I believe the point brought under notice to be one of considerable moment, but I cannot pursue it further on the present occasion.

It was my intention to have entered at some length into the treatment of fatty heart—I mean in its earlier stages—when, I believe, it may now be very frequently, if not always, diagnosed; but time forbids. I can therefore only state in a general way that I believe the disease quite capable of being stayed, if not cured. When I spoke on this point in 1849, I gave one case in which there was every reason to suppose a weak heart existed, and in which by perseverance for some months, a cure either was or seemed to be effected. The lady lived eleven years subsequently and died of a different disease. To this case I could now, did time allow, add others, where I cannot doubt the disease has at least been stopped. In our efforts to cure it, it is not to be forgotten that, generally speaking, there is no valvular disease to contend with, and the mere deposit of fat cannot be looked upon as an insurmountable obstacle to cure. In fact, whilst fatty heart may in one aspect be considered as serious as any other organic disease of this viscus, it may on the other hand, be placed as the most curable of its several affections.

As to any specific line of treatment, I know of none. The question resolves itself into those great general principles which should guide us in our efforts to improve the general health, and must consist more in attention to what is known as hygiene, than to any specific medicines whatever. A healthy state of the digestive organs and skin, a regulated quantity and quality of food, proper clothing, and above all, a well-directed system of bodily and daily exercise, are the measures we must look to. It must, indeed, be admitted that cases do occur where directions of this kind cannot, or rather will not, be followed, for many would rather take the most nauseous medicines, than restrict themselves in diet or alter their habits; and that the plan is one of restriction, and demands great firmness and discretion on the part both of patient and medical adviser, admits of no doubt. Still difficulties of this kind must be overcome, and offer certainly no reasonable objection to a plan of treatment which has physiology for its basis, and the certainty of at least benefiting, if not curing, the patient.

Whilst following it, however, care must be taken that it be not pushed too fast. "*Festina lente*" is to be the rule, and time must form an important *element in the treatment*; otherwise the constitution of the patient—and each

must of course be studied separately—may be strained too much, and serious injury follow. A very striking, or what might better be called an exaggerated example of this, has recently occurred in the person of the celebrated Heenan, whose constitution, there can be scarcely a doubt, suffered from the treatment he was subjected to before the last prize-fight, as in the course of eleven weeks he was reduced a stone and a half in weight, very nearly two pounds a week. I cannot, however, quite agree with the writer in the Lancet, who, when speaking on this subject, draws a distinction between muscular and vital force. It is not easy to understand how the muscular system should be so developed and powerful, as it was admittedly in Heenan, and yet the vital force be so impaired. But enough of this, if only a caution be taken from the circumstances.

In concluding these remarks, which, from the extent of the subject, forced me to omit much, I would just repeat that my chief object has been an endeavour to advance the diagnosis of fatty heart in those cases—by much the majority—where no valvular disease exists; and that the conclusions arrived at are based directly on the study of the morbid anatomy of the organ as found in this disease.—*Dublin Medical Press, April* 20, 1864, *p.* 404.

22.—ON THE EFFECT OF TOBACCO ON THE HEART.

M. Decaisne, in a communication addressed to the Academie des Sciences, exhibits another clause in the heavy bill of indictment against the use of tobacco. He states that in the course of three years he has met, among eighty-eight inveterate smokers, twenty-one instances of marked intermittence of the pulse, occurring in men from 27 to 42 years of age, and not to be explained by organic lesion of the heart. The absence of such lesion or other condition of health capable of inducing intermission of the action of the heart, and the fact that in nine of these instances, in which the use of tobacco was abandoned, the normal action of the organ was restored, M. Descaine believes, will justify him in concluding that, in certain subjects the abuse of tobacco may give rise to a condition which may be termed "narcotism of the heart," characterised by intermission in the movements of that organ and in the pulsations of the radial artery; and that, in some cases, a suspension or diminution of the practice of smoking is sufficient to cause an entire disappearance of this irregularity.—*Medical Times and Gazette, June* 18, 1864, *p.* 671.

DISEASES OF THE ORGANS OF RESPIRATION.

23.—ON THE THERAPEUTICS OF CONSUMPTION.

By Dr. Richard Payne Cotton, Physician to the Hospital for Consumption and Diseases of the Chest, Brompton.

There are few maladies in which Medicine has borne so variable a reputation as in consumption. We have heard over and over again of "specifics" for this disease; and over and over again has the honest but disappointed physician expressed his conviction that phthisis is scarcely, if at all, under his control.

In the present communication, I shall endeavor to show that whilst, on the one hand, we may confidently assert that at present, at least, we have no *specific* remedy for consumption, we may, on the other hand, as confidently assert that our materia medica offers a number of auxiliaries of more or less value and importance in the treatment of this disease.

With a view to determine experimentally the comparative value of different therapeutic agents in consumption, I resolved, some five or six years ago, to prescribe certain substances to a given number of my hospital in-patients, taking care *that such cases were* free from all acute symptoms, as well as

from any particular complications requiring special treatment. Each of the following substances was thus administered to twenty-five patients, this number having been chosen as sufficient, perhaps, for the purpose, and as possessing the advantage of at once showing a *percentage* simply by multiplying by four; viz., phosphorus; liquor potassæ; hydrochloric acid; iodide of iron; iodide of potassium; chloride of sodium; vinum ferri; glycerine; sesquichloride of iron; chlorate of potassa; quinine; phosphoric acid. The experiments were published from time to time in the Medical Times and Gazette; but hitherto they have not been compared with each other, neither has any attempt been made at a general conclusion.

Here I may as well anticipate one or two very possible objections to my experiments. Some may consider that the number of cases was too small to show any satisfactory result; whilst others may suggest that there could not have been sufficient similarity in the cases themselves to render the several conclusions trustworthy. To the first objection I would reply, that it has taken me five years to observe the effects of these twelve substances upon the number of cases I determined upon: and as there is a limit not only to life, but also to the time and capacity for working out such observations, I felt it was impracticable to augment the number of patients for each experiment. And to the second objection I would remark, that, as it is no inconsiderable difficulty to discover even *two* consumptive persons strictly alike in all their symptoms, the attempt to find *twenty-five* parallel cases would have been fruitless. I may, however, at once state that I am desirous that my experiments should be taken only for what they are worth. They do not profess to be absolutely conclusive, but are simply intended as certain additions to that general stock of therapeutical facts, which when put together and thoroughly sifted, may, some day or other, lead to the discovery of important truths.

Phosphorus in many cases proved a good tonic and stimulant, increasing both the appetite and strength of the patients. In a few instances, however, it obviously disagreed, producing nausea, a distaste for food, and abdominal derangement. Four of the patients (three of whom were in the first, and one in the last stage of the disease) left the hospital materially improved in every respect, and apparently with their general health restored; but several of those who derived no benefit from the phosphorus improved afterwards under other treatment. It was administered according to the formula of the Prussian Pharmacopœia, in which four grains of phosphorus are held in solution by an ounce of olive oil. Of this solution, from five to eight minims were given in a little mucilage twice or three times a day. Five minims contained one-twenty-fourth of a grain of phosphorus.

Liquor Potassæ, given in doses of fifteen minims two or three times a day, altogether failed to support its ancient reputation as a "deobstruent" in phthisis. It seemed generally to be inoperative; and a considerable number of patients, in whom it had produced no visible effects, improved afterwards under different treatment.

Hydrochloric Acid, on the other hand, was productive of very marked benefit. In doses of ten or fifteen minims of the dilute acid two or three times a day, it generally increased the appetite and lessened many of the urgent symptoms, especially excessive perspiration and pulmonary secretion. Under its continued use for several weeks, many of the patients materially improved.

Although, in certain dyspeptic conditions, the alkalies, in various combinations, are oftentimes of very great use in the treatment of phthisis, it appeared that the acids *separately* are far better suited to the so-called tubercular "crasis," than the alkalies *separately*. Several patients, who had made no progress under the liquor potassæ, improved very much when this was changed for hydrochloric acid; and four, who had been taking the acid with advantage, but to whom liquor potassæ was given as a change, urgently begged to return to the acid. In very few instances did the hydrochloric acid disagree; *now* and then it appeared to cause a little gastric pain, but it was never *necessary permanently to* abandon its use. I have reason to believe that the

other mineral acids will be found as effective as the hydrochloric. The circumstance, however, that the free acid frequently occurring during healthy digestion is the hydrochloric, and that this acid is the well-known solvent of the plastic constituents of the food, made me select it in the present experiments; but I have often prescribed both the nitric and the nitro-hydrochloric acids in the wards of the hospital at Brompton with the same good results.

Iodide of Iron, in doses of a drachm of the syrup two or three times a day, appeared greatly to benefit several of the very early cases, and was particularly serviceable in some which exhibited evidence of chronic secondary pneumonia. In a few instances it was discontinued, on account of its producing headache, with dyspeptic symptoms; but, as a general rule, it seemed to agree with the patients. Under its influence, many gained materially in weight.

Iodide of Potassium, which was at one time held in great repute in the treatment of phthisis, seemed remarkably ineffective in most of the twenty-five cases in which it was employed. Occasionally it gave rise to some dyspepsia, but more commonly its administration was unattended by any decided result. Only six cases materially improved during its use; and, in nearly every instance in which it was given, the patient's weight sensibly diminished.

Chloride of Sodium was prescribed in doses of from one to three drachms dissolved in water, two or three times a day. It was observed that one drachm could generally be taken without producing nausea; that, in a few instances, two drachms caused a slight degree of sickness; whilst three drachms sometimes gave rise to actual vomiting; although, as a general rule, these larger doses, when gradually arrived at, did not disagree with the stomach. A larger dose could be taken after meals than when the stomach was empty. In only three cases out of the twenty-five was thirst complained of. In this experiment, a fair number of patients increased in strength and appetite, justifying the conclusion that, in many such cases, common salt deserves to rank as a tonic.

A large amount of chlorides was always found in the urine of those who were taking the salt; but so little in relation to the quantity administered, that much must either have passed off by the bowels, or remained in the system. The search after chlorides led to the discovery that the urine of all consumptive patients, even of those in the last stage of the disease, contains such compounds in considerable quantity.

Steel Wine proved a most valuable auxiliary in the treatment of a considerable number of patients. It seldom disagreed, but tended rather to increase the appetite and improve digestion. In the cases of children and young persons, especially when combined with cod-liver oil, it was remarkably efficacious. The dose varied from two drachms to half an ounce, and in a few cases to an ounce, once or twice a day. From half an ounce to an ounce of the vinum ferri, taken immediately after dinner, is, I believe, one of the best means of administering steel in phthisis. In this way, it seems to interfere less with the function of digestion, and to be more easily absorbed into the system.

A well-known French author, M. Trousseau, has recently asserted his belief that steel, so far from benefiting, not unfrequently engenders phthisis. Has he not here failed in the distinction between the *post* and *propter hoc?* Unquestionably, many become consumptive in spite of all the steel we can get them to take; but, if consumption were connected with steel-taking, assuredly this disease would be even more common than it is!

Glycerine has been much extolled by some writers as a remedy for phthisis. I gave it in doses of from one to two and occasionally three drachms twice and sometimes three times a day. In five of the twenty-five cases, it either caused sickness or otherwise disagreed. Sometimes it seemed to relieve the cough; but few of the patients derived any material benefit from its use, and very few of them gained anything in weight whilst taking it. Several, who made no progress *under the glycerine*, improved subsequently under cod-

liver oil. Many observations were made in reference to the comparative usefulness of these two agents, the result being invariably and very considerably in favour of the oil. In ordinary cases of phthisis, indeed, I believe glycerine to be of little use.

Sesquichloride of Iron conduced to some very happy results. A large number of cases improved under its use; and six patients in whom the disease was decidedly established, after having taken it for several weeks, left the hospital with their health restored, and in a condition to resume their respective occupations. Were I to be asked which of the twelve remedies I had found most generally adapted to the ordinary run of consumptive cases, I should say the sesquichloride of iron. For several years I have been in the habit of prescribing it in doses of from ten to fifteen minims twice a day; and the mixture of the sesquichloride of iron is one of the most frequently employed formulæ of the Consumption Hospital *Pharmacopœia*. In private practice, it may be agreeably taken in combination with lemon or orange syrup; and in this way it may be continued with advantage for many weeks, and even months.

Chlorate of Potassa, like glycerine, has won various opinions as to its influence upon consumption. By one physician it has lately been considered even as a *specific*, whilst by others it has been deemed altogether a failure. After a careful analysis of its effects in the twenty-five cases, I came to the conclusion that it was very far from a *specific;* and that its usefulness was seldom apparent, except in those cachectic cases in which it and allied remedies are frequently serviceable.

Quinine, in doses of one or two grains twice and sometimes three times a day, although well suited apparently to a few of the twenty-five cases, generally produced disappointment; its effects being, as a rule, inferior to those of many other tonics. It would seem that quinine, like chlorate of potass, is chiefly useful in a certain class of cachectic patients, in which, irrespective of the tubercular condition, such agents are very generally prescribed. Several of the patients, however, who had scarcely benefited by the quinine alone, improved subsequently under the combined influence of quinine and iron.

Phosphoric Acid was prescribed in doses of fifteen minims two or three times a day. In three or four cases, it seemed to produce pain in the bowels, with nausea and loss of appetite; although, as a general rule, it agreed very well. Only three cases improved materially under its influence; whilst several of those in whom there was a moderate improvement benefited much more under other treatment. Phosphoric acid may be classed amongst the mineral acids as a general tonic in phthisis, but seems to me inferior to some of them, especially the *hydrochloric*.

The following table shows the general effect of these various agents upon the twenty-five cases in which they were administered.

	Considerably improved.	Slightly improved.	Not improved.
1. Phosphorus	4	5	16
2. Liquor Potassæ	1	2	22
3. Hydrochloric Acid	11	6	8
4. Iodide of Iron	10	4	11
5. Iodide of Potassium	6	5	14
6. Chloride of Sodium	8	6	11
7. Vinum Ferri	13	3	9
8. Glycerine	4	2	19
9. Sesquichloride of Iron	12	5	8
10. Chloride of Potassæ	5	4	16
11. Quinine	7	5	13
12. Phosphoric Acid	3	8	14

It must not be forgotten, in estimating the therapeutic value of these *different substances*, that this is illustrated by the table rather *comparatively than absolutely; since* we must not exclude from its proper share in the

result the combined influence of hope, rest, good diet, and general hygiene, under which, even by itself, many phthisical cases are well known to undergo very considerable improvement. The figures, indeed, can only be taken as a fair expression of the *comparative* usefulness of the various agents as therapeutical auxiliaries to general treatment.

The experiments to which I have thus briefly alluded, and which will be found more fully described in my papers in the *Medical Times and Gazette*, appear to be suggestive of the following general conclusions.

1. Since, during the administration of each one of the agents described above, several cases of phthisis were observed to run through the various phases of the disease, some even to a fatal termination, it is obvious that, whatever the amount of benefit which in some cases followed their use, no one of such agents deserves the title of "*specific*."

2. It may, therefore, fairly be concluded that the good effected by any of these agents was due to their respective tonic and upholding influence upon the general system.

3. In the majority of phthisical cases, steel (especially the sesquichloride of iron) and the mineral acids appear to be the most effective; but tonics generally are productive of more or less improvement.

4. Since, however, even steel and mineral acids, as well as other useful tonics, are undoubtedly inert in a certain proportion of cases, it is not improbable that there are varieties or modifications of phthisis, each of which may require a particular treatment. As there are special varieties of many other diseases, requiring special modes of treatment, and yielding to none other, it is possibly the same with consumption.

I would observe, in conclusion, that I think we are too apt to consider and treat phthisis as a separate and always similar disease, disregarding the almost endless varieties it presents. Whether such varieties are dependent upon original differences in the nature of the disease itself, or are determined by peculiarities in the individuals it may attack, we at least have before us a large field for experimental therapeutics. My own idea is, that the time is not distant when observation and experiment will show that, under certain at present obscure conditions of system, phthisis assumes definite and special forms, each of which requires definite and special management; and that the "therapeutics of phthisis," which many may now consider both unsatisfactory and unpromising, will contribute, some day or other, to great and unexpected results. I am not so sanguine as to look for a *specific* in consumption; but that the disease will ultimately prove as amenable as many others to proper management, is, I hope, not a mere day-dream.—*British Medical Journal*, *June* 25, 1864, *p.* 683.

24.—ON THE INFLUENCE OF LIVING IN THE OPEN AIR IN THE TREATMENT OF PHTHISIS.

By Dr. James Blake, of San Francisco, California.

Dr. Blake relates the particulars of seven cases showing the influence of what may be called the out-of-door plan of treating phthisis. The treatment pursued was to direct the patients to live entirely in the open air during the summer months, at an elevation of from three to five thousand feet above the sea, in the coast range of the mountains of California, where the temperature is very equable, and no rain falls for five or six months. This part of the world seems peculiarly well adapted for carrying out the treatment, for when the rainy season arrives, a sea voyage of four or five days may take the patient to Northern Mexico, where the winter climate is exactly analogous to the summer climate of San Francisco. The patients are directed not even to sleep in tents, but out under the trees, and the diet is plain camp fare, sufficient game being found in the mountains to keep the camp supplied. Dr. Blake believes that when the patients are able to avail themselves of the advantages of the summer climate of California, and of the winter climate of

Mexico, there are few cases that cannot be cured, except those in which the destruction of lung tissue is already too extensive. In the instances related by Dr. Blake, the patients had tried the usual remedies without any benefit, and the constitutional and local symptoms were carefully investigated, and the weight of the body was ascertained from time to time. The increase of weight is a more evident and tangible indication than any other of the success of the treatment, and this favourable result was observed in all Dr. Blake's cases.—*British and Foreign Medico-Chirurgical Review, July* 1864, *p.* 249.

25.—ON THE USE OF BROMIDE OF AMMONIUM IN HOOPING-COUGH.

By Dr. R. Peel Ritchie, Extra Physician to the Royal Edinburgh Hospital for Sick Children, and Physician to the Royal Dispensary.

[This drug was first introduced into practice by Dr. Gibbs, of the Westminster Hospital, and has since been extensively tried and commented on by many physicians and writers. Its action upon the throat and upper part of the respiratory mucous membrane seems chiefly to entitle it to notice.]

I commenced to use it in January last, and have since frequently prescribed it. The general result in the cases to which it was given has been relief to the spasmodic cough, and to the characteristic symptom of pertussis—the hoop, but I cannot say that the effect has been uniformly successful.

In prescribing a new remedy it is to be desired that we ascertain in what kind of cases benefit may be expected, and more especially is this desirable when a new agent is suggested for the treatment of such a disease as hooping-cough, for which probably more specifics have been recommended than for any other malady.

Dr. Gibb does not allege that it is invariably successful, but that it is exceedingly valuable in many cases. Professor Harley of London University, who has used the remedy extensively remarks that it "does not appear to act by removing the cough, but simply by preventing the occurrence of its chief and most disagreeable symptom, the hoop."

The result of my observations of the remedy have not been so favorable as those published by Dr. Gibb and Professor Harley. I shall therefore endeavour to indicate shortly what I have observed.

1st, As to the ages of those treated. The youngest was three months, the oldest, four years; the average age twenty-three and a half months. In my experience the remedy appears to be most successful in children whose age exceeds two years.

2nd, As to result. In nearly all the cases there was marked relief to the hoop after a few doses had been given. This was very noticeable in those instances in which the hoop was very frequent; the report given shortly after first administration in several of such cases was that the child was much relieved.

Infants are probably more liable to bronchitic attacks, and in them uncomplicated pertussis is more rarely met with than in older children. For this reason it possibly is that the bromide is less beneficial in those under the age of two years than in those above that age. In three of the cases death occurred—in two from bronchitis, and in the third from congestion of the lungs; but in all these the hoop had been relieved, especially in the two younger, aged three months and twenty months. In the eldest, aged two and three quarter years, the relief was not quite so apparent.

3rd, As to the dose. The quantity I have generally given has been from three to twelve grains a-day, in divided doses, administered every six hours.

These quantities are less than those prescribed by Dr. Harley, who gives a grain for each year, every eight hours, but this is regulated by the constitution and development of the child. In children of advanced age he gives large *doses. In the Lancet* of January 16, 1864, the particulars of two cases are *given; the one a girl aged* nine, took twenty-seven grains a day apparently

week; the other, a boy aged six, took eighteen grains daily for the same time. In both very speedy relief occurred. In the same journal instances of a child aged three years taking eighteen grains, and of another child aged four, taking twenty grains a day are given.

Dr. Gibbs gives two or three grains thrice daily to infants, and four, eight, or even ten grains, as frequently to older children.

I have not prescribed a larger dose than twelve grains in the day, but I have given that amount with benefit in cases of the age of four, three, and two and a-half years. Eight grains a day I have given to children two or three years old. In one instance a child of two years old, by mistake on the part of its mother, was given without injurious effect, fifteen grains a-day for four days, but this large dose was not attended by a better result than usual. Drs. Gibb and Harley prescribe it thrice daily in water, I prefer giving it four times a-day in syrup. If bronchial irritation exists, a mixture of ipecacuanha and squill is required. Dr. Gibb is also of this opinion. In complicated cases the administration of an emetic I find to be frequently necessary.

4th. In what cases is it suitable? In the *Lancet* of September 26, 1863, it is stated that Dr. Gibb had, in 1862, used the bromide in the treatment of hooping-cough largely, and that twenty-two children were mostly cured by its administration at the West London Hospital. Notes of several additional cases are given, and it is remarked as that physician's opinion, that "like many other remedies it could not be expected to cure the disease invariably," and as a permanent remedy he had more faith in his plan of treatment of dilute nitric acid and the topical application of nitrate of silver to the larynx. It is not stated, however, in what cases he finds the bromide to be most suitable.

In the observations in the same journal upon Dr. Harley's cases, it is said, "Dr. Gibb discovered in the bromide of ammonium a most valuable pharyngeal and laryngeal anæsthetic, and it is this special character of the remedy which Dr. Harley has endeavoured to turn to practical account in the treatment of pertussis." Whilst Dr. Harley recognises that the benefit is to the hoop alone, there is no indication as to the cases in which he finds the remedy applicable.

It is left, therefore, for each practitioner to determine for himself in what cases the remedy is suitable. Having used the preparation in upwards of twenty cases, if I may be allowed to express an opinion upon this head, it would be,—that the great efficacy of the drug is in uncomplicated cases,—that in those complicated with acute bronchitis or pneumonia, the benefit is so trifling that I prefer other methods of treatment,—that in my experience, infants are more liable to such complications than children above the age of two years. To this I consider it to be due, that in the former the benefit from the remedy is less than in the latter—for an acutely congested condition of the air-passages appears to lessen the effect of the bromide as a laryngeal anæsthetic. That the more frequent the paroxysms of hooping, the more marked and rapid is the relief; that greater relief appears to be experienced in those of some continuance than in recent cases; and, lastly, that when chronic bronchitis is present, the bromide should not be given alone, but combined with squill and ipecacuanha mixture, and occasionally with an emetic.

I have not observed that the bromide of ammonium has any tendency to cause nausea, or that it has any effect on the mental powers. In none of the cases in which I prescribed it have convulsions occurred.—*Edinburgh Medical Journal, June* 1864, *p.* 1096.

26.—ON GAS FUMES IN PERTUSSIS.

M. Guerard has brought under the notice of the Paris Hospital Society the new treatment which has been proposed for pertussis, consisting in the inhalation of the fumes arising during the purification of gas. The pulverized substance, *chiefly composed of* lime and oxide of iron, which has served

for this purification, is spread on the floor of one of the courts of the factory, in order that it may be revivified and re-employed. During this process large quantities of ammonia and volatile oils are disengaged. It was found that children suffering from the chronic stage of pertussis, and who happened to be living in this part of the factory, were so rapidly cured that numbers of others suffering from the affection were sent there, and in many of these marked success attended the experiment. M. Guerard learnt these facts when called in to inspect the factory, in consequence of the neighbours having complained of the nuisance caused them by these fumes. He suggests that the beneficial influence which they seem to have exerted in these paroxysms of pertussis may be analogous to that derived from caustic ammonia in fits of nervous asthma. M. Blache stated that he had met with two cases of children who had been sent to this factory, only with the result of aggravating the paroxysm; and several trials which he had made with some of the residue of gas purification, obtained from the factory, had proved of no avail. M. Bergeron, having knowledge of a case in which rapid amelioration had ensued, sent three other children to the factory, without any benefit being derived. M. Barthez had met with two cases in which the remedy seemed very efficacious, the disease being cured in the one in three, and in the other in four and a half weeks from its outset, therefore in a period of time far shorter than usual. M. Roger stated that he had been called to a child who had contracted pneumonia while employing this remedy, due probably not to gas inspired, but to exposure to the cold current of air of the factory. In two other cases he had found the remedy quite inoperative after a fortnight's trial. Nevertheless, he obtained a large quantity of this residue, liberated it in one of the wards of the Children's Hospital, and daily exposed many children, in whom the disease was on the decline and unattended with fever, to these fumes; but no benefit whatever resulted, and M. Roger is of opinion that at this stage of the disease simple change of air is a far preferable remedy.—*Medical Times and Gazette, August 27*, 1864, *p.* 238.

DISEASES OF THE ORGANS OF DIGESTION.

27.—ON THE DIAGNOSIS OF ABDOMINAL SWELLINGS IN RELATION, CHIEFLY, TO DILATATION OF THE COLON.

By Dr. Henry Kennedy, A. B., Physician to Sir P. Dun's, and the Cork-street Fever Hospitals.

[The present paper is not intended to embrace the wide field of abdominal tumours generally, but only a single affection giving rise to abdominal swelling. The writer especially refers to a means of diagnosis which has not received all the attention it merits, and which frequently affords most valuable aid to diagnosis, viz., the mobility of abdominal tumours as caused by the act of respiration.]

All are aware that some of these tumours can, by the pressure of the hand, be moved about. But it is the movement caused by inspiration of which I speak. The first occasion on which I noticed this occurred some years since, in the lifetime of the late Surgeon Neville. He had asked me to see a man who had a tumour in the epigastric region, of the nature of which he was doubtful. The question lay between a malignant tumour in the stomach and aneurism of the aorta. As the patient lay on his back, I thought I observed the tumour moving; and on closer scrutiny there could be no doubt of the matter, nor that it was due to the act of inspiration; and it at once struck me that it was a means by which a correct diagnosis might be made; for it was obvious that if the tumour were an aneurism no movement of respiration could affect it; but if it were in the stomach this might occur. Acting on this, the diagnosis of a tumour of the stomach was made, and the result confirmed the truth of this idea, for, when the man died, some months later, malignant disease of the stomach was found. It is worth noting, that, *between the time of my* first seeing this man and his death, the tumour less-

ened materially; and this seemed due to the great and, possibly, rapid ulceration which had occured in the diseased mass. Since this case came under my notice I have had many opportunities of confirming the point. It is quite obvious that its value is confined, in a measure, to the upper third of the abdomen, the depression of the diaphragm being its cause; and it can only affect an organ which admits of some mobility, as the stomach and transverse colon. But the opposite of this may also be of use to us to know; for a tumour not affected by the act of inspiration must then belong to one of the parts which are fixed, as, for instance, aneurism of the aorta. Since I observed this sign, it has not fallen to my lot to meet a case of abscess of the liver coming forwards. But there is little doubt that in such a case, and with a swelling perceptible, the act of inspiration would cause the tumour to move; and that if adhesions formed between the tumour and the parietes this sign would then cease—a point of no little consequence to determine, as bearing directly on the treatment. It may be observed, in passing, that the degree to which movement can be given to the parts close to the diaphragm varies considerably with the individual. Those who have the chest long, from above downwards, are able to depress the diaphragm, and necessarily the neighbouring organs, much more than others. This, indeed, is only what we should expect from the varying capacity of the chest in different persons. But it is curious to what an extent it exists; and in exact proportion with this will be the intensity of the sign to which attention is now called.

It is not to be understood, however, that the sign is entirely limited to the upper part of the abdomen. It may be well marked in the middle third, of which I have seen different examples. In one of these, a boy of fifteen, whom I had in Dun's Hospital for some time, a tumour, the size of a small apple, lay on the left side, between the floating ribs and the umbilicus, but nearer the latter. As the boy lay on his back, the tumour was quite visible, and could, of course, be felt; and, on making him inspire, it glided downwards one inch and a-half, and then rose again on expiration. Several gentlemen, including Drs. Law, Hudson, Moore, A. Smith, and Walshe, saw this boy; but, after the closest examination, no diagnosis approaching certainty was arrived at. The spleen was known to be enlarged; and it is a curious fact, and worth noting, that when the boy came into hospital the veins of the abdominal wall were much enlarged, but subsequently lessened so much as to be barely visible. My own impression of the nature of the tumour was, that it was a portion of indurated omentum, very probably the result of strumous disease. I had seen a similar case, some years before, with Dr. Cuthbertson, excepting that the tumour in this last case occupied the right side of the abdomen. Here it was only a *post mortem* examination cleared up the obscurity which hung over the diagnosis during life.

In reference to the distance to which the act of inspiration may affect abdominal tumours, I should say I saw one instance where a tumour which had reached the right iliac fossa was so moved. The case was one of malignant disease, which commenced above—I think in the liver—and grew with extraordinary rapidity till it reached low down on the right side. Here I could feel distinct movement when the patient made a deep inspiration. The explanation of this would, I think, be, that it was the movement of the liver above which was communicated downwards; and that, had the mass grown from below, the great probability is that no effort of the breathing would have affected it. At any rate, the fact that the respiration is capable of affecting movable tumors in the upper and middle thirds of the abdomen, and may even reach lower than this, is, I think, worthy of notice in reference to diseases of the ovaries, where it may be the means of arriving at a diagnosis as to the presence or not of adhesions, which, I should suppose, it would, at times, be of great consequence to know.

The affection to which I would now ask attention is dilatation of the colon —not the simple, and what may be called transient, state, and which it may be supposed exists, more or less, in all cases of constipation. But that state of the bowel in which the *dilatation is* permanent, and from which it is very

doubtful whether recovery ever takes place. The affection is obviously one analogous to what occurs in the other hollow viscera, as the heart, stomach, gall, and urinary bladders; and, though the natural and healthy functions of the colon might seem, as it were, to lead to dilatation, still this state is, in my experience, rare. Neither is its diagnosis as simple as might at first be thought. Indeed, mistakes, I know, may occur; other diseases may be mistaken for this one, or this state of the colon for others. An example or two may not be out of place:—

Case 1.—A girl, of nine years of age, was admitted into hospital, labouring under considerable swelling of the abdomen. On examination a number of masses could be felt through the parietes. They were very movable, conveyed the idea of being so soft as to take the impression of the fingers, and were shaped just like coils of intestine. The general impression was, that the case was one of dilated bowel, filled with fæces. It should be stated that the child had considerable fever. She wasted rapidly; and, as she did so, the tumours became the more marked, and were even visible to the eye. On *post mortem* examination a very considerable portion of the intestine, chiefly the smaller one, was found infiltrated with a lardaceous material, evidently of a malignant nature. It was so placed as to thicken the walls of the intestine, in some parts to the extent of half an inch, and thus lessen its calibre. The material was softish, and retained the impression of the finger. The case is, as far as my experience goes, unique.

Case 2.—A maiden lady, middle aged, and of a spare habit of body, began to get large in the abdomen. It was so very gradual as to be almost imperceptible, and but little inconvenience was felt. There was a tendency to constipation, which, however, yielded to purgatives; but the size of the abdomen was not reduced after their action. The patient died, and the whole colon was found enormously dilated, and full of fæces, the sigmoid flexure alone being four feet long and two feet in circumference. There was no stricture of any kind. This case has been given because the preparation has been preserved, and may be seen in our College of Surgeons; and, in the printed account of it, it is significantly added:—"The nature of the affection was not guessed at during life."

Case 3.—It is now more than six years since I was asked to see a lady on account of swelling of the abdomen. She was unmarried, upwards of fifty years of age, and of a spare habit of body. The lower extremities pitted on pressure, and she could walk but feebly. Though occasionally in the habit of taking aperient pills, there was no marked constipation. The swelling came on so imperceptibly that she could not say when it first began. It was quite obvious it increased at the slowest possible rate. Her appetite was fair; but she was, and had been, subject at times to fits of low spirits. When seen in bed the abdomen was found to be much swollen, and the swelling gave the idea of great solidity; and percussion confirmed this, for there was very little tympany. The veins over the surface were visible, but there was no marked enlargement, neither was there any fluctuation.

My first impression of this case was that the tumour was ovarian. But closer examination led me to doubt this; for I found that the mass was made up of five or six smaller masses; and that some of these could be moved in a very slight degree, not, however, from the act of inspiration, for I believe the abdomen was too full to allow of this, but by pressure from the hand. The greater masses I should say, lay on either side of the umbilicus, the longer axis being from above downwards. It is sufficient to add of this case, that after I had seen it three or four times, and having also in mind the case already given, of which the specimen is in the College of Surgeons, I came to the conclusion that it was one of dilated colon, and subsequent events have, I think, confirmed this opinion. For though the lady is, and, I suspect now, will be a confirmed invalid, she has yet been materially improved by treatment. The abdomen was considerably reduced; and as it became so, the tumours divided, if I may so speak, so that what was a single one this month, *became two* the next. She was able to take considerable walks, and otherwise *could enjoy life.* I only now see her at long intervals, as she is able to

manage herself. But at the present time I believe she is as well as her disease can allow of. The treatment adopted consisted mainly in courses of tonic purgative pills, given for twelve or fifteen days at a time, and then stopped. No medicine in the fluid form either agreed with her, or indeed was of any use. Circumstances prevented me carrying out all the measures which might have been of use in a case of this nature. Her greatest inconvenience, I should state, does not arise from her size, but from the weak state of her limbs, and the inability of walking. She is worse, too, at certain times than at others; and then there is usually considerable pitting of the lower extremities. From first to last there has been nothing of what might be called constipation, but more a sluggish action of the bowels.

Within the last eight months another instance of this affection came under my notice. It occurred in a man of sixty-eight, who came to me under the idea that he was getting dropsical. An examination, however, satisfied me that the swelling, which was dull on percussion, and had no fluctuation, was due to dilated colon; and in about three weeks treatment relieved him, at least for the time. There is every probability, however, that the state will return—the more so when we recollect his time of life. The case, however, calls for no further notice here.

But all cases of this affection do not present themselves to our notice under the same uncomplicated form. Other states and diseases may be superadded; of which the following is a good example:—

Case 5.—A married woman, about thirty years of age, was sent into Sir Patrick Dun's Hospital, by Dr. Leney, of Bray. She laboured under an affection of the heart, and had slight œdema of the lower extremities. She thought herself dropsical, even in the body; and when seen in bed the abdomen was found to be very full; but neither Dr. Aquila Smith, who saw this case, nor I, could detect fluctuation. The examination, however, did detect a number of tumours floating about, as it were, in the abdomen; for they were very movable. None were larger than an orange, and they ranged from that down to the size of a walnut. On inquiry, the patient stated that for upwards of a year the bowels had been getting irregular, but that otherwise she did not feel out of health. When, however, the pitting about the ankles came on, joined to the disease of the heart, as known to Dr. Leney, it was then thought advisable to send her in to Dublin. She was treated under the idea that the swelling of the abdomen was due to an enlarged and sacculated colon; the tumours being collections of fæces, and there being also air in much greater quantity than is usual in this class of cases. At the end of three weeks all the movable tumours had disappeared, and the abdomen had lessened in proportion. Still there remained more of fulness in the hypogastric region than was natural; and though the patient denied all possibility of being pregnant—and this had been asked her on her first admission to hospital—it still turned out that she was so, as proved by the stethoscope. She ultimately left hospital as well as a woman in her state could, for the heart disease was unequivocal.

But cases may be very like the one just given, and yet not be identical. Shortly after this, my friend Dr. M'Clintock showed me a case where two or three tumours, the size of an apple, and very movable, could be felt in the abdomen. The patient was a woman of forty-two, and had been married but two years previously. The tumours lay on either side of the abdomen, and would be referred to the lower part of each kidney. Dr. M'Clintock's opinion was, that the tumours were malignant, whilst my own was that they were due to a sacculated colon; and under this idea she was sent to Sir P. Dun's Hospital, to see what a course of treatment might effect. She remained, however, but a fortnight, being anxious to return to her home. It is enough to say, that she did not get better whilst in hospital, and that, after many months, Dr. M'Clintock's opinion was proved to be correct, for she died of malignant disease, involving nearly the entire abdomen. Of this case I shall only add that, to the present time I am at a loss to say where the disease could have commenced. For I can scarcely imagine that the tumours first discovered were the *ovaries. They seemed to* me to be too movable, and at too great a

distance from the usual sites of these bodies. This is, however, a matter for discussion.

There is one feature in the history of the affection of which I have been speaking which calls for notice here. As a general rule the bowels are sluggish, but not at all to the degree that we might expect. On the contrary, they may act regularly; and this is very likely, indeed, to lead us astray, and calls for the greater caution. So far, however, from there being constipation, or a tendency to it, there may be the very opposite, and diarrhœa may exist. One such instance I have known; it was exhibited by the late Dr. Mayne before our Pathological Society. In this instance the diarrhœa terminated life, and the entire length of colon was enormously distended. There was no ulceration, nor stricture of any kind.

The diagnosis of dilated colon is not by any means the simple matter it might seem; and I believe it to be but common prudence to withhold our opinion till we are able to make successive observations on the case. Some, no doubt, are so very plain that the diagnosis may be made at once; but it may, I think, be stated with certainty, that the great majority are not so. In the case of the lady already given, she was seen, but once only, by the late Sir Henry Marsh, and he told me plainly that he could not make up his mind on the case; and in this I think he showed his great good sense.

On what, then, is the diagnosis to rest? I can only answer this in general terms. In the first place there are usually more tumours to be felt than is common where organic disease exists, whether malignant or otherwise. For though I have given a case where malignant disease, and in a comparatively early stage, existed in two distinct regions, it will still, I think, be allowed that this is exceptional, and therefore not generally to guide us. On the other hand, a single tumour may exist, and yet be due to nothing more than a portion of the colon dilated, and filled with fæces. I have met no unequivocal case of this kind myself, but there are two—if I recollect right—in Bright's able monograph. Still, these are all exceptional cases. As regards the site of the tumours, in cases of dilated colon, something to assist our diagnosis may be gained. Thus they are commonly situated so as to occupy the middle third of the abdomen; and when they occupy, as they often do, both sides of the cavity, there are good grounds for supposing it is a dilated colon with which we have to deal.

But, probably, one of the most characteristic signs of this state is the way in which the tumours are impressed, as it were, with a certain amount of movement—I mean in themselves. This can, of course, only be observed when we can make repeated observations at certain intervals. When this is done it will often be seen that the tumours have altered their position; slightly, it is true, but still so as to leave no doubt on the point. There is every reason to set this down to a very slow peristaltic action of the bowel. With it, too, will occasionally be observed a certain degree of lessening of some of the tumours; and, in the last place, it may occur that the character of the discharges from the bowels, as regards their size, may assist the diagnosis. It might be supposed that this would be of common occurrence. It is quite the reverse; for weeks and months may elapse and yet no characteristic discharges occur.

Such, then, are the signs which may lead us to a correct diagnosis:—The number of tumours—their site—their movements—their alteration in size—and, occasionally, the character of the evacuations.

Into the symptoms caused by this state of the colon it is not my intention to enter here, farther than to say they are, or may be, all those which arise from constipation in general, and in addition, the increased size of the abdomen. The patients are very liable to attacks of pain like colic; and the two extremes of constipation and diarrhœa, of which examples have been already given, may likewise occur. It seems scarcely necessary to add that our treatment must be pointless and ineffective if the state with which we have to contend be not thoroughly understood. Much caution, too, is required as regards our prognosis. With our present experience we have no grounds *for supposing that the disease*, if once fully formed, is curable. But that it

can be alleviated is certain; and the extent to which this may succeed must depend on the circumstances of each case. The age of the patient, duration of the disease, length of intestine involved, temperament, &c., will all have an influence on the result. What, then, is the line of treatment to be adopted? Courses of purgative medicines, suited to the requirements of each case, given at stated intervals, and then omitted for the time, must be our sheet-anchors. It is most important, too, that these medicines should be used with the least injury to the system at large. Hence the value of combining tonics with aperients; and of the former I have found none more efficacious than the preparations of the nux vomica. The common extract of this drug, in union with the compound colocynth pill, or the former with the mass lately introduced in the *British Pharmacopœia*—the compound pill of colocynth with hyosciamus—make good combinations. Every one, however, will have his own favourites. The doses should not be large, but small, and frequently repeated—at least twice a day; and, except under some peculiar circumstances, pills are much superior to the fluid medicines. Injections, too, are at times useful; but it is very strange what little comparative effect they have.

The best directed medicines will, however, cause little good results unless they be combined with other means. The diet should be regulated so as to be the opposite of what is called a dry diet; and with this a system of frictions to the abdomen should be perseveringly carried out; and also bandaging, so as to give a steady support to the parts, should be enjoined. As a means of improving the tone of the muscular coat of the intestines electricity has been recommended, and in suitable cases would, no doubt, be of benefit; as would also all those measures which have a tendency to improve and invigorate the constitution. But these points are so obvious that they do not call for any further notice here.—*Dublin Quarterly Journal, August* 1864, *p.* 3.

28.—ON INFLATION AS A REMEDY IN INTUSSUSCEPTION.

By Dr. David Greig, Dundee.

[In all probability the most frequent cause of intussusception is spasmodic contraction of a portion of the bowel, and relaxation of an adjoining part. It is not by any means a rare occurrence in early life, though probably some cases die without a correct diagnosis having ever been made. Cases are best divided into inflammatory and non-inflammatory. The latter no doubt frequently takes place during life as a temporary derangement, and is met with in cases where there were no symptoms or uneasiness during life. With regard to the true inflammatory intussusception, a very fatal disease, the writer then passes on to observe:]

Every intussusception consists of three parts, the external part or covering, which is formed by that portion of the bowel into which the other has slipped, and the middle and internal, which consist of the invaginated part doubled upon itself. It is these two latter which are acted upon by the first, compressed, constricted, strangulated; and, as a matter of course, inflammation and sloughing ensue. Cases are recorded where inflammatory intussusception has taken place in the small bowel; these are, however, not very frequent. By far the greatest number take place at the ilio-cœcal valve, and are caused by the slipping of the lower portion of the ilium through the valve and into the cœcum. By the peristaltic action of the cœcum and colon, more and more of the small bowel is dragged through the valve, whose spasmodic action will prevent any returning; and when, owing to the mesentery, &c., no more can be dragged through, the cœcum is taken into the colon also, and this is generally the state of matters when such a case is examined after death. The peculiar anatomical characters of this part of the bowel are what we should expect to favour the formation of an intussusception, and to retain one when formed. That the great majority of intussusceptions are into the larger bowel, is a practical *fact well worth bearing* in mind, because an intussuscep-

tion below the ilio-cœcal valve is much more amenable to treatment than one above it. This intussusception generally occurs in healthy children; its presence is shown by a regular series of well-marked and almost unmistakable symptoms. The case, as might be expected, runs a rapid course, and death generally takes place on the third or fourth day.

Much has been written about this serious complaint, but by far the most complete and practical paper on this subject, is one by Mr. Gorham, in the third volume of Guy's Hospital Reports, which I would beg leave to recommend to the perusal of all my medical brethren who wish information on this subject. In that paper Mr. Gorham not only gives a very good description of the disease, but was, I believe, the first in this country who used insufflation as a remedy for it, having taken the hint from some successful cases of the kind recorded in the American Journal of Medical Science. That nature does succeed sometimes in restoring the bowel to its natural condition, there is every reason to believe; but it is not by any means a frequent occurrence. Dr. West well remarks of the cases recorded in medical journals, "I have observed only one instance in which the symptoms of intussusception having existed in a marked degree, at length spontaneously ceased, and were followed by the restoration of the infant to perfect health."

The diagnosis, symptoms, and treatment of such cases will be best illustrated by the narrative of five cases which came under my observation, singularly enough, all within a very short period; in consequence, my attention became specially directed to the nature of this disease.

Case 1.—M. S. G., a stout, healthy, female child, six months old, always enjoyed good health, never having had a day's sickness; never had any food except breast milk; never troubled with diarrhœa or bowel complaint. Was in her usual good health on Monday, 13th October, 1862, up to 6 o'clock in the evening, when without any obvious cause, she suddenly became fretful, kicking with her feet, bending the body backwards, and screaming. In about ten minutes she became very sick and vomited severely. The skin became cold and clammy, the countenance pale, and the lips livid. In a little while she revived, but soon became restless and as sick as before. She seemed to have great pain in the abdomen, which came on in paroxysms, and to increase in intensity until she vomited, when she would seem relieved a little, or at least so faint and sick as not to scream. When given the breast she would take it readily; but as the sickness and vomiting, with a paroxysm of pain, immediately came on, she latterly refused it. Immediately when she was seized a spoonful of castor oil was given, and hot fomentations were applied to the abdomen. The castor oil was soon ejected from the stomach, as was also a small purgative powder which was given. A warm water enema was attempted to be administered, but the bowel seemed to be in such a state of spasm that none could be thrown up. About 8 p. m., tenesmus came on, and she passed a little fluid blood, which continued to come with every paroxysm of pain during the night. The abdomen was soft, slightly tympanitic, and not painful on pressure, except during a paroxysm of pain, when pressure seemed to increase it. On deep pressure being made over the abdomen, under the umbilicus, a very distinct hard tumor was felt a little to the right of the mesial line. Seeing that the case was a serious one, and as the little patient was a near relation of my own, I asked my friend Dr. Pirie to take charge of the case. He attempted to give another enema, but with no better success, owing to the very peculiar spasmodic state in which the bowel was. On the morning of the 14th, as the child was no better, and as Dr. Pirie suspected an intussusception, he requested Dr. Nimmo to see the case along with him. In consultation it was decided that it was a case of intussusception of the bowel. The sickness still continued, but not so severe as on the previous day. The infant took the breast readily, and after taking it would lie still for a few minutes; pain would then seem to come on with sickness, and the milk would be ejected from the stomach with great violence, after which the child would seem exhausted and lie still for some time. It seemed to have great thirst, and took cold water greedily, which, however, was soon ejected. The *pulse was about* 130, small. Injections were again administered, but with the

same result as formerly. As everything had been tried, and nothing had done any good, and as it was evident the child was fast sinking, it was proposed to use the air injection which is mentioned in Dr. West's work on Diseases of Children, as having been used with success by Mr. Gorham. Fortunately, Dr. Nimmo had in his library the volume of Guy's Hospital Reports, which contained Mr. Gorham's paper on the subject, and after each of us had perused that paper, it was resolved to give the remedy a trial, as a last resource, and in truth with no very great hopes of benefit.

The nozzle of a small pair of bellows was introduced into the anus, and air injected to a considerable extent. Contrary to our expectation the air passed readily into the bowel, and seemed to give the child great relief. After the injection it lay very quiet, as if asleep, and evidently quite free from pain. In about twenty minutes from the time the air injection was administered, a slight rumbling noise was heard in the child's abdomen, followed by a crack so loud and distinct as to alarm the attendants in the room, who thought something had burst in the child's bowels. The child, however, continued as if asleep and free from pain, and in about half an hour a large feculent fluid stool, slightly mixed with blood and mucus, was passed without pain. During the night the child rested pretty well, had no return of vomiting, took the breast as usual, and in two days was quite well.

Case 2.—W. C., a fine, healthy, male child, 9 months old, never having had a day's sickness, was taken ill on Friday, 7th August, 1863. He was quite well in the morning, and his bowels were freely moved about 5 a.m. At 12 noon, without any warning, he was suddenly seized with very severe sickness and vomiting; he became deadly pale, and his skin was covered with a cold clammy sweat. In a short time he recovered from this state; he became warm, and his countenance resumed its natural appearance. He was, however, restless, and seemed to have thirst, but immediately on getting a drink of milk he fell into the same faint state, and the milk was ejected from the stomach with violence, the child, before the vomiting, evidently suffering great pain. Even a spoonful of cold water was not retained above a few minutes. The child continued in much the same state until I saw it at 11.30 p.m. At that time it seemed to be suffering from paroxysms of pain, which seemed to come on about every twenty minutes, and the sickness and vomiting continued almost as bad as at first. The skin was warm and moist, the countenance pale, pulse 120, thirst very great, and everything liquid was taken with great eagerness; it even took the breast with as great relish as it did when in health. Immediately after anything was taken into the stomach, it would lie quiet for a few minutes; it would then become restless, and this restlessness would increase until, during one of the paroxysms of pain, everything would be ejected from the stomach; and this was repeated again and again. The abdomen was flaccid and soft; but a distinct, hard swelling, or tumour could be felt under, and to the right of the umbilicus, which seemed to be somewhat tender on pressure. A spoonful of castor oil had been given in the afternoon, but was immediately vomited. The bowels had not been moved, but there was some tenesmus, and before I had been long beside the patient, about a spoonful of slime and florid blood passed from the bowel. I had not the slightest doubt the case was one of intussusception, and proceeded to administer a warm-water enema: but the same peculiar spasmodic state of the bowel, which was mentioned as existing in the former case, was again found here. In order to overcome this, I tried to plug the injection pipe into the anus with lint; but it was of no use, the injection seemed to give the child great pain; very little of the water would go up the rectum, and the little that I did succeed in throwing up, remained a very short time, and only brought away more blood. Nothing came from the bowels but fluid blood and frothy mucus, in which were found shreds of mucous membrane. This state of matters continued until 2 o'clock on Saturday afternoon, when I resolved to try the insufflation of the bowel.

Having got the tube of an ordinary elastic enema fitted on to the pipe of a small pair of bellows, I threw a considerable quantity of air in the rectum. It passed very readily into the bowel, there was no straining or attempt to

expel it, and it seemed to give the child great relief. It had taken some milk from the breast shortly before this, and although the sickness was coming on as usual with a paroxysm of pain, the insufflation seemed to relieve the pain, and the milk was not vomited. I waited for an hour, and as the sickness was again threatening to come on, and fearing I had not thrown in enough air, I administered another insufflation, and continued the process until the belly showed signs of considerable distension, and the child seemed to be somewhat uneasy in consequence. At this time I also administered a teaspoonful of castor oil, and was pleased to see the stomach retain that also; and from this time there was no more vomiting. After this the child fell asleep, and slept for about half an hour, awoke, was inclined to be amused, but still seemed uneasy. Warm fomentations were applied to the belly, which seemed to give relief. From this time the child seemed free from pain, and for the most part slept until about five o'clock, when a copious stool was passed, fluid in consistence, and mixed with some blood. From this time the child was quite well, took the breast with eagerness, retained what it took, bowels became regular, and it soon made up in flesh what it had lost during its short but severe illness.

These I look upon as very good specimen cases of true inflammatory intussusception. Other obscure cases I have met with in my practice, presenting some of the symptoms of this derangement; but with these we have nothing to do at present. As an intussusception of the bowels cannot be seen or handled like a hernia or a fractured limb, many medical men are very sceptical when told that a certain remedy relieved such a case, and can only believe an intussusception to be present by seeing it at a post-mortem examination; this is certainly very conclusive proof, but a kind of proof we would wish to avoid, however conclusive. To my mind the symptoms of an intussusception are unmistakable, and may shortly be said to be, the sudden seizure, the obstinate vomiting, the obstinate constipation, the paroxysms of pain, the hard tumour in the abdomen, and *chiefly* the passage of blood per anum; all these various symptoms may show themselves in other diseases, but when combined together, and especially when the last mentioned symptom, the passing of blood per anum is present, I think no one can have any difficulty in forming a correct diagnosis, and must feel convinced that an intussusception is present, without the necessity of a post-mortem examination.

The prognosis of such cases is always unfavourable; and it is well that the medical man guard himself by stating so, whenever he has made his diagnosis. It is true, numerous cases are on record where nature has produced a cure by the sloughing of the strangulated portion of the intestine, and the junction of the healthy parts. About two years ago, Dr. Hare had an opportunity of showing to the Pathological Society of London how neatly nature completes a cure of this kind, a patient of his having died of tubercular disease only three months after suffering from intussusception, in which several inches of the small bowel came away on the fourteenth day of the attack. At the post-mortem examination it appeared that the portion of the small intestine came away fifteen inches above the caput coli, and so perfect was the cicatrix, that it appeared as a mere line round the bowel, with puckering of the omentum, &c., around it. I would not, however, advise any one to forego treatment and trust to nature for a cure; as from my experience, nature is not to be trusted even when the case appears to be one of no great urgency.

As to treatment, purgatives in the first place naturally suggest themselves; but these are worse than useless, rarely remaining on the stomach, and if they do remain only stimulating the bowel and aggravating the disease. Warm-water enemata are useful, but can seldom be administered owing to the very peculiar irritable spasmodic condition in which the rectum usually is. In any case where this spasmodic condition is not present, or only to a slight degree, I have no doubt warm-water enemata, or, as I used in one case, warm water and air thrown in by a syringe would be useful. The ease, however, with which air is thrown into the spasmodically contracted rectum, *when it is impossible even* to introduce a tea-spoonful of warm water, gives

this agent a pre-eminence over all others, and astonishes all who have seen it used. The remedy is always at hand even in the poorest cottage, no matter how far away from town. Its application is so simple as to require no direction for its use. The only necessity being that enough air be thrown into the bowel to distend it as far up as the neck of the invaginated portion, or, in other words, that the operation be continued until the child begins to be uneasy, and the belly distinctly tympanitic. Amongst other means for relief, cases are on record where the abdomen has been opened for the relief of the bowel in intussusception. It is difficult to reduce one by manipulation after death, and I have no doubt it would be much more so to do it during life.—*Edinburgh Medical Journal, Oct.* 1864, p. 307.

29.—ON HEPATIC DISEASES IN THE EAST INDIES.

By Sir RANALD MARTIN, C. B., Physician to the Council of India.

[In this article Sir Ranald speaks entirely of Hepatitis, which is indeed the disease of the liver of most frequency and importance. In a case of this nature it is well for practical purposes to determine whether the inflammation be confined to the peritoneal covering of the organ, or to the parenchyma, and then to examine the probable extent of the morbid change in progress. It is seldom that hepatitis includes at once either the entire of the capsule or the entire of the parenchyma of the liver, various circumscribed portions of either, or of both together, being more usually affected by the disease.]

The form and character of acute inflammation of the liver as it prevails throughout India differ materially according to the region, the immediate locality, and the climate; the suppurative hepatitis prevalent in Bengal being, in common with the fevers and dysenteries of that great province, by much the most dangerous. In Madras hepatitis is said to be more frequent, while in Bengal it is more fatal. The surgeon will therefore have to consider with care the general climate, that of the particular locality, the age and constitution, the previous habits and course of service, indeed all the hygienic conditions of the soldier, before proceeding to treat him; and in each individual case he must be guided more by a well-weighed con-ideration and comparison of its assembled essential symptoms, than by the presence or absence of any one or two of them. In this disease, as in all others, there is no precise uniformity of symptoms. Where the covering membrane of the liver is the part chiefly inflamed (sero-hepatitis), the sufferings of the patient are sufficiently declared to claim the instant attention of the most cursory or youthful surgeon, the degree of the pain and other symptoms depending on the extent, duration, and violence of the disease. There is always more or less fever, increasing towards evening to severity, preceded by rigor or sense of cold, and followed by a hot skin, and a sharp pain, especially in the hepatic region, on attempting to make a full inspiration, or on lying on the left side. There is often a catching hacking cough, with a tensive apprehensive uneasiness of the right hypochondrium. Sometimes there is pain about the right clavicle and shoulder, and generally an increase of pain on pressing the hepatic region with the expanded hand: these feelings are multiplied when the inflammation is centered in the covering of the upper convex surface of the right lobe of the liver. The symptoms may then extend into the chest; respiration is shallow, short, and is carried on chiefly by the intercostal muscles; while pain and suppressed cough increase along with the fever. It is important here to remark that while even a moderate degree of inflammation of the peritoneum will cause the distresses here described, the most intense and destructive inflammation within may proceed quickly to abscess without any great comparative suffering.

When inflammation is chiefly seated in the covering of the right lobe, or posterior-inferior region, the local symptoms take corresponding directions, the general disturbance remaining much as above described, with the additional distress in the latter instance of disorder of the renal function, as well

as of that of the bladder. Naturally, the train of symptoms will vary as the concave surface of the liver is involved: here the pain will be less acute, and is referred to the epigastric region through to the back; and there will be hiccup, nausea, and vomiting of vitiated bile, with anxiety and urgent distress; the functions of the ducts, duodenum, pylorus, and stomach being much disturbed, with tumidness of these regions; all the secretions are vitiated, and the tongue is very foul.

We come now to the consideration of a more obscure morbid process, but one of vastly greater urgency and danger—namely, the inflammation of the parenchyma of the liver. The deep-seated congestive and suppurative inflammation of the liver, as I have termed that of Bengal, would seem occasionally to be a disease of acute congestion, venous and biliary, more than of pure inflammation; and this morbid association, together with its seat in the parenchyma, will account for its latency and insidiousness in so many instances, and for its enormous dangers. Dr. Copeland says truly that inflammation does not here arise or reach its acme in a few hours, but congestion may; and Dr. Morehead suggests that actual inflammation of the liver is confined to the capillaries of the hepatic artery, and generally to a small portion of them; while congestion, he conceives, is a turgescence of the portal capillaries, and the cause of the augmented size of the liver.

In my work on the Influence of Tropical Climates on European Constitutions, this form of disease is stated to be, in Bengal, by much the most frequent, while its dangers are greater than those of all other forms of hepatic inflammation put together. It is, indeed, one of the most dangerous of diseases, because of its insidiousness, and of the absence of urgent symptoms; the progress which leads to destruction is here silent and rapid.

Whether occurring in the older resident or in the new comer, it arises more generally in the cold season, and is caused immediately by exposure to alternations of heat and cold; such as the chilling thorough-draughts on issuing from a heated ball-room, for instance, or in going into the damp cold of the early morning for purposes of sport. The disease is sometimes preceded by a perceptible falling-off in the general health, with a peculiar impairment in the action of the liver and kidneys, and with functional disorder of all the other abdominal viscera, as indicated by loss of bulk, dry cough, irregular and embarrassed breathing, loss of appetite, and the complexion assuming gradually a muddy sallow hue. But it more generally comes on in the midst of apparent health, the patient being seldom seen until inflammation has commenced, or proceeded for hours or even days. He will then generally complain of abdominal uneasiness, but more particularly of the epigastric region and that of the liver with some degree of fever, preceded by slight rigour or aguishness; but all these may be so slight as too often to attract but little of the patient's attention. Perhaps, after more or less loss of time, he consults his physician on account of slight diarrhœa, supposed to result from errors in diet. Medicine affords relief, and the patient may proceed in his ordinary occupations for days, or, where the action is less acute, for weeks together, though under great depression of the mental and corporeal energies; till at length his altered appearance, night fever and restlessness, hacking cough, permanently dry skin, invincible rough furred tongue, and morbid taste—all expressive of suppressed and depraved secretions,—attract some more serious notice on his own part or that of his family. The real nature of the case, with its accumulated dangers, may still remain a secret to both patient and physician; and it may not be till actual tumour of the liver of unknown duration, and a marked succession of rigours or profuse clammy sweats, announce in unmistakable terms the formation of abscess, that either party becomes awake to the imminence of the danger; and then it is too late.

I should say, then, that a rigour or diarrhœa, followed by feverishness, the peculiar harsh state of the skin, with a muddy or (more rarely) a jaundiced complexion, the tongue having the roughness of a coarse file and an adherent coat, together with the local uneasiness already described, cough, scanty sur-*charged urine, often a* deep saffron colour, ought immediately to warn the

physician of the suppurative inflammation which leads rapidly to abscess. Nor should exploration of the abdomen be less carefully made, even where the case is of far less urgency than that here detailed. Nothing, indeed, should satisfy the medical attendant as to the condition of the patient short of the most painfully assiduous examinations, often repeated; for he has here to deal with a disease deadly in its nature and rapid in its progress, when insufficiently treated, beyond most others—a disease, when once allowed to run to its suppurative stage, more surely fatal than any known in the East.

[Sir Ranald then proceeds to consider the treatment of the disease. As it commonly occurs in Bengal, it proceeds to suppuration in a very short time, hence any treatment to be of use must be prompt and effective. General blood-letting must be carried to the extent of relieving pain, when such is present, or the dull tensive weight and uneasiness complained of in other cases. If this is not rapidly effectual it must be followed by the application of leeches to the side. But blood-letting by leeches alone is not successful in treating the disease. With regard to mercury the writer then continues:]

On my own observation and experience, I would administer calomel, purgatives, sudorifics, and diuretics, after the system has been prepared by blood-letting for their proper actions; all with the view to their general influences, as well as with the view to their power to derive from the organ endangered, and from not feeling justified in withholding any known means to that essential end. Until experiments shall have yielded more definite and determinate results, such is the advice which I would take leave to submit to the consideration of the young medical officer. The actual fate of certain exclusive doctrines and practices within the memory of most of us, and the impending fate of some others, ought to warn us against accepting any proposition, in a case such as the one before us, unless it be of proved advantage, on evidence beyond question. Those who can look behind them, and a little way before them, will be at no loss to balance the comparative values of the past theory and the passing theory. In our profession, as in others, there are subjects which rise and fall—that occasionally come to the front, and then fall away to the rear; and it will continue thus with us until our modes of investigation become more precise.

I believe calomel to be a cholagogue, experiments on the lower animals notwithstanding; and I concur with Dr. Watson in regarding its exclusion from the treatment of acute hepatitis, in Bengal for instance, as unsafe and unwarranted. This great physician, speaking of those who object to the use of mercury in the early stage of hepatitis, "as being stimulating to the liver," says: "I suspect that this is a piece of theory; but at all events, after the first violence of the inflammation that remedy is not to be omitted." On this principle, which I believe to be strictly true, the most eminent men of my time in Bengal acted, and by none of them was mercury, in this or in any other disease, carried beyond its very mildest influences; and when the general habit was reduced to anæmic, mercury was used but once or twice in aid of a purgative. Robert Jackson, who seldom used calomel in treating the diseases of the West Indies, declares, nevertheless, that where the biliary function is morbidly affected there is no other remedy to be compared to it. The relief obtained in certain forms of jaundice from one dose of calomel, after all other remedies had failed, is familiar to every one; and who has not witnessed the astonishing results, in torpor and passive congestions of the liver, from a spontaneous biliary diarrhœa?

[The violence of the acute disease having been subdued the treatment must be cooling and depurant, the diet being of a sparing character, and the patient shielded from cold and currents of wind and confined to bed.]

When vascular turgescence or a state of passive congestion remains, after the subsidence of all the more acute sequelæ to inflammation, the persevering use of the nitro-muriatic acid, externally and internally exhibited, will, according to my observation, go farther and better towards restoring the healthy condition of the liver than any other means; and the effect may be aided, when necessary, by blisters, or by the ointment of tartarized antimony. For restoring the *functions of the liver*, and for promoting healthy secretion gen-

erally, after the turmoil of such an invasion as acute hepatitis, I repeat that I know nothing, whether used in India or at home, so conducive to natural convalescence and cure as the combined acids, employed in the joint manner stated. Mercury is then but seldom required, and its use during the few days of acute morbid action usually brings us to the end of its proper term of employment. Externally I have used acids for many years, both in Bengal and at home, in the form of bath, general and local, and as a swathe placed round the abdomen worn day and night; while, internally, they have been given with fluid taraxacum, chiretta, or simaruba infusion. When eliminants and emulgents are needed in the stage of recovery, the podophyllum combined with henbane, extract of Indian hemp, and ipecacuanha, will prove very effective, used as an occasional means; while the diet must still be carefully regulated, making free only with green vegetables and fruits—otherwise absorption of interstitial deposits will not take place. In this cautious manner only can we secure the patient, whether in hospital or in private practice, against chronic sufferings, functional or organic.—*Lancet, July* 16, 1864, *p.* 64.

30.—CARCINOMA ABOUT THE GALL-DUCTS, CAUSING JAUNDICE.

(Case under the care of Dr. Gull, in Guy's Hospital.)

John G., bricklayer, aged 58, was admitted on the 19th February last, with extreme jaundice, the skin being of a greenish-yellow colour, and he was much wasted. He was of healthy parentage. Twenty-four years ago he had laryngitis, but in other respects had always been healthy. About ten weeks before admission he had to repair a slate roof, and it was raining hard the whole time; he got very wet, and went home feeling great pain and a sense of weight at his right side. In the night he was in such agony that he rolled on the floor, and thought he should die. A week after this his wife noticed that his skin was of a yellow colour all over, and that the "whites of his eyes" were also yellow. From this time he became worse, and sought admission into the hospital. He said that for about four months before his illness he could never drink a glass of ale without nausea or vomiting, and he frequently had diarrhœa. He died on the 25th, six days after admission.

Autopsy, ten hours after death.—The under part of the liver was adherent to the stomach and duodenum, and this again to the colon. These adhesions, together with the thickened tissue around and induration of the pancreas, caused a hard mass to be felt in this region. The infiltration was caused by a tough fibrous tissue, which did not present any other appearance than those of inflammation, and might have been so considered had it not been for the cancer of the liver. When the colon was dissected off there was found a hardened mass of material surrounding the portal vessels, &c., and thus the duct was perfectly closed at its termination in the duodenum. This also somewhat constricted the duodenum itself. The pancreas was excessively hard, suggesting at first a scirrhous cancer of the organ; a section, however, showed it to be hardened by inflammatory fibrous tissue around and amongst it. The new material in this neighbourhood was hard and fibrous, and gave out no juice on pressure. The gall-ducts throughout the liver were much dilated, and filled with watery mucus; the gall-bladder was contracted, and occupied by white inspissated mucus; the liver was of a deep-green colour; the pancreatic duct somewhat distended. At the back part of the liver were several deposits, two of them being the size of beans, and hollowed on the surface as in cancer; they were firm and dry. Near these were several smaller ones, but the region where they existed was circumscribed. There were also several very hard deposits of the same kind on the under surface of the diaphragm. No disease was observed in any other part. The microscope showed the composition of the deposits to be nucleated cells with *abundance of oval nuclei.—Dublin Medical Press, June* 29, 1864, *p.* 686.

DISEASES OF THE URINARY ORGANS.

31.—ON THE PATHOLOGY OF DEPOSITS OF URATES.

By Dr. George Harley, Professor in University College, and Assistant-Physician to University College Hospital.

Deposits of Urates are of a yellow or pink colour, according to the amount and kind of urohæmatin present in the liquid. The higher the colour the more febrile the state of the body. At one time their presence in the urine was regarded as a sign of the crisis of disease, and hence they received the name of critical discharges. Modern investigation has, however, proved that they cannot always be looked upon in this light, as they may also appear under a variety of conditions, which, although abnormal in themselves, are not truly diseased states, according to the usual meaning of the word. Thus, for example, urates may be deposited in the urine after a slight attack of indigestion, the result of over eating or drinking. Great exertion, especially if accompanied by profuse sweating, will also cause them to appear. Hence we find them present in the urine after a fatiguing walk, a long day's hunting, or even after a ball, or other such occasional amusement, especially if it has been associated with much mental excitement. Hard study, and even a fright will in some persons be followed by a deposit of urate of soda. A sudden change in the mode of life is a very common cause of their appearance, as, for example, confinement in town to country people, or a few days' residence in France to an Englishman unaccustomed to French dishes. Under none of these circumstances can the deposit be said to indicate a state of disease. It does nothing more than denote a temporary abnormal condition of system which may soon disappear without treatment. If, however, the deposit, instead of being merely temporary, lasts for some days, that of itself would denote that something more than a mere ephemeral derangement of the system is present, and may deserve immediate attention. A deposit in the urine is always a sign of something being wrong, and although, as we have seen, it may occur from very trivial causes, whenever it takes place without appreciable cause, in the otherwise apparently healthy, it is a sign not to be disregarded, as, under such circumstances, it is not unfrequently either the forerunner or associate of gravel or stone. Uric acid in some form or other is the commonest ingredient of all calculi, and there is no period of life exempt from them.

Urates are a very common deposit in the course of acute disease, and they even seldom fail to recur at some period or other in the course of chronic affections. It is, however, only in diseases of an acute febrile or inflammatory type that their sudden appearance can be regarded as indicative of a crisis. Their sudden appearance is due to an important change having occurred in the condition of the patient, and in general, though not always, it is a change for the better. Such, for example, is observed to occur in cases of gout and rheumatism where the climax has been reached. So also in pneumonia and pleurisy when re-solution and absorption commence.

Should a patient, not labouring under any febrile or inflammatory affection, be every now and then troubled with a pink deposit in the urine without any assignable cause, it will be found, in almost nine cases out of ten, that he is suffering from some chronic affection of the heart, liver, or spleen, with which is associated a tendency to gravel. In all such cases, therefore, steps should immediately be taken to counteract this disposition by the administration of alkaline tonics. Should there, however, be any counter-indication to the correct alkaline treatment, those acid salts are to be employed which, during their passage through the body, are converted into alkaline carbonates—such, for example, as citrates, tartrates, lactates, and acetates. Every now and then an exceptional case may arise, where a mineral acid tonic is demanded; under such circumstances the above rule may be departed from, and the case treated *according to its special* requirements.

It not unfrequently happens that there are crystals of free uric acid scattered among an amorphous deposit of urates. This is more frequently the case in the course of chronic than of acute affections, and generally arises from there being an excess of acid in the urine which has combined with some of the alkaline base and set a portion of the uric acid free.

The crystals of uric acid form on the bottom and sides of the vessel as the urine cools, and, if large they may be readily detected by the naked eye, in consequence of their yellow, red, or brown colour. Uric acid, like urea, when crystallised in urine always takes up a part of the urohæmatine or any other colouring matter that may chance to be present. So that, the paler the urine, the less coloured are the crystals; the darker the urine, the more coloured the crystals. In abnormal urines containing blue, black, or saffron-coloured pigment, the uric acid crystals are blue, black, or yellow; as in these variously-coloured specimens now before you. The crystals are easily recognised by the naked eye if the urine be put into a wine-glass and the deposit allowed to fall to the bottom.

Uric acid is not necessarily in excess when it crystallises spontaneously—though that is the general rule. It has just been said that an excess of free acid induces its crystallisation. I may now add that it is also deposited when from any cause the proportion of alkaline base is abnormally diminished. This arises from the circumstance that uric acid is much less soluble than any of its salts, even the urate of ammonia included.

The daily amount of uric acid excreted varies very considerably in disease. It is generally in excess during the course of all fevers (yellow fever excepted), exanthematous affections, and inflammatory diseases. It has been found doubled in typhus (Parkes), greatly increased in small-pox, as well as considerably augmented in pneumonia. In certain diseases crystals of free uric acid are spontaneously deposited in the urine. This is more particularly the case in hepatic, cardiac, and splenic diseases.

In liver affections there is frequently a spontaneous deposit of free uric acid among the amorphous urates, which, as already said, are so common in those affections. This is more particularly observable in cancer of that organ, in which case, too, the uric acid is generally in excess;—a fact frequently made use of in diagnosis, for in non-malignant hepatic disease, especially towards its latter stages, the uric acid is found to be remarkably diminished.—*Medical Times and Gazette, May* 28, 1864, *p.* 584.

32.—ON URIC ACID.

By Dr. George Harley, Assistant-Physician to University College Hospital.

[Although uric acid calculi are most common in early life they may occur at all ages. After speaking of the relative frequency of uric acid calculus compared to other forms, Dr. Harley passes on to the treatment of uric acid calculus.]

When the calculus is in the urethra or bladder, cutting or crushing, if the condition of the patient admits of it, ought immediately to be had recourse to. But when the calculus is in the ureter or kidney, it is unfortunately beyond the reach of any instrumental interference; and in the majority of such cases, if the stone be large, incurable. Should, however, the case be seen in the early stage, it is in our power, not only to arrest any further deposit, but even, by suitable remedies, to some extent, redissolve what has already formed, more particularly if it be in the shape of sand. When, however, a large solid stone has formed, palliation, not cure, is all that can be hoped for.

The remarks now made are limited entirely to uric acid calculi; for as you will afterwards see, there are some calculi of so soluble a nature as to admit of removal by dissolution even when in an advanced stage of formation.

Various remedies have been resorted to with the view of retaining any excess of uric acid in a soluble state until after its expulsion from the body. *Carbonate of soda* has been much used for this purpose, also the bicarbonate

of potash, and more recently the carbonate of lithia. Each of these remedies is useful, as, indeed, all alkaline carbonates are, with the exception of that of ammonia. The carbonate of ammonia should be avoided in the uric acid diathesis on account of the salt which it forms being much less soluble than any of the others.

In treating cases of uric acid deposit, it is unnecessary to render the urine more than neutral. Indeed, there is a danger in making it alkaline from the circumstance that phosphates have a great tendency to form round uric acid calculi (as is seen in the twelve specimens of various shapes and sizes now on the table), and whenever the urine becomes artificially alkaline, although the deposition of uric acid is interrupted, a deposit of triple phosphates takes its place. The stone, therefore, goes on forming as rapidly as before, the only difference being that its nature is changed.

In cases where the alkaline carbonates are counter-indicated, citrates, tartrates, or acetates may be used in their stead, for the reasons before given. Some have imagined that uric acid gravel only appears in urine of the well-fed and corpulent. This is a great error. It equally occurs in that of the poorly nourished and emaciated, the only difference being that it is more common among the former than among the latter. In this urine, you perceive, there is a copious deposit of uric acid. The bottom of the glass looks as if it had been sprinkled over with a layer of yellow sand; yet this is the urine of a delicate, anæmic lady, with a miserable appetite. In her case, the deposit appears to be hereditary—at least, her father is said to have suffered from gravel.

The most remarkable illustration, however, that I can give of the truth of the above statement is furnished by the sand and gravel in this paper, which was passed by a patient of Mr. Ramsbotham, of Amwell street. The gentleman who passed it was not only out of condition, but was actually on what might be called starving diet. The following is the brief history of the case, kindly furnished to me by Mr. Ramsbotham:—

"An officer in the army, upwards of sixty years of age, who had seen active service in every quarter of the globe, on his return from India by the overland route, deviated from the usual track for the express purpose of restoring his shattered health in the hydropathic establishment of Prisnitz, in Silesia. He remained there some weeks; but failing to have his juvenility restored, he repaired to a rival establishment a few miles distant, in which a directly opposite mode of treatment was adopted. Instead of using water freely, both internally and externally, as in the other establishment, in the latter very little food and no water was allowed, so long as the patient's strength would permit the deprivation, and then, perhaps only at distant periods would a wine-glassful of water be given. The urine passed was scanty in quantity, and, when simply filtered, left on the paper the powder, of which this is a portion."

On analysis, I found the powder and small concretions consisted of uric acid, and urate of soda, with a very small quantity of urate of ammonia.

Enough has now been said regarding uric acid in excess; we shall therefore turn our attention to the equally important fact, that under certain pathological conditions its formation is diminished. We are not yet in a position to speak confidently regarding the nature of all the conditions, but we are sufficiently familiar with the fact that the elimination of uric acid by the urine is diminished in the following diseases, and that it is to some extent under the control of remedies. In yellow (Porcher) and remittent (Parkes) fevers the excretion of uric acid is much decreased. In diabetes it is likewise found to be reduced to a minimum. Lehmann, indeed, thinks that in diabetes it is replaced by hippuric acid; but although I have frequently verified the former, I have as constantly failed to confirm the latter statement of Lehmann's. In the case of saccharine urine from injury to the head, during the first few days there was a total absence of uric acid in the man's urine. In albuminuria I have frequently found the uric acid greatly diminished, and absolutely wanting in two cases. It is said also to be deficient in the urine of *cholera*. *In* chlorosis, anæmia, and hysteria it is like-

wise decreased. The excretion of uric acid is diminished before the outbreak of the paroxysm of gout, but in this case it is known to accumulate in the blood. Dr. Garrod has frequently obtained crystals of uric acid from the serum of the blood of gouty patients. His method is to take from one to two fluid-drachms of the serum of blood, and put it into a flattened glass dish or capsule, about three inches in diameter, and one-third of an inch in depth; to this is added ordinary strong acetic acid, in the proportion of six minims to each fluid-drachm of serum, which usually causes the evolution of a few bubbles of gas. When the fluids are well mixed, is introduced a very fine thread, consisting of from one to three ultimate fibres, about an inch in length, from a piece of unwashed huckaback or other linen fabric, which should be depressed by means of a small rod. The glass is then put aside in a moderately warm place, until the serum is quite set and almost dry; the mantel-piece in a room of the ordinary temperature answers very well, the time varying from twenty-four to forty-eight hours, depending on the warmth and dryness of the atmosphere.

In normal blood the acid occurs in such small quantity that it is scarcely possible to detect it.

Uric acid has also been obtained from the serum of a blister placed on a gouty patient, but not when it has been placed over the goutily inflamed part —(Garrod). Both before and during the attack of gout the uric acid is diminished in the urine, but no sooner does convalescence begin than it rises even beyond the normal standard. From the twenty-four hours' urine of a convalescent patient, who was under Dr. Parkes in the University College Hospital, I obtained no less than 1·15 grammes (17·825 grains) of uric acid —double the amount the man would probably have passed under similar circumstances had the uric acid not been retained in his system before and during the paroxysm.

During the last two years I have had, at intervals of about three or four months, a patient aged sixty-five, under treatment, who has been a martyr to very severe paroxysms of gout for at least twenty years. All the joints of his hands and feet are distorted from deposits, and he has no less than four well-marked concretions of the urate of soda in his right ear. This man's urine has been carefully analysed on several occasions, and the following is a sample of the results. The attack I now allude to began on November 15, 1863, was at its height on the 21st, and had disappeared on December 10.

Twenty-four Hours' Urine.

	November 18, Third Day.	November 21, Climax.	December 7, nearly Convalescent.
Quantity . .	2000 c. c.	1062 c. c.	1620 c. c.
Specific gravity	1011	1010	1008
Uric acid . .	0·095	0·023	0·178

On the third day of the attack he came to the Hospital, which was November 18, and was put upon the following mixture:—℞. Vini colchici, 3ss.; liq. potassæ, ♏ vi.; aquæ, ℥ss., M., ter in die.

It would appear, then, as immediately after the gouty paroxysm has passed the uric acid increases in the urine, the increase of that ingredient in the course of the paroxysm ought to be considered as a favourable symptom.

The gouty deposits commonly called chalk stones, and which are so frequently met with in all cases of long standing, are not chalk at all, but consist of crystals of urate of soda. (Garrod.)

A deposit of urate of soda in the cartilage of the joints precedes or accompanies every attack of gout. Hence arises the necessity of freeing the system of this product. Some persons have thought, that as colchicum—the favourite remedy for gout—instead of increasing the excretion of uric acid, diminishes it, and not it alone, but even the urea, the benefits derived from this agent must, if they are not entirely imaginary, be due to some different *cause than its action upon* uric acid. I, however, believe that colchicum

does act upon the uric acid, but not, as is usually imagined, by merely affecting its excretion. On the contrary, I believe that by some means or other it arrests its formation, and that it is on this account that an occasional dose of colchicum during the intervals is found to ward off the attack of gout.

Dr. Ranke has shown that the formation of uric acid is diminished by quinine, and Dr. G. Pringle, while working in my laboratory, made some experiments upon himself, the results of which are quite confirmatory of Dr. Ranke's views.

January, 1858. ——. Age 24, healthy and well developed.

Twenty-four Hours' Urine.

Diet.	Quantity of urine.	Sp. gr.	Reaction.	Quantity of uric acid.	
	c.c.			grammes.	grains.
Mixed; drink only water, neither tea nor coffee	656	1031	acid	1·89 =	21·545
After taking during five days 10 grains of quinine daily; food and drink as before..	665		"	0·724 =	11·222
The same after seven days of quinine treatment	602		"	0·577 =	8·945
The effect of the quinine did not pass off immediately.					
Diet as before; quinine discontinued for two days	600	1036	"	0·66 =	10·230
Do., do., do., three days	525	1033	"	0·792 =	11·276

Quinine combined with colchicum was Bequerel's favourite prescription for gout. Curiously enough, too, he used to combine it with digitalis, another substance which has the effect of diminishing the uric acid. A good formula is—

℞. Ext. colchici, gr. x.; digitalini, gr. ½; quinæ sulphatis, ʒj.; conf. rosæ, q. s. Fiat massa et divide in pilulas, xij., sumat unam ter in die.

While in Munich, a few years ago, when talking to Professor Pfeuffer regarding his treatment of gout, he told me that he found the acetate of potash a good remedy, and it so happens that this also has the effect of diminishing the amount of urea and uric acid in the urine; and, as I said with reference to colchicum, I believe it is not simply the elimination of these substances which is arrested, but their actual formation that is checked.

I have already pointed out how animal diet increases uric acid. I may mention that there are certain other remedies besides those already spoken of, which diminish the formation of uric acid; these are atropine and cod-liver oil. Tea and coffee produce the same effect. On the other hand, there are some which, although they do not diminish its formation, yet lessen the quantity in the system by increasing its elimination from the body. Among these may be mentioned the phosphate of soda, and the liquor and bicarbonate of potash. Both in the urine of gout and of rheumatism the uric acid has been found increased after the administration of liquor potassæ, as well as after a few doses of the simple carbonate of potash, and it is not at all improbable that the beneficial action of these remedies in the above-named diseases may in a measure be due to their hastening the neutralisation and elimination of the acid. Indeed, were I asked to give an idea of the pathology of gout and rheumatism, I would say that all our knowledge as yet tends in one direction—namely, to show that both gout and rheumatism are due to the presence of an abnormal amount of acid in the system; most probably uric acid in the one case, lactic acid in the other. I have repeated Richardson's experiments of injecting lactic acid under the skin of dogs, and although not always successful, have on three or four occasions produced artificial articular rheumatism. On one occasion I even got the endocarditis he has so ably described.

Uric acid is not only eliminated by the kidneys, but by the skin, and even by the lungs; so that, by increasing the action of the skin and pulmonary exhalation, we may aid its elimination from the blood. Experience has shown that whatever interferes with cutaneous transpiration augments the quantity of uric acid in *the urine*. *The effect of* cold has been already alluded to.

Skin diseases have a similar effect. In eczema and psoriasis, uric acid increases in the urine, and in the pustules of pemphigus crystals of it have been detected (Malmeston). In the urine of a severe case of pemphigus, under the care of Dr. Hughlings Jackson, which he sent to me to examine, I found 17 grains of uric acid to the 32 oz. The urine was of a dirty brown colour, with a very acid reaction, and had a specific gravity of 1023.

With regard to the influence of alcoholic drinks, beer included, it is interesting to observe how they lessen the urea and increase the uric acid in the urine; whereas, with tea and coffee, the reverse occurs; they lessen the uric acid and increase the urea. Then, again, there are substances which act similarly on both uric acid and urea. The acetate of potash, colchicum, quinine, and cod-liver oil, for example, diminish, while bicarbonate of potash and liquor potassæ increase both.

These facts furnish us with several hints which may be turned to good account in practice. Remedies we possess in abundance if we only knew how to apply them.

When I hear men saying that the medicines they are employing are little better than useless, it often occurs to me that they are, perhaps, not far wrong, although the want of success may not be, as they "vainly" imagine, due to the impotence of remedies, but to the want of knowledge and judgment brought to bear on the employment of them. A man needs only to pay a little attention to the teachings of physiology to learn what powerful weapons nature has put into his hands. Like all weapons, however, they may be turned to good or evil account, according to the knowledge or the inclination of their employer. I would even venture to suggest to our sceptical brethren that it would, perhaps, be wiser if, before passing judgment on any particular remedy, they first made themselves familiar with the how, when, and why to employ it.—*Medical Times and Gazette, June* 4, 1864, *p.* 609.

33.—ON THE COLOURING MATTERS OF THE URINE.

By Dr. George Harley, Assistant Physician to University College Hospital.

The colour of the urine varies greatly in disease. It may be perfectly white, yellow, brown, red, black, green, or blue, and each of these tints, in the absence of ingesta capable of accidentally producing them, invariably indicates the existence of grave disease.

Normally coloured urine does not, however, exclude the possibility of disease, for the colour of the freshly passed urine is no absolute criterion either of the quantity or the kind of colouring matter it contains. The abnormal like the normal pigments are often combined with some of the other urinary ingredients in the form of colourless compounds, and it is not until the compound is decomposed, and the pigment set free, that we can take cognisance either of its quantity or quality. For example, here are three urines:—1st. A pale, almost colourless urine from a healthy infant, aged eighteen months. It has a specific gravity of 1018. 2nd. An equally pale, almost colourless urine, from a girl, aged nineteen, suffering from chlorosis. It has also a specific gravity of 1018. 3rd. A dark straw-coloured, but perfectly transparent urine from a healthy man, aged thirty-three. It has likewise a specific gravity of 1018. In fact, these three urines have been purposely selected on account of their having the same specific gravity. To each of these add a quarter of their bulk of strong nitric acid, and bring them to the boiling point. Watch the change. The infant's pale urine is scarcely altered; the man's dark urine is only slightly deepened in tint; whereas the almost colourless urine of the chlorotic girl has assumed an intensely red hue. What is the cause of this difference? The infant is in the bloom of health—there is no waste of blood corpuscles in it—all the blood discs it possesses are employed in the development of its frame. The man has arrived at maturity; he is still in the prime of life, and in the enjoyment of perfect health; his blood *corpuscles are not* wasted, but merely consumed in the tear and wear of

every-day life. The young woman, on the other hand, is suffering from chlorosis; she has a pale lip, and a blanched cheek; her corpuscles are being too rapidly consumed; her life's blood is oozing away by the kidneys, and there it appears as an excess of urohæmatin in her urine.

Take again these two urines so different in appearance. They are from young men about the same age (twenty-four years). The one urine is perfectly colourless, like water;* the other is of a deep red colour—case of hæmaturia from disease of the kidney. On adding strong hydrochloric acid to the colourless urine it rapidly assumes a port-wine tint, whereas the same amount of acid added to the red urine, instead of heightening, actually destroys the colour it already possesses. And why? simply because the pale urine contains an excess of combined urohæmatin, which is liberated by the acid; whereas, the red urine contains merely a number of free blood corpuscles, which become coagulated, and as the colouring matter in them is insignificant in quantity when compared with the amount of urohæmatin in the other, no sooner are their cell-walls destroyed and the contained hæmatoglobulin set free and coagulated, than the red colour disappears. Now, which of these two classes of urine denotes the most danger? Most assuredly, not that containing the free blood-cells. A very small quantity of blood will sometimes colour a great deal of urine; whereas an immense destruction of blood corpuscles may take place in the body, and their *débris* be so eliminated as to be invisible to the eye until the application of an acid sets it free. In fact, experience has shown me that the normally coloured urine of disease is a most treacherous guide to go by. It often lulls the inexperienced into the belief that there is nothing materially wrong, when a grave lesion is making rapid strides towards a fatal termination. Not very long ago I was told by an intelligent Practitioner, that a young lady, regarding whose health we were consulting, was laboring under hysteria. "The secretions," as he termed them, being "all right," my opinion had been asked more with the view of satisfying the friends who were getting fidgety than anything else. The case certainly appeared to be exactly what he said, until, in the course of conversation he added, "she is well fed, and yet she loses flesh, and I don't know why." This remark at once brought us back to the urine, which he assured me was perfectly natural in colour, and contained neither sugar nor albumen. I had some sent to me, and in the end, the case turned out to be the very counterpart of the lad's just spoken of. Although the girl's urine was natural in colour, her life's blood was imperceptibly oozing away with it. This is not a solitary example; I might cite several more to prove that many of the cases called hysteria are in reality cases of serious, though obscure, disease; but that is surely unnecessary, for who amongst us has not seen patients die, and yet their disease put down as, only hysteria? The time is, nevertheless, not far distant when we shall learn that hysteria is something more than "mere functional derangement." Hysteria may be a convenient term; but, after all, it is only a cloak of ample dimensions which hides the rags of ignorance. It is a name instead of an explanation, a sham instead of a reality. And what is still worse is, that it tends to keep us in ignorance by stifling legitimate inquiry; for no sooner do the majority of us find a name for the disease than we cease to fathom its symptoms. In some of those cases of obscure disease the excess of urohæmatin in the urine is so great that after it has been set free by an acid and taken up with ether, the ether, after standing, solidifies into a red-current jelly like mass, and may actually, in some cases, be cut with a knife.

The best way of showing this is to boil four ounces of urine along with some nitric acid in order to set all the colouring matter free. When cool, put the urine into a six-ounce bottle along with an ounce of ether, Cork the bottle, thoroughly shake it, and afterwards place it aside for twenty-four hours. At the end of that time the ounce of ether will sometimes be found

* The patient from whom this urine came was sent into the Hospital (under the care of one of my colleagues) from his being supposed to have stone. None, however, was detected, and his symptoms appeared to arise from excessive spinal irritation. At the time the above sample of urine was passed the lad had already been six weeks *in the Hospital*.

to be a red tremulous jelly. Of course such a case is a very bad one; but these are, nevertheless, not nearly so uncommon as one who has not worked at the subject would imagine. The urine in the worst form is neutral or even alkaline.

Sometimes in disease a great part of the urohæmatin exists in the urine in a free state; and in that case the urine is red in colour before any acid is added, although the addition of the acid makes it still darker. These cases are quite different from hæmaturia; for in them the urine is clear and transparent, and devoid of blood corpuscles. If it contains a deposit, the deposit may or may not be high coloured; but in any case the supernatant liquid is clear as well as red. Another fact, which is of great clinical importance, is that the urohæmatin is not always in the same state of oxidation; and, like indigo, its colour depends on the amount of oxygen it contains. So that it may be pale yellow at one time, red at another, and brown at a third. In consequence of this, different acids act upon the urine differently. In one case we may find that the addition of nitric, sulphuric, or hydrochloric acid gives rise to exactly the same results; whereas in another case hydrochloric acid may turn the urine red, while nitric acid only causes it to become yellow. In a third case sulphuric acid may develop the colour of the urohæmatin better than either of the others. Here is the pale urine of a case of chlorosis, and you will observe that both hydrochloric and sulphuric acids turn it red; yet nitric acid merely changes it to a yellow.

[August 3, 1864.—To-day, while arranging these sheets for the press, I examined a urine which so well illustrates the truth of the foregoing remarks that I cannot refrain from briefly noticing it. It is that of a gentleman thirty-three years of age. To the eye it appears perfectly normal in colour (of a dark straw tint), the specific gravity being 1030, and the quantity passed about thirty ounces. On adding strong nitric acid to this urine it immediately became of a blood-red hue, whereas hydrochloric had no effect upon it until some minutes had elapsed, when it gradually caused it to assume the same tint as that produced by the nitric acid. When sulphuric acid is slowly added so as to fall to the bottom of the test-tube, a reddish-brown line appears at the point of contact, and this gradually deepens until, in the course of six hours or so, it assumes a more intense colour than that produced by either of the other acids.

This is an important case in another point of view—namely, as proving the value of urinary analysis in cases of obscure disease. The only symptoms that this patient labours under are those of an irregular kind of dyspepsia, with an occasional pain in the epigastrium. He is in easy circumstances, takes plenty of nourishing food, wants for nothing, yet he gradually gets weaker and weaker, and has lost 17lbs. in the course of the last year. Even the locality in which the patient lives is a remarkably healthy one (he was sent to me from Anglesey by Dr. D. Williams, of the Menai Bridge), and if it had not been for the condition of the urine, his symptoms would have remained a mystery. This, too, was one of those cases in which the amount of urohæmatin was so great that it caused the ethereal solution to solidify into a jelly.]

An excessive excretion of urohæmatin is not limited to cases such as we have been describing. It occurs to some extent in several diseases, especially those in which there is an excessive tissue metamorphosis, and consequent too rapid blood consumption. Hence we occasionally meet with it in low fevers, in diphtheria, in pneumonia, and some other inflammatory affections, in lesions of the nervous system, during an attack of gout, after the fit of ague, and during convalescence from nearly all grave diseases. It is, however, in chlorosis (either in the male or female), and the many unnameable obscure affections of that class where it becomes a dangerous symptom. In fact, it always indicates the existence of a past or present mischief meriting the closest attention of the Physician, and where we cannot remove the cause we must at least attempt to check the effects of the symptom—namely, to restore to the blood as much as possible of the material which is being drained *from it. Every one knows* the effects of iron upon the system, but iron alone

is not always sufficient for our purpose. Something more is wanted, and that is best supplied by the preparations called the syrups of phosphate of iron. Many syrups have been sold under this title. There are the American syrups: the compound syrup of the phosphate of iron, or "chemical food," as it is sometimes named; the syrup of the superphosphate of iron and lime (excellent for children); and a few others. One and all of them are good in particular cases, but they must always be associated with a judicious selection of regimen in order that the full benefit may be derived from them.

The preparation of zinc, in grain or grain and a half doses, are also occasionally useful astringent tonics when the drain is very great.

In some cases of disease the excretion of urohæmatin by the kidneys appears to be diminished; but this is only when the system has been so drained that there is little more to come away. In the last stage of chlorosis a great diminution in the amount of the urohæmatin in the urine takes place; so also in chronic cases of hæmaturia, notwithstanding that the urine perhaps looks red. Likewise in cases of chylous urine, in the albuminuria of pregnancy, and in chronic Bright's disease. In all these cases the blood has already been well drained of its constituents before a marked diminution in the amount of the eliminated urohæmatin takes place.—*Medical Times and Gazette, Sept.* 10, 1864, *p.* 271.

34.—CASE OF HYDATID DISEASE (ECHINOCOCCUS HOMINIS) AFFECTING THE KIDNEYS.

By Dr. A. Leith Adams, Surgeon, 22nd Regiment.

Private P., aged thirty-five; countenance sallow and climate-worn; served in India for eleven years. He stated that shortly after his arrival in England from India, in 1855, he began to experience dull aching pains in the loins, especially over the left kidney; and that he had been annoyed by these pains frequently since, but more so during the last three years he had been quartered in Malta. In March, 1862, the pains increased in severity, so that on the 2nd of April he came to the hospital complaining of intense pain in the lumbar region shooting down the ureters into the bladder, but more especially on the left side. It became so intense as to oblige him to walk in a bent position, and lie in bed with his legs drawn up. He complained likewise of a pricking and uneasy sensation along the course of the urethra. His urine was scanty, and the skin hot and feverish. From the intensity of his sufferings it was surmised that one or more renal calculi were making their way into the bladder. Neither the warm bath, fomentations, nor opiates seemed to relieve him, until the following morning he got up to void urine, but finding some obstruction in the canal he was on the point of sending for aid, when suddenly, after a little straining, he expelled a gelatinous mass, which turned out to be true echinococci cysts. A few were entire, and of the size of a hemp-seed; but the larger individuals were much torn. He voided a few more about an hour afterwards, when the symptoms disappeared, and in the course of a few days he returned to duty apparently cured. For the following four months he continued free from all of the above symptoms, and enjoyed a degree of health he had not experienced for many years, when suddenly, on the 4th of August he was seized with darting pains in the urethra, and a desire to micturate. After making violent efforts for a few minutes, a large torn cyst came away with a copious discharge of urine, which was slightly acid, and contained abundance of epithelial cells. Instant relief followed. On the 18th of the same month he passed one large and several small echinococci, preceded by exactly the same symptoms, and followed by the same result. Again, on the 26th, after a sudden seizure in barracks, he voided a colony of cysts, varying in size from one as large as a pigeon's egg to many entire specimens of microscopic dimensions. As usual, complete relief was the result. On the 25th of September, after an interval of ease, his sufferings returned as before, and lasted

for several hours, during which he kept passing large quantities of cysts. The largest individual was much torn, but had evidently equalled a hen's egg in size. On this last occasion he voided more than at any former period. The result was that for eight months he seemed to have got completely rid of the parasite, and his general appearance betokened a decided improvement in health. Suddenly, however, on the 14th of May, 1863, the lumbar pains shooting along the ureters and urethra set in, and he voided a few cysts; and scarcely a week passed that he did not get rid of what seemed entire colonies. Racking pains in his back and limbs, accompanied with constant thirst, loss of appetite, and increasing debility followed, so as to render him perfectly unfit for military duty. Accordingly he was invalided, and sent to England in the following October. Since then he has been discharged from the service.

Remarks.—The microscopic examination of the cysts showed the double circlet of hooks, varying in number (as many as forty were counted on one crown); few seemed complete, having lost many of their hooks either by decomposition or the rough usage sustained during their passage outwards. Occasional hooks were found detached or lying on the surface of the cysts. Each hook was slender, tapering, and acute. Two cell-walls were distinctly visible, and appeared to be perfectly structureless and transparent until treated with caustic potash and red ink, when they presented a stratified and wavy appearance. The four suckers mentioned by authors could not be made out even by a lens magnifying to 500 diameters. The circlets of the large vesicles were not visible, possibly from having been removed by friction; there was no difficulty, however, in finding them in individuals as large as a hemp-seed, together with the calcareous corpuscles which were abundantly strewed over the surfaces of the coats. The uniform size of the corpuscles, together with the number and configuration of the hooks, at once distinguish the above from the acephalocyst. From the injuries sustained by the vesicles, the pedunculated buds from which the armed cysts arise could not be made out. Whatever may be the specific distinctions between the two forms of echinococci described by Küchenmeister, and denied by Leuckart, Cobbold, and others, according to the diagnosis of the first-named helminthologist, the individual in question agrees with his description of E. altriciparíens seu E. hominis of authors.

The symptoms were in general characteristic of the separation and passage of the cysts, especially when the pain was intense; but latterly this was not always the case, and it is more than probable he may have voided them unconsciously long before and after they were first discovered. The continued absence of any internal swelling either over the kidneys or bladder, and the non-appearance of hyatid trembling (*frémissement*) on percussion, would seem to indicate that the collection was never extensive enough to give rise to these symptoms. The lumbar pains being generally most extensive over the left kidney and ureter led me to suppose that the parasite existed, if not altogether, at least in the greatest abundance in that organ. From the history of the case, there is a probability that the patient contracted the disease whilst in India. According to his own confession, and my experience of his state of health for nearly fourteen years, I have been unable to find out that he had been ever affected by tape-worm, although many of his comrades have been frequently infested by the tænia mediocanellata, which I apprehend is the most common form amongst soldiers. At least, of many heads examined by me, I have not been able to discover any hooks. Moreover, the obstinacy wherewith this parasite almost invariably adheres to the intestine, in spite of a carefully conducted treatment and the most powerful anthelmintics, together with the anatomical characteristics of the matured segment, leave little doubt as to its being a distinct species from tænia solium. I believe, however, the last-named cestode is also to be met with amongst soldiers, more especially where pork forms a portion of their ration, or at stations such as India, where inferior qualities of animal *food* are sold in the native bazaars; but as beef is the most general form of *animal food on which* the soldier subsists, and seeing that the cysticerus of

tænia mediocanellata infests oxen, the circumstantial evidence tends towards the supposition that the germ is introduced into the alimentary canal by eating their raw or underdone flesh. Out of 103 carefully recorded cases, I find that 55 men had been frequently in the habit of eating raw portions of beef, mutton, pork, or bacon; whilst 48 stated that they positively abhorred the idea. However, many of the latter preferred their meat ration much underdone. It would seem, moreover, from inquiries I have made, that it is no uncommon practice for soldiers and the lower class of English labourers to eat raw meat. Although I have failed in being able to discover cysticerci in either the beef or salt pork supplied to the troops in Malta, there is no lack of "measled pork" in the butchers' stalls in the villages. I lately saw a pig's liver at a stall in a village of Goho charged with cysticerci. The owner, however, soon removed it when he saw it had attracted my attention. At all events, with reference to soldiers, there is no doubt that tape-worm is very common, more so than should be the case amongst a body of men so well fed and cared for. The mode of cooking in the army has, moreover, been defective; but now a system is being introduced which must effectually prevent the introduction of cysticerci by badly-cooked animal food.—*Lancet, October* 1, 1864, *p.* 375.

35.—ON A NEW REMEDY IN THE TREATMENT OF CERTAIN FORMS OF DROPSY.

By Dr. W. Abbotts Smith, Physician to the Metropolitan Free Hospital, and to the Finsbury Dispensary.

In March, 1863, I brought under the notice of the Medical Society of London a remedial agent, which, in consequence of its diuretic and local tonic properties, I had found very efficacious in the treatment of certain forms of dropsy, and as I have since had opportunities of observing its medicinal value, I venture to make it the subject of a few remarks.

The remedy to which I refer is the *Erodium cicutarium*, or common stork's-bill, an indigenous plant which belongs to the natural order Geraniaceæ, and grows abundantly in sandy situations near the sea-side. My attention was first drawn to it by some observations made in the "Medical Times," by Mr. Byerley, F. L. S., of Seacombe, in Cheshire, who stated that it had been productive of great benefit in a case of dropsy which had come under his treatment.

The first case in which I prescribed the erodium was that of a man, of about forty-five years of age, who had been a patient of mine at the Metropolitan Free Hospital, for a period of two months, owing to his suffering from renal disease, complicated with anasarca, and subsequently with ascites. During the time that he had attended as an out patient he had taken squills in small doses, digitalis, scoparium, and many of the diuretics ordinarily given, without any permanent good results. The digitalis appeared for a short time to keep the effusion in check, but it soon lost that power. I had also administered elaterium, the *pulvis jalapæ compositus* of the London pharmacopœia, and other drastic purgatives, but as they certainly weakened the patient without being productive of any adequate degree of benefit in the reduction of the dropsical effusion, their use was abandoned. At this crisis I commenced the administration of the decoction of erodium, in three ounce doses, four times a day, and in order that I might be enabled to form an impartial opinion of this remedy, and also that I might avoid the fallacy of arriving at a *post hoc, ergo propter hoc* conclusion, I ordered the discontinuance of all previous prescriptions. Upon the patient's next visit to the Hospital I had the satisfaction of learning that the swelling of the legs had considerably diminished, and that the abdomen was smaller, as was shown by the comparison of a measurement, made by means of a tape passed round the body, at a point about an inch below the umbilicus, with the dimensions noted upon the occasion of the patient's last visit. The flow of urine was stated to have been very copious. The medicine was continued for three weeks longer, after which *time the patient* was placed upon a short course of

tonics. At the end of November, 1862, he was discharged cured, and since that date he has been able to follow his usual out-door avocation,—that of a cooper.

Another case in which I tried the erodium was of an equally unfavourable character. The patient, a man of about sixty years of age, had suffered from repeated attacks of ascitic effusion, consequent upon enlargement of the liver, caused by excesses in drinking spirituous liquors. The decoction of erodium was given for a fortnight, in four-ounce doses, three times daily, unaided by any other medicine than an occasional compound colocynth pill, for the purpose of keeping the bowels open. At the expiration of a fortnight the abdominal effusion had nearly disappeared, and the patient was subsequently placed upon a course of alterative and tonic medicines, with a more nutritious diet, which soon completed the cure.

In some other cases of a similar nature I have found the erodium valuable. The form in which I have generally employed this remedy is that of decoction, which is best made by placing two ounces of the dried plant in three pints of boiling water, which should be allowed to simmer until the quantity of fluid is reduced to two pints; the remaining liquid should then be poured off and strained, so as to render it fit for use. An extract has been prepared by Messrs. Clay and Abraham, of Liverpool, but my experience of that preparation is too limited to allow of my speaking decisively of its merits.

I do not, of course, advocate the substitution of erodium for all other remedies used in the treatment of dropsy, as this disease depends upon so many different causes that it would be absurd to suppose that a specific could be discovered for every form of dropsy. I believe, however, that it is often worthy of a trial, and that it will be found a serviceable adjunct to other plans of treatment, especially in the large class of cases of dropsy in which, although we may feel that diuretics would greatly facilitate the cure, we yet hesitate to resort to their use, owing to the serious complications which not unfrequently follow the administration of those commonly employed, through their excessively stimulating action upon the kidneys.—*Medical Mirror, Sept.* 1864, *p.* 530.

SURGERY.

DISEASES OF THE BONES AND JOINTS, ETC.

36.—ON EXCISION OF THE KNEE.

By W. Fergusson, Esq., F. R. S., Surgeon to King's College Hospital, and Surgeon Extraordinary to H. M. the Queen.

It was a great honour done by Mr. Park the surgeon of the Liverpool Hospital to Percival Pott, of St. Bartholomew's, when, in September, 1782, he wrote "a few sheets, in which," to use his own language, "I hope to show that in some of the affections of the knee and elbow, in which amputation has hitherto been deemed indispensably necessary, surgery has yet another resource, which, as far as my reading and experience enable me to judge, has not yet been attempted by any other practitioner.....the resource I mean is the *total extirpation of the articulation*." Park had actually underlined the latter words, thereby, as it were, upholding impressively the originality of the expression.

Four-fifths of a century have passed since Park thus wrote, and the latest echo to this grand announcement is thus recorded in 1861 by one of the present surgeons and lecturers on surgery: "The excision of joints does not find much favour at St. Bartholomew's. It has been practised only once, and that in the elbow." This refers to the year 1860.

But there are many excuses for the great Percival Pott. If he, as a leading star, took no action on such an announcement, he in this respect but resembled his fellow-mortals. Not a breath, not a pen, not a knife, stirred in England on the subject. Even Moreau the elder, although in a manner inspired by our English surgeon, failed to impress upon his immediate contemporaries any possible value of the "extirpation of an articulation." The modest effort from Bar-sur-Ornain was as little heeded as that from the banks of the Mersey. Although Park and Moreau had shown that which had not previously been dreamt of, each passed to his grave with hardly even a recognition by their contemporaries of the great things they had done. Like many poets and painters, their greatest fame has been posthumous, and my own impression is that even to the present day the value of their labours has not been thoroughly recognised.

It is a remarkable fact that, in referring to excision of the knee and elbow, Park invariably put the operation on the lower extremity foremost. This, doubtless, was because he had operated on the knee first in the living body. Although he suggested both operations, it was reserved for the elder Moreau to have the honour of first performing that on the elbow. This now famous operation was done 26th June, 1797, fifteen years after the date of Park's announcement. The ingenious Frenchman gives Park his full credit, and in that respect differs from some of our own countrymen, who have evinced a disposition to award the honour regarding the early start of excision of the knee to Filkin, of Northwich, who, as stated by his son some twenty years after, performed this operation successfully in 1762. But Filkin's case, like that of Justamond's partial resection of the elbow, had no influence on Park's original conceptions. The knowledge of these cases came to him at second-hand afterwards, and he it was, in fact, who first gave them notoriety when publishing his second case of excision of the knee in 1789. Had it not been for Park himself these cases would probably have been shrouded in everlasting obscurity. They *no more detract* from the merit of the Liverpool surgeon

than do the occasional excisions of the elbow (said to have been practised in the midland districts of England since his time) from the just credit due to Mr. Syme, of Edinburgh, for the revival of the operation.

My present time will not permit much further historical reference. Suffice it to say, that a profound slumber closed upon the efforts of Park and the Moreaus until within our own period. A few spasmodic efforts seem to have been made from time to time, and among our own countrymen none were so remarkable as those of Hewson, Crampton, and Syme. The first two were comparatively apathetic on the subject, and the proceeding was discouraged by the last, who some thirty years ago operated on two cases, from which experience he expressed himself unfavorable to the operation. Nothing, save the usual discouraging historical notice, was said of it for about twenty years, whilst during that time excision of the elbow became an established operation. Often and often had I myself felt deeply grieved to see a well-made foot, totally free from disease, and a leg on which the pathologist would scarcely glance, swept away by amputation in the thigh for disease in the knee. Often did I think of the casuistical expression of those who know no better, that "amputation is the opprobrium of surgery!" Why, I thought, should excision of the knee in incurable disease of the joint not do as much in proportion as excision of the elbow in a similar condition? What but bulk, or comparative magnitude, constituted the great difference between the two operations? If excision of the elbow saved a useful hand and forearm, why should not excision of the knee be equally useful in saving the foot and leg? The comparative size of the limb and joint seemed the grand objection; but here it became a question whether excision of the knee or amputation in the thigh might prove most or least hazardous to life. These latter points seemed to me those of the greatest interest in regard to this proceeding; for whilst we had indifferent results as respects the condition of the limb after both operations, we had the recorded experience of Park regarding his first case, in referring to it six or seven years after, that the patient had "made several voyages to sea, in which he was able to go aloft with considerable agility, and to perform all the duties of a seaman; that he was twice shipwrecked, and suffered great hardships, without feeling any further complaint in that limb."

It seemed to me that this great question had never yet been investigated. One or two cases here and there did not appear to me sufficient to settle such an important matter.

In July, 1850, I performed the operation on a patient in King's College Hospital in whom all the circumstances seemed most favourable. Whilst there was painful disease, of which the patient was tired, and for which he willingly consented to amputation, there was youth, and apparent health elsewhere. The disease seemed limited to the cartilages, and I could not imagine a more favourable instance for the object I had in view. Yet nothing could have been more disastrous than the result of this operation. The patient died, after sufferings equal to any I had seen follow great operations. In particular, the starting of the limb appeared equal to, if not more troublesome, than any I had ever witnessed in compound fractures of the lower limb or after amputation. A post-mortem examination showed that acute necrosis of the lower end of the femur had ensued.

I confess that I was in some degree daunted by this case; yet as I had seen as much local mischief and also death occur in equally promising cases after amputation in the thigh, I resolved to suspend my judgment. Not long afterwards, the late Mr. Jones of Jersey, essayed the operation with more satisfactory results; and as time rolled on, his highly successful repetitions, as well as the operations of others, attracted the attention of many of the young surgeons of the day. Among these I may particularize Dr. Richard Mackenzie, surgeon to the Royal Infirmary of Edinburgh, who, although remarkably familiar with excision of the elbow, took the trouble of a distant journey and voyage to see Mr. Jones's cases and operations. The impression on his mind was such as to induce further investigation; and had it not been for his untimely death, in pursuit of professional knowledge and honour "*at the cannon's mouth*" in the Crimea, I have no doubt in my own mind that

we might have had by this date an amount of evidence for and against that would have gone far to settle in the minds of many who still doubted, the very important question as to the eligibility of the operation. Beyond the impression which my own humble exertions and example made, nothing went further for a time than that produced by the active practice of Mr. Jones. That gentleman, in a comparatively limited sphere, operated on fifteen cases, and in fourteen of these with success.

Various pupils of my own devoted special attention to this subject, amongst whom I may mention Mackenzie, Smith, Edwards, and Price; but fortunately there were others on whom my personal influence might be supposed to have little effect, who wished to give trial to this comparatively novel proceeding, and thus the operation was practically tested on a pretty extensive scale. It seems almost invidious to mention names where so many are concerned, and it may suffice, particularly for my present purpose, if I refer to the writings of Mr. Butcher of Dublin, Mr. Pemberton of Birmingham, and Dr. Humphry of Cambridge. These gentlemen have, each in his own way, given remarkable notoriety to this operation, and it was at a still more recent date put again before the profession by Mr. W. M. Clark of Bristol, in a paper of great research which he read to the British Medical Association in August, 1863.

Excision of the knee seems, like all similar operations on other joints, a very formidable process, particularly when performed on the normal tissues in the dead, or when witnessed by any one not familiar with such operations on the living body. The destruction of tissues and parts, on the dead, before the component parts of a joint can be cut away, appears more than human strength can bear or survive; and the chaos of tissues, structures, and surfaces, with, in most instances, the discharge and separation of blood, pus, and substance, perceptible in the living body, naturally lead any one, as yet uninitiated to this practice, to the conclusion, that for roughness, tediousness, magnitude, and irregularity of surface, the chances of the patient for limb or life are slender indeed. There is scarcely a coarser operation in surgery than excision of the elbow, yet in the present day no one objects to it either on that score or on any other, unless it be some old stagefs, who adhere, even in the wane of the nineteenth century, to the customs of their grandfathers. Excision of the knee is simple and elegant in comparison, although it must look formidable in all respects to the novice. It is a common custom to compare amputation and excision, as regards local appearances and facilities of proceeding, and amputation invariably carries the palm. The clean incised wound in the arm or leg for diseases of elbow or knee seems what might be called a luxury to the patient when compared with the haggling process of excision. Yet in excision the member is left; in amputation it is gone! No human power can restore it. Look to the condition of one who has had his arm amputated for disease of the elbow-joint, and look to one who has had an equally successful excision. No artificial substitute can compare with the human hand!

If excision of the elbow has superseded amputation in the arm as a general practice for incurable disease of the elbow, why should the practice not hold equally good in the lower limb? That is a question which I for years put to myself,—which I still do, though in a less uncertain mood; and it is a question which I avail myself of such an opportunity as this to put again to myself and the profession.

The largest number of collected and original cases with which I am acquainted was made some years ago by my friend and former assistant, Mr. Price. The list amounted to nearly 250, and went to show that the fatality was pretty much the same in excision of the knee and amputation in the thigh. I do not think that as yet we have data of greater importance on this point. The success, or want of success, of amputation in the thigh, as detailed by Mr. James, of Exeter, Mr. Bryant, of Guy's, or Mr. Callender, of St. Bartholomew's, can have no direct bearing on this question until a like number of cases of excision of the knee in the same or similar institutions, can be *brought to bear on the point*. There is nothing in physiology, anato-

my, pathology, or practical manipulation, which should make this operation more hazardous to life in proportion to amputation in the thigh, than excision of the elbow is in proportion to amputation in the arm.

My impression is that excision of the knee is, or should be, by proper treatment, as little destructive to life as amputation in the thigh, and if this be proved and granted, as possibly it may be in time, then I firmly believe that all other objections to this operation must pass away like those which so long retarded the application of excision to the elbow.

The time required for a cure, so called, has been a great element in this operation. It has been common in amputation of the thigh to dismiss patients from hospital, or to give up regular attendance, in the course of three, five, seven, or ten weeks after. Six weeks may be taken as a favourable average period to show the rapidity of cure. Little, or, perhaps, no notice has been taken of some small sores or sinuses still present; nor has the after-course of these, or of the stump, been specially referred to, excepting when, perhaps, many months or years after, there has been a necessity to repeat an amputation for an ill-conditioned stump, or to remove some portion or portions of necrosed bone. But even then a whisper has never been raised against amputation, or the particular kind of amputation; or, possibly, the surgeon who performed the operation may have been criticised freely, but the fault has never been (and very properly so in my opinion) laid to amputation itself as a surgical operation. In the elbow and shoulder, where excisions are extolled or sanctioned, the wounds are in reality often slow in healing; months—ay, years—elapsing in even most satisfactory cases before the sinuses are perfectly healed. But in such instances the patients can move about with seemingly little defect. The arm can be carried; but if anything prevents the lower limb doing its proper carrying function the defect is more manifest. As the knee is big in proportion to the elbow, so may the entire healing process in excision be longer in proportion: and as the subsequent strain upon the lower limb is greater than that upon the upper, so must there be greater time given for a perfect result. These features, with a slight variety, are admitted between stumps of the lower and upper extremities, and they seem to me to be equally worthy of attention in the cases of excision.

But in my opinion a mistake has been committed in such comparison; for the wound in all great features is not different from that of amputation, and the continued presence of the lower part of the limb constitutes such an important fact, as compared with its absence, that it seems to me a waste of time and argument to dwell further on this question. The true comparison of excision of the knee is with compound fracture of the lower end of the femur or upper end of the tibia, possibly communicating with the knee-joint, or with a direct penetrating wound of the joint itself, which has exposed the interior to the surrounding air. If I am not much mistaken, the feeling in modern surgery is, that time is of little moment contrasted with a useful and therefore creditable limb. If months, even years, elapse before such results are effected, does surgeon or patient ever begrudge the time? Is there a surgeon of experience who has not felt his heart palpitate with pleasure on looking at one of these illustrations of the powers of nature, and of his own faith and skill, in bringing about such a pleasing conclusion? Suppose a compound fracture—say of the shaft of the tibia; has he not been gratified, even after the separation of inches of the necrosed bone, to see a useful limb retained? Does he not feel in his heart that Ambrose Paré and Percival Pott did not live and suffer in vain?

As a compound fracture in the lower extremity is uncertain and slow in healing compared with a simple one, so may the wound in excision of the knee be compared with the healing of a stump. But, to put this part of my argument briefly and on a practical basis, I admit freely that such wounds are generally more slow—much more slow, in healing than others; yet I have seen after excision of the knee what I have never seen after compound fracture of the tibia—patients walking about freely, on crutches, eight weeks, *six weeks*, even three weeks afterwards; some even putting the foot to the *ground with a pressure* which no stump of the thigh could have borne at such

a date. But I must refer to this subject again, and to give it justice I must bring others under present notice.

The condition of the limb afterwards has, perhaps, attracted most attention. In a brief lecture like this, and coming near the end of it too, it would be impossible to notice all that has been said on this subject. The result in Park's first case seems to have been almost overlooked. From 1789 until within these few years all by silent assent seem to have sanctioned the conclusion that a stump in the thigh and an artificial limb, whatever its kind, were better than protracted disease or death. Excision well-nigh slept, and, with perhaps the exception of what had been done and said by Crampton, Syme, myself, and Jones, no one seemed to have thought seriously on the subject.

[To the fact that, after excision of the knee in persons under full size, the limb does not grow in proportion to the other, attention was first drawn by Dr. Humphry, of Cambridge, and Mr. Pemberton, of Birmingham. In some cases the limb actually ceases to grow, and is out-stripped by its fellow some five or six inches.]

This is hardly the time or place to discuss the merits or questionable points of Dr. Humphry's ingenious and admirable papers on the growth of bones in their long axis at their epiphysial cartilages. I mean, therefore, to allude to them briefly. They seem to have been suggested chiefly, if not solely, by excision of the knee, and their main object appears to be that of pointing out generally that long bones grow in length entirely at the cartilage at the end of the bone next the epiphysis, and particularly that the femur grows in length chiefly at its lower end, where it joins or is joined by the epiphysis. The practical deduction from this is, that if, in resection of the knee in a growing subject, the epiphysis and epiphysial cartilages be cut away, the femur will not grow in proportion to the rest of the body; and hence a serious objection has been raised to the operation in young persons. This matter was first hinted at in Dr. Humphry's paper read before the Medical and Chirurgical Society in March, 1858, and was more elaborately worked out in subsequent papers laid before that Society in 1861 and 1862. The same author has alluded to the subject in his valuable "Treatise on the Human Skeleton," published in 1858, and he there comes to the conclusion that in such instances, when the tibia and femur might unite by ossific junction, "the objection urged against the operation might prove valid."

The union of physiology, science, and practice here has given great force to the observations of Dr. Humphry; but attention was still more prominently brought upon the subject by a remarkable paper published by Mr. Oliver Pemberton, of Birmingham, in 1859. The main object of this paper was to show that in a youth operated on for excision of the knee in 1854, the limb in 1859 was nine inches shorter than its fellow. Another analogous case is cited from the practice of Dr. Keith, of Aberdeen, where, in the course of "nearly six years the deficiency of growth is measured by five inches." To add to this objectionable feature in Mr. Pemberton's case, there was no bony union; and in Dr. Keith's case, at the end of the time referred to "the union was not strong, and there was a bending outwards."

But the true parallel, the true value of excision of the knee has never yet been drawn to its full extent, in as far as I am aware. Life—life and comfort may be considered as the highest and best result of surgical interference in a case of incurable disease of the knee-joint. Even yet we have not sufficient data on those heads. My own impression is that the question of life between amputation and excision will be pretty nearly balanced; indeed, I shall venture a step further, and say that if excision were to get all the subsequent comparative advantages willingly given to amputation, the hazard of one operation would be less than that of the other. If I am not mistaken, an idea prevails that excision of the elbow is in reality a safer operation than amputation of the arm; but such is not mine. Few fatal cases of this excision have been recorded, but I have seen as many as to convince me that the mortality is probably as great as that of amputation above the elbow. Happily, *such a result is rare in either* instance.

Whatever the mode of amputation, it must be acknowledged, that neither skill, forethought, manipulative perfection, nor after treatment, whether local or constitutional, can invariably avert some of the evils (less than death) which are known from ample experience to follow such an operation. The occasional evils of secondary hemorrhage, of unusual retraction, of scanty covering, of chronic sore therefrom, of caries or necrosis, of tender cicatrix, of neuroma on the great nerves, and of secondary operative interference, have all been in a manner ignored in this comparison. And yet how often have all men of experience seen cases of the kind? Neuroma is certainly rare in the lower limb; yet, allowing the most perfect result—allowing the stump to be above criticism, what is it after all? It is only a portion of thigh whereon to fix an artificial limb. If this cannot be done, the sufferer must ever after be dependent on crutch and stick.

I beg it to be observed that I do not bring these features forward as objections to amputation in the thigh in cases where the operation may be deemed absolutely necessary. If that dire proceeding must be, patients must take their chance. Surgery can never entirely avert the occasional necessity for amputation, but in the case for which I now speak I maintain that she frequently can. And now let us see the case of excision. The foot and leg are left; the limb is shortened positively by the length of bone taken away—say from two inches to four. Allowing for the loss of growth in length before operation, and for arrest of growth after the operation—say five inches, say nine inches; allowing even more, the lower end of the thigh, the leg, ankle, and foot still remain. It is worthless in surgery to compare a bad stump with a bad lower limb after excision. Take the perfection of a stump, even in the estimation of the most critical and with even a medium limb after excision, and the comparison will not stand for an instant. With the best results, it seems absolutely absurd to compare the two.

In the general comparisons hitherto made I take leave to set aside the evils of excision, as drawn or indicated by the authorities already named, as exceptions to the rule, and as being equalled in evil in most respects by those following amputation; but I say unhesitatingly that if the comparison is drawn between the perfect stump and the perfect result of excision, the comparison is just as unreasonable as that between an artificial limb and one of flesh and blood.

Inequality in the length of the upper extremities is of little moment, but it is awkward, to say the least of it, in the lower, as we frequently observe after fractures and after disease of the hip or knee in early life. Such inequality may, in some instances, be attributed to bad treatment, but it often occurs despite the best skill in surgery. Yet who would in such a result say that the patient would have been better with the limb away by amputation? Who does not think that when a person gets well of a diseased knee or hip, with shortening even to the extent of five or nine inches—no uncommon result,—he is yet somewhat fortunate—fortunate in not having had amputation performed on his thigh. We do see occasionally cases of great distortion of the lower limb after disease of the knee, but even these patients sometimes congratulate themselves on having the leg and foot. In some few such cases amputation in the thigh has actually been performed years after the so-called cure. Yet in such cases it is not our custom to lay blame on the treatment which may have been adopted, although I believe that it has often been highly defective. Whatever the amount of distortion after the cessation of disease in the knee-joint, I hold that, excepting very special cases, amputation is unjustifiable, as resection of the distorted knee is both safer and better. But I shall not press this point at present; let me rather again draw attention to the fact, that we never cry out against either nature or the surgeon in cases of shortening and distortion of limbs after disease. Yet such defects are common. Whilst meditating these remarks, I have rarely known a day in taking one's ordinary rounds that I have not observed persons walking in the streets with shortened and distorted limbs after disease of the knee. Most of them have moved more nimbly, and with greater apparent security *and comfort, than if on* the artificial limb after amputation of the thigh. Yet

shortening has in a manner become the bugbear of excision of the knee. I admit that it is a defect, but abstraction or excision and arrest of development are evils elsewhere as well as here. Again, I admit that in this locality disparity of size, particularly in length, is an awkward circumstance. Let us see, however, in what this consists. A difference of three, five, or nine inches! What is the contrast which has been drawn with this defective limb? It is with its neighbour! And here I imagine we hit upon the weakest of all the objections hitherto made to excision of the knee. The risk of loss of life, distortion, uselessness of that which is preserved, are all serious objections or blemishes to this proceeding. A short leg to a long one is, I again admit, a defect; but in this respect surgery no more fails than Nature does after disease. What, I ask, is the alternative for excision of the knee proposed by those who object to this operation? It is amputation in the thigh! I cannot allow that which might be an easy answer to the question,—Why perform an operation at all? Why not cure the disease, and thereby avoid amputation? That is a question of a totally different kind. I am not now discussing the question of amputation, or continued, and possibly other treatment to save the limb. It is the question between excision and amputation as regards the future condition of the limb. In ordinary amputation under such circumstances, half the thigh, or possibly two-thirds, may be left. The body is mutilated to nearly the entire extent of one extremity. Say what you may as to the quality of the stump, there is left a shortened femur, a shrivelled thigh; emphatically a stump. Even Samuel Johnson's explanation of the term gives an exalted idea of the noun substantive which scarcely holds good with us: "The part of any solid body remaining after the rest is taken away," is a flattering description of one of our stumps of the thigh. It is in reality with us barely more than a peg whereon to hang an artificial limb. In youth, in middle age, in advanced years, it never improves. It never can be more than a shortened bone, with shortened and shrivelled materials around; and this even with the perfection of a stump. The defective results of excision I am disposed, in accordance with what I have said before, to class with defective stumps. In justice to the subject I now deal with, I take a fairly perfect result of excision. Whatever the shortness, that may readily be made up by a high-soled boot or shoe. There are left the lower end of the thigh, nearly the whole of the leg, the ankle and foot; the former two slightly damaged, the latter unscathed. The foot, leg, and thigh do as much as in the cases of distortion or shortening after disease; and who, under such circumstances, would compare an artificial substitute to the limb of life?

But I cannot leave the argument here. A well-healed stump never in reality improves, unless, possibly, it gets somewhat more callous, whilst often it gets more tender and irritable; but the seeming perfect result of excision at the end of six or twelve months (just when stumps are generally at the best) is no criterion of true perfection. If the limb is properly managed afterwards, it goes on improving for months—aye, for years. Without again discussing the question as to length, and without applying the remark to all, I can affirm, from ample experience in my own practice, that thigh, leg, and foot enlarge in bulk; and in particular, that with this change the leg and foot improve in muscular energy. It may to some be more impressive when I say that the calf of the leg shows again in increased muscularity and vigour. This observation has never yet, I believe, been dwelt upon by those who have written of the good qualities of limbs after excision; but it is an important fact, and one which, in my opinion, goes far to balance that of shortening, which has been so eagerly put forward by writers previously referred to.

I do not think that the value of the human foot has been sufficiently estimated by those who amputate, or even by some who advocate excision. It is certainly as wonderful in its mechanism, if not more so, as any of the organs of special sense; and, without drawing a useless comparison between it and the hand, it is certainly in its entire state a thousand times more perfect, as part of the future support of the body, than the point of a thick stick, or any fabricated imitation of a foot. We see persons walking about with limbs

shortened from various causes; some with high-soled boots, some with apparatus of iron, some with a pin of wood to make up the proper length, each with a foot of tolerable dimensions and vigour. Whatever we may say to grace or symmetry in these cases, we yet, under ordinary circumstances, consider that a fair compromise has been made with formidable disease. Even as an unfortunate maimed one who has suffered amputation in the thigh halts along, we may pity him as the victim of imcurable disease, yet we claim his case as a bright illustration of the powers of surgery. The foot is rarely thought of, whether it has been swept away by amputation, or it be a portion of a shortened distorted limb. Yet I doubt if there is any substitute at all to be compared with it.—*Lancet, July* 9 *and* 16, 1864, *pp.* 31, 59.

37.—ON WOUNDS INTO LARGE JOINTS.

By Dr. Wm. Stuart Monro, Surgeon to the West Hartlepool Iron Rolling Mills, and Iron Ship Yard, &c.

Out of a large Surgical practice I select the two following cases of severe wounds penetrating into large joints, which I think are sufficient proofs of what may be accomplished in saving limbs, though in the case of a working man I am decidedly of opinion that it would be preferable had amputation of the leg been at once performed in the latter case:—

John Harrison, aged thirty-three, a labourer on this railway, of temperate habits and previous excellent health, when at Coxhoe on duty on the 16th of April, 1862, fell off a wagon when in slow motion, and a part of the wheel coming in contact with the left elbow, broke the humerus into several pieces in the joint, and also the same bone at about an inch above this articulation, where it projected through a hole in the integuments. The olecranon process was also detached from the ulna, and a small hole over it was in the skin; the radius was also broken in its upper third, and there was a small hole over the external condyle opening directly into the joint.

The accident happened about 12 noon, and he had to be removed hither, a distance of 35 miles, before any surgical assistance could be procured. During this journey, which occupied three and a-half hours, he lost a very large quantity of blood.

On his arrival a Surgeon was in immediate attendance, and called in another Medical gentlemen when it was decided that the arm must come off. To this, however, the man and his friends objected, and when I was called in the case appeared so doubtful that I proposed to operate as if for excision of the joint, and remove all the broken pieces—there being no sign of any injury having been done to the ulnar nerve, and the pulse at the wrist was as good as could be expected after such a loss of blood.

I saw no more of the case until the 18th, when I was again called in to meet Mr. Foss, of Stockton. The arm was now much swelled and discoloured, and the pulse at the wrist much smaller and weaker than at the other arm, and evidently labouring to overcome the increased local pressure on its circulation. It was now deemed too late to excise, and every means were at once adopted to save the arm.

April 19.—Bowels freely opened; tongue slightly furred; pulse smaller and weaker in the injured than in the other arm; swelling and discolouration a little increased since yesterday; considerable anxiety in countenance; did not sleep well last night; feels very weak. Is ordered beef tea and a little wine.

20th.—Slept very badly, which he ascribes partly to the position (on his back) in which he was compelled to lie since the accident, and partly to the close bed (one in a wall) to which he is confined. His countenance is very anxious, his arm more swelled and discoloured than it was yesterday, and its pulse very weak, evidently approaching stagnation. Ordered to get six leeches applied immediately to the front of the arm and to diminish the quantity of wine during the day.

Vesper, 20th.—Leeches bled well, gave great relief, reduced the swelling and a little of the discolouration. Have lifted him out of bed, placed the arm in an open case, and made him comfortable for the night in a large chair.

21st.—Swelling still troublesome, but he is now in much better spirits. Slept a little last night, and is considerably refreshed. Pulse at the wrist of injured arm still much smaller and weaker than at the other.

22nd.—Have had to reapply the leeches, the discolouration having increased so much as to create alarm. The swelling, however, is not so great as on the 20th, and he is in excellent spirits.

25th.—All has gone right since last report. He is now placed on generous diet, sleeps well, is in good spirits, and has the arm, which is placed firmly in the supine position in a wooden case and suspended by a sling from the neck, dressed morning and evening with large plasters of strong turpentine liniment. The wound where the humerus projected through the skin is nearly well, and the other two have coalesced into one. When dressing, the cartilage on the ends of the humerus and radius can be distinctly seen and synovia running out of joint.

May 2.—Everything has gone on well since last report. Upper wound completely healed and the other one contracting gradually, though when passive motion is practised in the joint the radius can be seen to move freely on the humerus. The turpentine dressing gives him no pain, and he is now allowed to go out.

20th.—He is now able to attend at the surgery; wound into joint healed; there are signs as if small portions of the olecranon would come away.

September 6.—Has now been at work as a gate-keeper on a railway crossing for a month. He is gradually recovering the use of his arm in the motions of flexion and extension, but those of pronation and supination are almost entirely gone. He has no pain in the joint, and his general health is excellent.

August 15, 1864.—Since last report several small pieces of bone came away from the olecranon, but none within the last nine months. Has now a very useful arm, and is in the very best of health.

James M., aged forty-five, a healthy and sober man, of a spare habit of body, and by trade a rigger in the iron ship yard here, when at work aloft on December 30, 1863, lost his footing, and made a spring to save himself, landing on the ground on his feet after falling nearly ten yards.

When carried home he was found to have not merely sprained his right ankle, but to have nearly completely smashed the astragalus and broken the tibia and fibula in the left leg within about one inch of the ankle-joint.

He would not hear of amputation, so an effort was made to save the limb by placing it comfortably and firmly in McIntyre's splint; his diet was ordered to be liberal, and as all the functions of the body were properly carried on, no general treatment was adopted.

January 25.—As he has been complaining of sharp pain in the outer ankle for a few days past, the bandages and splint were removed, exposing considerable inflammation and threatened sloughing at the seat of pain. Poultices were ordered to be applied.

27th.—A large slough came away last night, leaving an opening into the joint about the size of a walnut, and about two inches of the fibula (now united) exposed and quite bare. Treatment ordered to consist of good support, stimulants, and turpentine liniment dressings.

June 14.—The wound has been gradually healing under the treatment, but there is about half an inch of the fibula remaining uncovered, and a small aperture on the dorsum of the foot, through which a probe detects the roughened surface of the astragalus.

July 15.—The exposed piece of fibula, weighing one and a-half scruple, has come away, but a little thick pus still escapes from the aperture on the dorsum of the foot.

August 15.—Not being able to support himself longer, he was compelled to apply to the Parochial Board for relief, and is now, I believe, an inmate of the Sunderland Infirmary, though before he removed thither he was able

to bear the weight of his body, in the acts of standing and walking, on the injured limb.—*Medical Times and Gazette, August* 27, 1864, *p.* 219.

38.—ON THE WIRE COMPRESS AS A SUBSTITUTE FOR THE LIGATURE.

By John Dix, Esq., L. S. A., Surgeon to the Hull and Sculcoates Dispensary, &c.

[The method of arresting hemorrhage known as acupressure is well known to the profession, but has we think not been tried to the extent which it deserves, especially by the London surgeons. Some have tried it and abandoned its use, others who have tried it continue to employ it and speak favourably of it.]

For myself I early made trial of the needles. My case—an amputation of the arm—is published in detail in the Medical Times and Gazette of 2nd June, 1860. As there related, I was to a certain extent pleased with the process; but I also observed in it certain inherent disadvantages.

Most, or all of these, as it seemed to me, might be obviated by the use of a fine iron or silver wire instead of the steel needles, the principle of action being the same, and the mode of application not dissimilar.

So soon as opportunity offered, I put this idea to the proof. The results are the subject-matter of this paper.

Having satisfied myself of its feasibility by some experiments on the dead, I first used the wire on a living subject in a case of amputation of a finger, in September, 1860.

The patient was a woman, aged fifty years, affected with constitutional syphilis, the second finger of whose right hand required removal on account of caries. The head of the metacarpal bone was covered over by a single flap, taken from the internal lateral aspect of the finger. Two arteries were secured by compression—a fine silver wire being used for that purpose. These wires were removed on the third day. There was no bleeding, and the wound healed readily, though not absolutely without suppuration.

Encouraged by this success, and convinced of its safety, I next used the "wire compress"—as I propose to designate this method—in the same patient, upon whom Chopart's amputation was performed (April 26, 1861), for extensive disease of the cuneiform and adjoining metatarsal bones.

The operation was done in the usual way—a long flap being cut from the sole of her foot. Five bleeding arteries were secured by silver wire, in the way about to be described. The flap was carefully adjusted, with many sutures of iron wire, and from the appearance of the stump, the universal verdict of bystanders was, "*that* seems a great improvement on the needles."

On the 28th, viz., forty-eight hours after four of the wires were withdrawn without difficulty and without bleeding; and on the 30th, or fourth day, the remaining wire, which commanded the posterior tibial artery, was in like manner removed. It was found that the wire was easily applied, as easily taken away, and entirely effectual for the purpose it was intended to serve, viz., the arrest of the bleeding from the cut vessels.

The patient, as I have before explained, was the subject of constitutional disease, and the case did badly from the first. There was sloughing of the entire surface of the wound, and this process gradually involved the substance of the flap, which was ultimately almost entirely destroyed.

This was an unfortunate thing for the patient, and not very satisfactory to the surgeon; but it was certainly advantageous to science, inasmuch as it put this method of securing arteries to the severest possible test, and also gave me an opportunity of observing the process and its effects more closely and accurately than would otherwise have been possible.

So soon as the sloughing action set in, the sutures were removed, and the flap opened out, and thoroughly cleaned by gentle washing. Thus it happens that I am in a position to speak positively about the action of the wire *compress upon the* arteries so treated. It was found then that the continuity

of the vessel was not destroyed where it was compressed by the wire; it was not divided, nor was its terminal portion cut through as by the ligature. The patulous end was securely sealed and closed, instead of sloughing away, as necessarily happened when a silken thread was fastened thereupon, which could only be got rid of by destruction of the tissue upon which it was fixed.

Nor can anything more clearly prove the firmness and security of the adhesion, which this process excites in the vessel itself. Every surgeon knows how common is secondary hemorrhage in a sloughing stump, and understands the reason why. I certainly expected it in this case. I think I have observed before, in cases of hospital and other forms of gangrene, that an artery is by no means the first tissue to slough. Its tough and well nourished coats often resist the destructive process longer than surrounding textures, and the length of time required for a ligature to become detached from a large artery, confirms this view.

But however this may be, it is certain that in this case the divided ends of the arteries exhibited healthy granulation, whilst other tissues were sloughing around. I shall have occasion to refer to this highly important and interesting observation again by and by.

This patient progressed from bad to worse; unhealthy suppuration and sloughing extended up the ankle in the sheaths of the tendons, and ere long pyæmia set in—of which she died on 9th May, being thirteen days after the amputation.

I related these cases at a meeting of the East York and North Lincoln Branch of the British Medical Association, on 22d May, 1861.

The third and last case in which I have used this method was an amputation of the thigh, of which the following is a brief history:—

A. W., aged 20 years, had suffered from disease of the right knee-joint for 11 years, by which he was so dwarfed and emaciated that his appearance was that of a child of 14. There were sixteen discharging sinuses, and his weight was three and a half stones.

21st Sept., 1861.—Amputation was performed according to Mr. Luke's plan, viz., the posterior flap was first made by transfixion, and the anterior, of exactly similar size, by cutting from the surface down to the bone. The bone was sawn at the junction of the middle and lower thirds. Five wires were applied upon as many arteries, and with the femoral artery the femoral vein was intentionally included. Two small arterial branches were treated by torsion. The flaps, which fitted well, were accurately adapted by means of wire sutures, pads of lint, and strips of plaster.

The case went on favourably in every respect; there was very little bleeding at the operation, and none afterwards. The lad immediately ate and slept well, and was evidently much relieved by the removal of the offending member.

On the 24th, being seventy-two hours after the operation, four of the presse-artère wires were withdrawn with perfect ease and without bleeding. The wire upon the femoral artery remained until the 26th, that is, five days altogether, when it too was removed, without any difficulty and without a trace of blood. At this time considerable union had taken place between the flaps, but there was slight suppuration about the deeper parts; so that when I related this case at a medical meeting on October 9th, being the eighteenth day after operation, I spoke of it as a forward stump, and hoped for an early cure. In this I was disappointed; for although the lad improved in health and appearance, and was soon walking stoutly on crutches, still the stump did not heal. Three or four sinuses remained, discharging pus of a thin unhealthy character; the granulations were thin and flabby, with occasional outbursts of more acute suppuration.

The explanation of all this was subsequently found in a necrosis of the sawn end of the thigh bone. A considerable ring of this was gradually loosened, and cast off in the usual way; and when found to be detached was removed by an incision made into the face of the stump. In a few days after this the whole was soundly healed. It has continued so ever since; the lad walks particularly well on a common wooden peg leg, and is now ripening into a man, and earning *his own livelihood* as a railway clerk.

What then do these cases prove with regard to the method employed for securing the vessels? simply this, that it is practicable, efficient, safe, and manageable, nothing more. As yet I can show no positive results better than might have been obtained by the use of ligatures in the usual way. The great desideratum, primary union, has not as yet been arrived at. Any advantages then that may be claimed for this procedure at present rest *chiefly* on theoretical grounds; not *altogether* so either. Surely it has some superiority, indisputable and demonstrable, over the ligature, as I now propose to show. In pursuance of which object it will be convenient, first, to consider the effect, action, and results of the ligature.

It will probably be conceded by most surgeons, that a ligature is at best a necessary evil. It is a great nuisance in a wound, and the chief obstacle to primary union in many operations, and especially in amputations. Union by adhesion, after a large amputation, is an occurrence of extreme rarity. The recollections of a long surgical experience furnish but here and there an isolated case. Many men will say they have seen it once (I have met with no one who has seen it *more* than once), whilst many do not believe in it at all, and even doubt its possibility.

But how constantly does one read, in reports of surgery, such a sentence as this: "fifth day, complete union, *except in the track of the ligatures*," Such cases every one has seen; they are common enough. Surely then the inference is not far-fetched or illogical, that, but for the ligatures, suppuration might often be altogether avoided. What, indeed, is a thread of silk or hemp between the flaps but a miniature seton! The whole number of ligatures collectively would form one of considerable size, and are a necessary and certain exciter of suppuration; such a seton being, in fact, the very means we use when we wish to set up or to maintain this process.

Again, how is a ligature detached from the vessel to which it has been applied? Why, by ulceration! another unhealthy and objectionable process. The part of the artery encircled by the thread dies by strangulation, whilst a further portion, which has been drawn out and detached from its sheath for the application of the ligature actually sloughs away.

This drawing out of the artery by the forceps is of itself bad, as it breaks up the vascular and nutritive connexions of the vessel, and so retards those vital and physiological processes by which the closure and permanent sealing up of the cut tube is effected. How frequently, too, is this disturbance of the vessel itself, and of the reparative action going on around and within it, renewed by traction made on a ligature, supposed to be detached when it is not really so. In this way also it occasionally happens that a considerable piece of the artery above the site of the ligature is plucked away,—fully half an inch I have seen ere now.

The suffering occasioned by this experimental traction, be it futile or successful, is horribly familiar to every patient, and dreaded by every humane dresser. The pain of dressing a stump is over when once the ligatures are all out.

The knot of a ligature (often deeply buried between the flaps) cannot be withdrawn without breaking up whatever adhesion has taken place between the site of the artery and the edges of the integument. Hence, will occur bleeding from torn granulations, and manifest derangement of the healing process.

Again, a ligature once applied, the surgeon is at its mercy (so to speak), and the patient too. Who can say when it will be thrown off? I know a gentleman who carried one in his arm eleven months after amputation. I remember another, who, having had his testicle excised, had two ligatures remaining in the unhealed wound to the day of his death, which happened several weeks after the operation. It is obvious that so long as a thread remains, a complete cicatrisation is impossible.

But I must push this argument a little further. I need scarcely dilate on the advantages of primary union could it be obtained. Time is an important element in most cases. A cure by adhesion is the work of but five or six *days; the secondary* processes require nearly as many weeks. But there

are other considerations far more important than this. For instance, what are the chief sources of danger from amputation?

First and foremost, the much dreaded pyæmia, which, it is said, kills half those who die after amputation.

And the parent of pyæmia is suppuration. So soon as the secretion of pus commences, so soon is there danger of purulent infection, which occurs not at all during healthy adhesive repair. Who will gainsay the intimate relation between suppuration and the ligatures? Have we not seen that they are almost inseparably connected, and that the ligature may be, and indeed often is, an active source of pus formation.

The next most fatal accident is secondary hemorrhage, which arises chiefly or altogether from the ulceration or sloughing of an unsealed artery. And have we not proof that this very ulcerative process is set up *in the vessels themselves* by the ligatures? It is a necessity of the case, part and parcel of the natural action of the thread, which cannot in any other way be got rid of. On the other hand, it is not very clear why, except for this necessity of casting off the foreign body, ulceration of an artery should ever occur, at least in a healthy stump. Yet it happens not unfrequently that bleeding occurs, even when reparation is well advanced, and everything appears to be going on favourably.

Surely, these are striking and remarkable facts, and demand our most serious attention. Here are two unhealthy processes, the grand sources of the most fatal sequelæ of amputation, shown to be intimately mixed up with, or actually inaugurated by, the very means we use to stop the bleeding. I said that a ligature is at best a nuisance. I hope to show that it is no longer a necessity.

Yet I am prepared to hear, as I have heard before in discussions on this subject, that some surgeons are content with the results of the ligature.

Such I would ask to call to remembrance their cases of pyæmia and secondary hemorrhage following operations, and to remember that in their ligatures they have, to say the least, *a possible incentive* to both these dangers.

I entreat them to ponder over the following formula:—

Pyæmia is the offspring of purulent formation, of which the ligatures are an efficient and probable cause.

Bleeding arises solely from ulceration of a blood-vessel, of which the *primum mobile* is again the ligature.

Contrast with the foregoing statement (unexaggerated and unprejudiced, as I believe), the effects and action of the wire compress. I unhesitatingly assert that, from one and all of these objections to the ligature, it is almost or altogether free.

Thus, it has not to be detached by ulceration, neither does it give rise to this unhealthy and undesirable action. On this point I speak positively from absolute proof, as related in Case 2.

It has little or no tendency to excite suppuration, and certainly does not necessarily, or even usually, occasion it. This is in accordance with the well-known pathological law, that metallic substances, from not absorbing fluids, and perhaps from other causes not so well understood, are freely tolerated in the living body, and often remain therein embedded with perfect impunity for any length of time, not causing suppuration, ulceration, nor even irritation. Frequent experience of metallic sutures has proved this to the satisfaction of most surgeons, and it needs not to be further dwelt on here. It is an accepted fact in surgical pathology.

Again, the wire compress is applied without disturbance of the natural relations and vital connections of the vessel. It is removed at any time, according to the will and judgment of the surgeon, without any interference with the adhesive repair which may have taken place in the artery, and in the rest of the stump, almost without pain to the patient, and entirely without those torturing attempts and uncertain trials which appertain to the withdrawal of the ligatures. To which I may add that the wire once properly applied, is not liable to loose its hold, or become detached too soon, as not unfrequently happens when a *thread is tied upon* a brittle diseased artery, or upon a bleed-

ing vessel in a sloughing wound, as, for example, in secondary hemorrhage from hospital gangrene—that twigs of nerve, if accidentally included in the embrace of the wire, are not injured and excited as by the tight strangulation of a ligature. Hence, there will be but little or no twitching and jumping of the stump, which was an observed fact in the cases here narrated.

And, lastly, it is quite easy to compress the adjacent veins along with the arteries if it be deemed desirable. It is certainly well to do this if the veins show a tendency to bleed, as thereby all oozing and formation of coagula between the flaps—than which there is no greater obstacle to primary union—is entirely prevented. Even those who fear to tie a vein need not dread any ill results from the simple compression of it.

Surely all these collectively form an aggregate of no mean or trivial advantage as compared with the ligature.

It may be thought that the few cases in which this plan has been tried, are but a small experience on which to speak so confidently, and almost didactically. It is right, therefore, to state, that what is here advanced is deduced not from these cases alone, but also from the recorded results of acupressure, which has been successfully used in numerous instances, and which offers many of the advantages, and confirms most of the conclusions on which is based this advocacy of the "wire compress." *This* is in reality but a modification of acupressure, from which it differs in detail rather than in principle or *modus operandi*. Hence it is that I lay no stress on originality in the matter; this merit, so far as I know, belongs to Dr. Simpson alone. Yet, I believe the substitution of the wire for the needles to be a decided and important improvement, as *it* is free from most of, or I may say all, the shortcomings of "Simpson's Skewers," as the acupressure needles have been irreverently called.

The following may be mentioned as the chief defects of the needles:—When several of them are required, the stump resents as it were being thus pierced through and through in many places and in various directions. From this cause, and from the obstruction to the capillary circulation caused by the pressure of the unyielding steel, we find much tension, œdematous swelling, and in some cases very great pain. The pain especially has proved a very serious evil, so much so as to lead one surgeon to my knowledge to abandon the use of the needles entirely. Again, their projecting ends, and the puckering they cause in the substance of the flaps, interferes very much with that accurate adjustment of the cut surfaces and edges, which so greatly aids the chances of union by adhesion. None of these objections apply to the "wire compress."

The first point is with regard to the mode of application of the wire which may be best done in the following manner: take a piece of surgical wire, six or eight inches long, and thread each of its ends upon a straight needle.

Seize the bleeding mouth of the artery with forceps, and pass one of the aforesaid needles close on each side of the artery, about a line above the points of the forceps, directly down through the substance of the flap, so that they emerge at the cuticular surface, about half an inch distant from each other. Draw them both through together, till the curve of the wire compresses the vessel on the face of the flap. Now, get rid of the needles by clipping through the wire close above their eyes, also detach the artery forceps. Place a piece of cork cut for the occasion upon the skin between the points of exit of the wire, and over this twist the wire tighter and tighter till the bleeding is arrested. Lastly, cut off the superfluous wire. All which is done much quicker than described. Repeat this process upon as many vessels as require it.

Two arteries lying near together may be embraced by one wire; and, as I have said, the veins may be included or excluded at will.

The wire should be either silver, or, what is much cheaper and equally manageable, the finest and softest passive iron. The generality of the iron used for sutures is too hard and stiff. That which I have employed was supplied by Mr. W. B. Hilliard, surgical-instrument maker, Glasgow, who also furnished the needles, which are about three inches in length, straight, and three-edged, with an eye adapted for carrying wire.

Special care is necessary in threading the wire, that it be kept perfectly free from all kinking or twisting. The forceps are used, not to draw out the artery, as when a ligature has to be applied—this, indeed, is to be *particularly avoided*—but merely as a guide to mark the exact position and course of the vessel. The cork is necessary to protect the skin from the pressure of the wire. The stump will now be dressed according to the fashion of the operation.

Of course, objections will be raised to this scheme. The only one to which I need reply by anticipation, is that which naturally occurs to any one looking at the thing for the first time. How is that wire to be *withdrawn?* This, at first sight, appears an insuperable difficulty. Let any one who entertains that idea try the experiment on the dead subject, and he will probably be surprised, as I was, to find how easily and certainly it is effected. To which he may add, my *experimentum crucis*, that it is equally easy and certain in the living body. Here I ought to mention, that it was from Mr. Hilliard I first learnt that the abrupt bending of the wire is not an impediment to its removal. He also suggested to me the use of two needles in the way just described, by which the wire is applied much more expeditiously and exactly than by using a single needle. The only conceivable obstacle to its withdrawal would be a kink or hitch in the wire, which might cause much inconvenience. Hence the stress I have laid on the necessity of carefully avoiding this mischance in threading and fixing the wire. Its removal, no doubt, requires to be cautiously conducted. Thus, clip the wire close to the edge of the piece of cork, and straighten out the curve it has necessarily formed at its exit from the skin. Remove the cork, and apply instead the tip of one finger, with which press firmly upon the flap, making traction, gently and gradually, upon the other end of the wire. In this way it comes out with great facility; but if this were roughly and harshly done, it might break up the adhesion which we suppose has taken place between the surfaces of the flaps, and it is quite possible that a kink in the wire might lacerate the artery in passing over it.

But it is certain that none of these evils need happen with ordinary care and tact.

As to the period of withdrawal, further observations are desirable; but it has been shown, in numerous cases of acupressure, that for small vessels a few hours of compression is sufficient; and for the largest arteries a much less time than might, *a priori*, have been supposed. However, as a general rule, it is not desirable to disturb a wound in any way for from twenty-four to forty-eight hours, at the end of which time all wires commanding the secondary branches may be safely removed, and probably also from the large arteries; but, as a matter of prudence, one would at present prefer to keep a check upon such an one as the femoral for three or four days at least. Better it should remain needlessly long than be prematurely removed, for, as has been said repeatedly before, the presence of the wire is almost innocuous.

Hitherto I have spoken of this method of securing arteries only in reference to amputation of limbs. It is equally applicable to many other operations. Thus, supposing the testicle to be excised, how easily and nicely the vessels of the cord may be compressed by a wire, safely removable in a few hours. In a case of this kind, operated on a year ago, two ligatures were applied. One of these did not come away for nineteen days, during which time, of course, many attempts had been made to withdraw it, amid the grimaces and exclamations of the patient. Moreover, most of the wound healed by first intention, and I am firmly of opinion that but for the ligature there would have been no suppuration.

This method would also be particularly suitable in a case of popliteal aneurism. I would ask special attention to this, for in no case are its advantages more obvious and decided. Surely such a wound as that made during the operation, a single clean incision on healthy tissue, might reasonably be expected to heal at once, were it not for the foreign body, the ligature or seton, which keeps it *open*. Moreover, the ligature remains some eight or

ten days, long after it has ceased to be necessary or useful; and, above all things in the way of objection, *it sets up ulceration*, by which it has *to cut its way through the vessel* before it can be detached, whereby the blood channel is opened, and hemorrhage may and sometimes does result.

Suppose such a case treated by the wire compress. The wire would be passed under the artery by means of a tubular aneurism-needle made for the purpose, then brought through the integument at a convenient situation, by threading each end upon a common needle, and twisted upon a cork in the usual way. And now, mark the contrast. I have shown that in amputation of the thigh, the severed femoral is securely closed in three or four days by the action of metallic pressure, and there seems to be no reason why the same thing should not happen here; from which it follows that the wire might be safely removed in four days. Whilst there, it would be innocuous, not exciting suppuration; and in the end the coats of the artery would remain intact, and *bleeding would be impossible.*

Surely these are great and manifest advantages; as yet theoretical, I grant, but nevertheless self-evident, and based on sound reasoning and very exact analogy.

It will probably be found that the pedicle in ovariotomy may be conveniently dealt with by this method, the arteries being secured individually, and the entire stump also fixed to the abdominal wall by another wire. This idea has been already promulgated by my friend Mr. Spencer Wells.

To some wounds the wire compress is inapplicable. Thus, in the removal of a breast, it is usual to have bleeding from the twigs of the intercostal vessels which pass upwards through the muscles. Upon these the wire could not be applied; and the same difficulty might arise in the removal of any deep-seated tumour.

In these cases the short needles of Dr. Simpson have been found to act well; but I have elsewhere given reasons why the wire is preferable where it can be used. Neither should I expect to find it easy to apply the wire upon the vessels of a flat-faced stump made by the circular incision. But this is merely an additional reason, if such were needed, for preferring a flap operation.

It is scarcely necessary to allude to the idea, recently put forth as a novelty, of using *wire* as a *ligature*, by *tying it* upon the arteries. It is neither new nor good, for it is evident that *it*, like any other ligature, can only be cast off by ulceration, which process, as we have seen, the metallic substance does not readily excite, and therefore it would probably be more difficult to get rid of than even the old fashioned thread. Nor, I suppose, will any stress be laid upon the now exploded doctrine, that for the safe closure of an artery it is necessary that its inner coats should be divided. The results of acupressure have sufficiently shown the non-necessity of this. It is certain that the needles passing over an artery can effect nothing of this kind, and yet, by *its* use, secure and firm obliteration *does* take place, and that readily and speedily; from which it is evident, that if such a necessity existed about the beginning of this century, when the doctrine was first invented by Jones, it is no longer needful at present. Now-a-days it is sufficient that the current of blood should be arrested for a few hours by simple pressure.

I take it for granted, that some who read this paper will, ere long, be making trial of this contrivance. I would warn such against disappointment which might arise, from anticipating too much, and also at the same time protect myself from suspicion of exaggeration and extravagance. Therefore, let it be borne in mind, that *entire union by adhesion* in large wounds is not to be expected as a frequent event, neither will pyæmia and secondary hemorrhage be utterly abolished. But it cannot be denied, that ligatures are an impediment to healing, and that the absence of these impediments increases the probabilities of union; that the presence of a ligature in a *suppurating* wound increases the suppuration and prolongs its duration; and that the longer the period of purulent formation, the longer the danger of pyæmia exists, and the greater are the chances of the occurrence of this bane of surgery.

And if my statement should appear in some degree sanguine and enthusi-

astic, this arises solely from the implicit confidence I feel in the intrinsic merits of the little device just propounded. I have met with no difficulties or drawbacks that have not been frankly narrated, nor am I acquainted with any dangers that can be alleged against it; and, for myself, I should as soon think of stuffing a wound with charpie, or of searing a stump with a red-hot iron, as of using a ligature where the wire could be applied.

Allow me, then, in conclusion, to give in a few words, a tabular resumé of the points of contrast between the ligature and the wire compress.

The main distinctions are these:—

The ligature consists of *organic* material. It absorbs fluids of the wound—is itself liable to decomposition, and is very obnoxious to the living structures.	The wire being a *metallic* substance is non-absorbent—non-irritant, and almost innocuous to the tissues.
The ligature is fixed to, and fastened upon, the artery itself. It lacerates i s structure, and puckers up and corrugates the tube.	The wire is not attached to the vessel, nor indeed to anything else; it does not tear the coats of the artery, but closes it in a smooth and even manner, by gently pressing together its internal surfaces.

Which of these, it may be asked, is the more favourable condition for healthy adhesive repair, *quoad* the artery.

The ligature excites, promotes, and prolongs suppuration.	The wire has no tendency of this kind, hence will there be less danger of pyæmia, and a greater chance of adhesion of the wound.
The ligature necessarily causes ulceration of the artery, and death of the part on which it is tied.	The wire has no such effect, hence, secondary hemorrhage will be less likely to occur.
The ligature remains for an indefinite time, and on the other hand, from a brittle or sloughing artery, it may be cast off too soon.	The wire is removable at will, and at the same time it cannot be spontaneously dislodged. Another safeguard against bleeding.
The ligatures project between the edges of the integument, presenting a mechanical impediment to their union.	The wire does not interfere with the edges of the wound, nor with the adjustment of flaps: hence union by adhesion will be more probable.
In the application of the ligature, the vital connexions of the artery are damaged, and its *vasa vasorum* broken up.	The wire is applied without any disturbance of the artery itself, which therefore, is probably earlier and more securely closed.
The removal of the ligature is uncertain; ineffectual attempts are common, very painful, and injurious to reparative action; and the knot especially, in its withdrawal, tears through granulations and breaks up adhesion.	The wire is withdrawn at once, and certainly without any painful, premature tractions, or disturbance of parts, or any interruption to the healing process.

Of these advantages, some appertain equally to the needles, long or short; but the wire alone combines them all. It is, at the same time, free from the chief defects of acupressure, and in most cases will be found by far the most simple, manageable, effectual, and satisfactory method of applying *the principle of metallic compression*, any form of which is preferable to a ligature.

P.S.—Since the above paper was written, I have used the wire compress in two other cases, viz., excision of a testicle, and excision of a breast.

The testicle was very large from sero-cystic disease, the scrotum being also greatly distended by hydrocele, so that the wound was fully eight inches long. Two arteries in the cord—which was healthy—were secured by two separate wires, fixed, one on each side of the incision. They were not disturbed till the fourth day, because the pieces of cork seem to assist in keeping the edges of the wound in apposition. The case did well; the suppuration was slight, and the healing *rapid*.

The breast was affected with cancer. A cut had been made into it a few weeks before I saw it, in search of pus. From this wound a fungous growth was sprouting exuberantly. The incision for its removal was six inches long. Three arteries bled; none of these were deep-seated, and they were readily secured by the wires. These were all removed in twenty-four hours. For some days there was a considerable oozing of the liquefied fat; but the suppuration was very trivial, and in ten days the cicatrix was firm and the cure complete.—*Edinburgh Medical Journal, Sept.* 1864, *p.* 207.

39.—CASES IN WHICH VESSELS WERE SECURED BY WIRE LIGATURE, AND IN WHICH BOTH ENDS WERE CUT SHORT, AND LEFT IN THE WOUND.

Under the care of BARNARD HOLT, Esq., at the Westminster Hospital.

A. T., a moderately robust woman, aged forty, was sent into the hospital by Mr. Kelly, of Fetter-lane, suffering from carcinoma of the mamma. The patient stated that she noticed a small swelling about eight months since, which at first grew slowly; but two months before her admission, it had so rapidly increased as to be as large as a hen's egg. She complained of the ordinary lancinating pain, both in the tumour and also extending down the arm, which was sufficiently severe to materially interfere with her night's rest. The tumour was movable, unconnected with the deep structures, but adherent to the skin. The nipple was retracted. The mamma was excised by Mr. Holt a few days after her admission. Five arteries were secured with the wire ligature, and the ends being cut short, the surfaces were then approximated, and retained in apposition by suture and a light bandage. When the bandage was removed (the second day after the operation), almost the whole of the wound was found united by the first intention; a small portion near the centre, about half an inch, had not united, and gave exit to a trifling quantity of pus. On the fourth day all the sutures were removed, and the patient was able to be up. No inconvenience was experienced by the retention of the wire, and the woman speedily left the hospital with the wound sound. Five months after the operation, Mr. Holt received a report that no wire had escaped, and that not the slightest inconvenience had resulted from its retention.

C. H., was admitted under the care of Mr. Holt, suffering from carcinoma of the breast. Her history and appearance were very similar to the case already recorded; and Mr. Holt, on the fourth day from her entry into the hospital, removed the whole breast. Six vessels were ligatured with wire, and the ends of the ligatures cut as short as possible. The greater portion of the wound healed by the first intention, and the patient left the hospital in a much shorter time than usual. The detention of the wire gave not the slightest inconvenience either at the time or subsequently.

The third case occurred in Mr. Holt's private practice. He was called in consultation with Dr. George Pearce, of Westminster, to see a lady who had suffered for a long period from carcinoma of the breast without having received any advice for it. At last, however, the tumour became so exceedingly painful as to necessitate her consulting Dr. Pearce. Upon examination, the diseased mass was found not only to occupy the whole gland, but to have caused the axillary glands to become enlarged and diseased. The growth was adherent to the integuments, which had ulcerated to some extent, and the tumor could not be freely moved upon the chest. The whole mass, as well as three enlarged axillary glands, were removed. The bleeding was smart, and seven vessels required to be ligatured. The silver ligature was employed, and the ends of the ligatures were cut short. So much of the integument was diseased that it was found impracticable to bring the edges together; the bleeding points, with one exception, were covered. As in the former cases, no inconvenience resulted from detaining the wire; and Dr. Pearce informed Mr. *Holt some months* afterwards that the patient was convalescent.

J. B., aged thirty-three, of tolerably good constitution, was admitted on the 6th of February last. Twenty years ago his left ankle gradually enlarged, with considerable pain and inflammation; and he shortly afterwards became an in-patient at Guy's Hospital, under the care of Mr. Bransby Cooper, who made a free incision, and let out a considerable quantity of pus. Diseased bone was detected, and Mr. Cooper wished to amputate the limb; but the patient would not consent, and shortly left the hospital with the wound healed. Until within eight months of the present time he had had a useful foot; and though somewhat stiff at the ankle-joint, he yet suffered no pain. Last May, however, the ankle-joint began to swell again; and when he was admitted, Mr. Holt made a free incision, and let out a considerable quantity of pus. A few days afterwards he detected diseased bone, and, on account of the unhealthy condition of the integuments, he amputated the leg above the ankle-joint on the 23rd February. The ankle-joint and the articulations of the tarsus were destroyed, the bones in some places were quite bare, and the synovial membrane was greatly thickened with deposits of cretaceous matter. The internal lateral ligament was almost entirely destroyed; whilst the external lateral ligament was healthy. The wound healed rapidly, and none of the six wire ligatures which were used came away.

D. P., aged fifty-two, and looking emaciated and haggard, was admitted Feb. 2nd. Eighteen months previously he met with an injury to his left elbow, which caused considerable inflammation and ended in abscess. At the time of his admission the joint was quite anchylosed, with several sinuses leading down to bare bone. The integuments over the joint were greatly indurated and unhealthy-looking, and the muscles of the limb were very much wasted. Mr. Holt thought that amputation would in this case be preferable to excision, and accordingly amputated above the elbow. Five wire ligatures were used to tie the vessels; the ends were cut short, and the wound healed firmly. Nearly two months have elapsed since the operation, and no inconvenience has been caused by the retention of the wire ligatures.

In some clinical remarks Mr. Holt observed that, although in no instance were the incisions followed by immediate union through their entire extent, yet a much greater portion had healed by the first intention, and much less suppuration followed, than where the vessels were secured in the ordinary manner; and it appeared very probable that in cases of amputation of the leg and arm, the non-irritation of the wound had prevented any unfavourable result, the hospital being at the time of the performance of the operations in an especially unhealthy state from the presence of hospital phagedena, which had attacked many of the cases of ulcer and wounds. There was nothing new in the principle of cutting short the ends of the ligature and allowing them to be retained in the stump; for it would seem to have been simultaneously adopted, about 1798, by an American naval surgeon and Dr. Maxwell of Dumfries. Hennen and other military surgeons extensively followed the practice, and it was the last named surgeon who first suggested the use of hair ligatures. So far as the present cases went, they tended to show that as great security was afforded against secondary hemorrhage by the use of the wire as of the thread; and certainly there was less risk of abundant suppuration and of the occurrence of pyæmia than where the hempen ligature was employed. The surfaces of the wound were likewise kept in undisturbed approximation; the patient was saved the pain of removing the ligatures—a pain occasionally very severe; the suppuration was infinitely less; and in favourable cases there were great probabilities that immediate union might be secured through the entirety of the wound, the retention of the wire ligature not in the least degree militating against such a result, or giving the slightest inconvenience either immediate or remote. As a further proof of this, Mr. Holt referred to the continued good results which followed the subcutaneous tying of the veins in two cases of varicocele already reported in this journal. Mr. Holt also alluded to the probable advantage that might result in cases of aneurism where pressure had failed, but where it was desirable that a modified current of blood should be permitted to pass through the artery, by partially compressing the main artery by tying the wire loosely and

cutting both ends short; the experiment seemed to be worth a trial.—*Lancet, July* 23, 1864, *p.* 91.

40.—ON INJURIES OF THE HEAD.

By Dr. JOHN ASHHURST, Jr., Executive Officer to the Cuyler United States Army General Hospital.

There are certain remedies and modes of treatment in injuries of the head, and especially in fractures of the skull, which have become venerable by age, and which are hence liable to be adopted as correct, without its being considered if they are really commended by science and experience. I propose very briefly to consider the most prominent of these, and to state, as modestly as possible, my own convictions concerning them.

And first, as to trephining, or trepanning: that the operation has often been successful (that is to say, that patients have recovered after it) of course cannot be denied; but it is doubtful if they might not have recovered without it. Desault bored a great many men's heads, and they all died; he then gave up boring men's heads, and they all, or nearly all, got well. To elevate a depressed piece of bone is desirable, but so many cases recover where the bone remains depressed, and so many die where it is elevated by trephining, that the remedy has proved, in such cases, hardly an assistance to nature. The products of inflammation have been occasionally evacuated by trephining, but there have been so many unsuccessful and fatal cases, on the other side, that I think very few surgeons now would resort to the operation in this contingency. Of thirty-five cases in which the trephine has been used in our army, from September, 1862, to March 31st, 1863, twenty-eight died, two remained undecided, and but five have recovered; and it is not impossible that some of these might have recovered equally without operation. I do not say that I would never use the trephine; but I do say, unhesitatingly, that I have never seen a case where I think its use would have been justifiable. I would, of course, remove all loose fragments, and if I could elevate a depressed portion of bone without too much interference, I would do so, but beyond this I should be disposed to leave the case to nature.

Bleeding is another remedy which has been very largely employed in injuries of the head, and is still recommended by those who forego its use in other cases; and yet I think it is hardly indicated either by reason or experience. A clot on the brain will not relieved by bleeding, nor will bleeding prevent a clot's formation. The results of bleeding in inflammations of other organs have not been so favourable as to lead us to resort to it from analogy in inflammation of the brain; and bleeding as a prophylactic appears to me as irrational as it is cruel. I believe free stimulation is more often required in fracture of the skull than the use of the lancet. I have, indeed, recorded in this paper a case of fractured frontal sinus, which recovered after bleeding; but I should be loth to infer that the bleeding was the cause of that recovery.

Calomel and tartar emetic have been much used in injuries of the head, and Desault's experience with large doses of the latter was extremely favourable to its employment. But I imagine there are but few surgeons now who would be inclined to push it to the same extent that he did. If inflammation occur in these cases, I should be disposed to regard calomel in small doses as a remedy which might possibly be of use. But I cannot see the propriety of giving it in large amounts, as has been done, to every man who may have a broken skull.

The course then, which I would pursue in a case of fractured skull or contused brain, would be as follows: If the case were a compound fracture, I would remove any fragments that were detached, and if a portion of bone could be elevated without dangerous interference, would restore it to its proper position. I would place the patient in bed, in a darkened room, with his head shaved, and cold locally applied. I am convinced that the profession *does not appreciate* the great advantages to be derived from cold, and

specially dry cold, in surgical cases. I consider it second to no remedy in the treatment of injuries of the brain. Taking the view that I do, that the danger in these cases is from inflammation, I would endeavour to obviate any causes that might excite it, and to keep the system in a condition as well prepared to meet it as possible. As these accidents generally occur to men in robust health, and as there has generally been but little hemorrhage, the diet may, for the first few days, be very limited. There has been no drain upon the system as in other surgical injuries, and the risk of exhaustion and fatal debility is, therefore, less. I should not, however, hesitate, if it was indicated by the symptoms, to resort to free stimulation from the outset; and it will generally be necessary to have recourse to it in the progress of the case. I should use opium, in some form, freely in almost every case. I cannot understand the fear of opium in injuries or inflammation of the brain. The same principles which induce its administration in peritonitis make it suitable in these cases. I would, therefore, "put the brain in splints," if the expression may be allowed, by the use of opium.

But the most important thing is to carefully watch the case from day to day, and to endeavour, in the words of Dr. Watson, to "obviate the tendency to death." If coma be threatened, a few cups or a small blister to the back of the neck may be of service. Derivation from the bowels by means of purgatives may be sometimes particularly indicated. An attack of facial erysipelas (a mark of constitutional depression) will call for the use of wine or brandy; and in short, every case must be studied for itself, taking into view its natural history, so to speak, and its pathology, with all the lights that can be derived from reason, analogy, and experience.

In conclusion, I would say a few words as to two symptoms which are supposed to characterise fractures at the base of the skull. These are orbital ecchymosis, and bloody or watery discharges from the ear or nostril. Now neither of these is pathognomonic, and though, of course, their concurrence would make the diagnosis very probable, they might both be present, and yet no fracture exist. Orbital ecchymosis shows merely that a vessel is ruptured within the orbit; and this may occur in connexion with a fracture of the malar or superior maxillary, as in a case published by Mr. Holmes, and referred to by Mr. Hewett in his admirable monograph on injuries of the head, or even without any fracture at all, as in the case of McAllister, reported in the *Proceedings of the Pathological Society of Philadelphia (Am. Journ. of Med. Sci.* for July 1862, *p.* 112). Similarly a bloody or watery discharge may proceed from the cavity of the tympanum alone, without any fracture of the skull, as in cases reported respectively by Mr. Gray and Mr. Hewett. —*American Journal of Medical Sciences, July* 1864, *p.* 25.

41.—THE TRAVELS OF A BULLET.

By. J. Millikin, Esq., Borough, London.

About thirty years ago, one of the sons of the late Dr. Cheyne, of Dublin, whilst visiting an estate of his father's in the interior of Ireland, was shot; but whether by design or accident I cannot remember. The bullet entered either the cavity of the chest or abdomen. The principal surgeons of the metropolis hurried down to render aid, but the ball was not extracted. At length the patient was brought up to Dublin, and both Sir P. Crampton and Mr. Cusack used their utmost efforts to discover and remove it without effect. After some time he recovered, and for several years went about as well as ever he was, when suddenly he was attacked with symptoms of stone, which was found on sounding to be of large size. I cannot say which of the above gentlemen operated, but the stone was removed by one of them, the nucleus of which proved to be a leaden bullet. And Mr. Cheyne may, for all that I know, be in the full enjoyment at the present moment of sound health and spirits.—*Lancet, Aug.* 20, 1864, *p.* 221.

42.—ON SEPARATION OF THE EPIPHYSES OF LONG BONES.

By Jonathan Hutchinson, Esq.

I have taken frequent opportunities of directing attention to the subject of separation of the epiphyses of long bones. It is a kind of accident different on the one hand from a true fracture, and on the other, from a dislocation. It is a form of injury too of very great importance. In the first place, it is of frequent occurrence (in spite of the fact, that it is scarcely mentioned in any of our systematic works). Secondly, it is difficult both in diagnosis and treatment; and lastly, it is very apt to be followed by serious consequences. The peculiar dangers attached to it are, 1st, that periostitis may occur; and 2ndly, that the union when complete may be attended with much impairment to the adjacent joint. To these risks I may add a third, which is, however, of minor importance: viz., that the injured bone may not grow. I believe it is chiefly in the cases in which periostitis follows the injury, that arrest of growth is the consequence. The cartilage placed between the epiphysis and the shaft is damaged in the inflammation, and its further process of growth and ossification is arrested. Of course you are all well aware that bones grow in length by the ossification of their epiphysal ends, not by any change in the bony shaft itself.

Periostitis is more frequent after these injuries than after fractures, on account of the density, strength, and vascularity of the periosteum at the ends of bones. Muscles, ligaments, &c., are here inserted into it, and give to it a greatly increased thickness. This thickness and strength leads to the consequence, that in injuries involving the displacement of one part of the bone with much violence, the periosteum instead of being torn across is extensively stripped up.—*Clinical Lectures and Reports of London Hospital*, 1864, *p.* 88.

ALIMENTARY CANAL.

43.—ON THE OPERATION FOR HARE-LIP.

By William Fergusson, Esq., F. R. S., Surgeon Extraordinary to H. M. the Queen.

Regarding the operation itself, my personal experience extends to between 300 and 400 cases. Prior to 1850 I had kept notes of only a few of such cases. At that date I was asked to operate on one which had baffled the best efforts of Mr. Liston and Mr. Lonsdale, and at the same time to use a spring or truss, to push the sides of the lips forwards—an invention of Mr. Hainsby,

the father of the child. The accompanying wood-cut gives an illustration of the instrument when in use. The operation proved successful, and I have *good reason to be satisfied* that the instrument had been efficacious. Since *that time I have used* a similar appliance in upwards of 250 instances, and

consider it far superior to any other means—by straps or otherwise—for drawing the parts together. Particular notes were taken of these cases, some of which are interesting. 146 were males; 153 were on the left side; 53 were double fissure; and no less than 208 were associated with cleft palate. 169 of these have been operated on at King's College Hospital.

As to the results, three of these patients have died, seemingly from the proceeding; not from bleeding or shock, but from some child's ailment supervening, such as thrush or diarrhœa. I have never seen a single instance of convulsions afterwards, at any period of life, and I have operated at all ages between a few days old up to thirty-six years. Taking all things into consideration, I am of opinion that the earlier the operation is performed the better; assuredly before teething. I decidedly prefer about the end of the first month. In a simple case and healthy infant, it may be done any time earlier to within a few hours of birth. If the child is weakly and the gap large, particularly if complicated with split palate, I strongly advise delay for some months until additional strength is acquired, and also that the parts may be pushed closer in apposition by the use of the truss referred to. I have sometimes made babies wear this for many weeks or months before, and have always noticed its great value.

In double fissure I have generally operated on both sides at the same time. Occasionally, however, I have taken first one, and then the other; selecting the simplest first, and performing the second operation a few weeks—possibly months—after. In some of these double clefts the middle portion of the lip has been so scrimp that I have used it for the columna, particularly in instances where the nose has been flat. When it has seemed needful or best to take away the mesial projection, the closure of the gaps has always been a very easy matter, but when this part has been prominent, there has often been cause for much anxiety as to the result. The tension of the lip over this part has threatened to be too much for the recent adhesions. In only one instance has there been total failure of union, and in that I afterwards repeated the operation with success. In several cases there has been serious threatening of non-union by the gap opening an hour or two, or a day or two after the stitches have been removed. In such instances I have scraped the surface, introduced needles again, and put all up as at first, and thus made the process appear only as one. This method I have rarely seen fail. On one occasion a child was running about eight days after a very successful operation for a single fissure. It unfortunately fell on its face, and at once split the union open. Although eight miles off, it was brought to me within a couple of hours, when I introduced fresh needles, and with the ordinary care the result was as perfect as could be desired.

After trying a variety of lines of incisions, and seemingly cunning devices for adaptation of opposite surfaces, so as to give the best possible appearance to the lip, I confess that with few exceptions, the old-fashioned straight line, from the root of the cleft to the free margin of the lip, appears to me to be the best. If a notch or irregularity is left in the lip, it arises generally, I believe, from too little having been cut away from the margins of the fissure.

To make sure of a good and easy approximation of surfaces, I strongly recommend a free separation of each side of the fissure from the alveoli. Some have said that the frænum on the mesial side in a single fissure should not on any account be cut. It is often unusually large in such cases, and I say from my experience, that I see no reason why it should not be cut as readily and freely as any other part of the mucous junction of the lip to the jaw.

To take a refined view of a perfect operation, I have myself found the most difficult part to be that of bringing the opposite sides so accurately together that the margin between the mucous membrane and skin should meet on a proper level.

The position of patient and surgeon during the operation deserves some notice. I have often seen the operator sit or stand in front of his patient, and, in general, before the work has been finished, his face and dress have been *spluttered over with blood*, saliva, and mucus. A far better plan is to

be behind, or at the side, so that all the annoyance referred to may be avoided. In infancy the head should rest between the surgeon's knees, who should sit; and in the adult the operator may stand behind a chair on which his patient sits, or at the head or side of a table on which his patient is laid. In either of these positions he may escape the unseemly damage to personal appearance to which I refer.

A few of my infantile patients have taken the breast after the operation, but most have been fed by hand, and some modern devices with caoutchouc and bottles have been of great service.

Many of these observations are of no novel character, but possibly the experience which I have had may render them of additional value. It may, perhaps, be thought, that in bringing this minor subject in surgery before you in these lectures, I deal but lightly with my position. You may have noticed that I look upon some of the so-called minor subjects in surgery as being far more important than some imagine; and in extenuation of my present course, I may refer to the circumstance that the illustrious Roux, in writing the experience of forty years of surgical practice, did not disdain this topic, but actually made it the subject of one of his famous letters to "Cher Lawrence," his equally distinguished and experienced contemporary.—*Lancet, June* 25, 1864, *p.* 721.

44.—ON THE OPERATION FOR CLEFT PALATE.

By William Fergusson, Esq., F.R.S., Surgeon Extraordinary to H. M. the Queen.

The early history of the operation for cleft palate sounds like a romance. In 1819 a medical student applied to Roux, then one of the surgical luminaries in Paris, with a defect of this kind. Roux pared the edges of the cleft, and brought them together with stitches. Union followed; the palate became like a normal one, and when the youth appeared amongst his former friends, the change in his voice was such that he could scarcely be recognized as the same person.

I doubt if this case, although fairly made public by Dr. Stephenson in his inaugural dissertation on Velo-synthesis, when taking his degree of M. D. in the University of Edinburgh in 1820, produced the full effect on the surgical mind that it should, even when further elucidated in the famous essay by Roux, published in 1825. Possibly the rarity of the condition and the difficulties of the operation led to apathy, and down to the period of Roux's death no one seems to have had any experience on the subject at all equivalent to his. Like others taking their early surgical lessons in the third decade of the present century, I was attracted by the romance referred to. But I had seen little to absorb special attention. Whilst busy in dissecting-room work, a subject with cleft palate came under notice. At that time, as even now I suppose, few students took the pains to dissect the palate; but it was my fortune to have this one to luxuriate upon. I made a careful dissection of all the muscular apparatus, and came to the conclusion that I had rarely seen it so highly developed, although the palate and throat were small, being those of an aged female. The whole matter fell aside for years. I had performed the operation on the living body, and had heard of others doing so, without success. The subject in a manner slept on this side of the Atlantic, with the exception of the doings of Roux himself; but about 1840 all Europe, in a surgical sense, rang with the brilliancy of Stromeyer's operations for club-foot and Dieffenbach's for strabismus. Tenotomy and myotomy became the fashionable surgical mania, and I bethought me of my former dissection of the cleft palate. For anything that I knew, it was original. I compared it with the normal condition anatomically and physiologically, and then reflected on what I had seen and heard of surgery as applied to this condition by Roux and others. My zeal was further stimulated by a paper by Dr. Mason Warren, of Boston, which told of a larger proportionate success by Dr. Mütter, of Philadelphia, and himself than to my knowledge had

yet been attained by any others, not even excepting Roux. On additional reflection, I fancied that I had fallen upon new views in anatomy, physiology, and surgery, and my conclusions were embodied in a paper, which was submitted to the Medical and Chirurgical Society of London in December, 1844. That paper was honored with a place in the volume of "Transactions" of the Society for 1845. Its main features went to show how the cleft palate was closed in deglutition by the action of the superior constrictor of the pharynx; how the palato-pharyngei in cleft palate acted differently in this state than in the normal palate, and, instead of closing the opening between the pharynx and the nares, in reality tended to draw the parts asunder—an act which was overbalanced by the vigour of the upper constrictors of the pharynx. Above all, looking to the surgical aspect of the malformation, I gave it as my opinion that the action of the levatores palati probably exercised such an influence on the lateral portions of the palate, after the operation of Roux, as to mar its good intentions. I showed, in as far as one could by reference to the dead and living parts, how the levator muscle on each side had such free and uncontrolled action that, whenever excited, it drew the margin of the cleft outwards and upwards, and so tugged upon the stitches put in by the surgeon that ulceration in their sites and separation of the junction was a most probable result—that, indeed, which had caused the failure of Roux's operation in so many instances.

The inferences which I drew were, that if the palato-glossus, palato-pharyngeus, and levator palati on each side were divided, the soft flaps would thereafter, for a time, be so relaxed that in all probability the mesial line of adaptation would be so little disturbed that union would take place. The tensor palati I considered would have little disturbing influence, nor did I put much importance on that of the palato-glossus. My impression was, that the action of the levator palati and palato-pharyngeus, particularly that part in the posterior pillar of the fauces, was likely to prove detrimental; and, in accordance with the somewhat novel and already popular practice of myotomy and tenotomy in other directions, I recommended division of these muscles as an adjunct to the ordinary operation for cleft palate.

By modern custom, the department of anatomy associated with the professorship of surgery in this college has been held of comparatively little account, although both my predecessors have displayed remarkable acquirements in this direction, which they have turned to great account in the field of surgery. As professor of human anatomy, I hope that I may not be out of order in claiming to be the first who solved the problem of how the cleft in the soft palate is closed during deglutition. The drawing influence of muscle has been most recognised; the pushing has been less taken into account, although it is very considerable. Swallowing, the vermicular action of the intestines, and defecation, are notable examples of this force, just as palpable in the estimation of the anatomist as the drawing of the biceps in flexing the arm. Anatomy without physiology would be in a manner senseless, but when joined together they give life and soul to surgery. The influence of the constrictor muscles of the pharynx in the process of deglutition was well known to physiologists; but how, during that process, the gap of the cleft palate was closed in vacant space was an enigma, until I had the good fortune to show that the parts are pushed together by the action of the superior constrictor particularly, so that the gap between the pharynx and the nostril is as completely closed during deglutition as if the velum were entire. Then, for the surgical aspect of the investigation, I showed that by temporarily taking off the influence of such muscles as in common action tended to draw the two portions of the soft palate aside, there was a probability of such entire rest that union in the central line was most likely to take place—certainly, at any rate, more likely than with these muscles in full vigour, irritated, too, as they might be by the wounds, by inflammation, and by the presence of stitches. The almost intolerable distress, by depressing influence, the actual danger, associated with the injunction against swallowing laid down by Roux and others, made the early operations of this kind examples of human endurance which few could *follow out to* the full extent. Such injunctions had

been occasionally disregarded, and Sir Philip Crampton gave some notable examples of this kind. Since I showed, anatomically and physiologically, that during deglutition the parts are actully pushed together, that process is no longer forbidden; and now a fair share of suitable nourishment is freely administered—a matter of great consequence as regards successful issue.

With a single exception, which shall be nameless on such an occasion as this, I am not aware that any anatomist or surgeon of repute has controverted my views as expressed in the paper referred to. Nor need I do more than advert to the amiable and flattering device of a continental admirer bringing them all out anew, a few years after, as if they were his own. I have heard of nothing yet to impugn the anatomical explanation which I first gave of how various hap-hazard incisions might or might not facilitate the approximation and adhesion of the margins of the cleft in the soft palate. The knife for dividing the levator palati was my own device, and it was proposed at the same time, and soon subsequently applied, for the purpose of separating the soft palate from the hard in cases where there seemed a probability of closing a cleft in the hard palate, as well as the soft.

Those who have devoted attention to cleft palate during the last twenty years must have been surprised at a recent dispute as to priority in separating the soft from the hard palate with a view to close the cleft in the hard. Dr. Mason Warren describes this process in 1843; and it was referred to in my original paper. I myself performed it in January, 1845, and have since repeated it on all fitting occasions. The late Mr. Avery devoted special attention to this portion of the palate; and after his death the subject was further ably developed by Mr. Pollock in a paper published in the Medico-Chirurgical "Transactions" for 1856. The first idea that I had of this portion of the operation for cleft palate was obtained from Dr. Mason Warren. I deem it but fair to the reputation of that distinguished surgeon to state that I know of no originality before his, and that I look upon all modern claims to such originality as arising either from ignorance or a desire to rob the fair reputation of a name which, in son as in father, will stand for generations among the brightest in surgery.

Although working at the subject of cleft palate since the date of my first paper, I cannot pretend to add much that is novel to the views which were then expressed. I have little to add, little to detract, from the anatomy and physiology which I ventured to submit as original. I am still as much convinced that the tensor palati has little or no influence on the soft palate—certainly that it has none to counteract the closing of the cleft. The palato-glossus I am of opinion has no practical influence; and excepting in rare instances, I am convinced from experience that there is no necessity to interfere with the palato-pharyngeus in the posterior pillar. I am equally convinced of the value of dividing the levator palati, for that is the muscle which, by drawing upwards and outwards, separates the edges so as to prevent union or break it up when the stitches are removed.

There is ample experience to show that union has taken place, despite muscular action to the contrary. The experience of Roux and others has proved that. But I know of no experience equal to my own to prove what I contend for—namely, that by taking off muscular action for a time union can be rendered more certain than by leaving the muscles untouched.

Since I entertained the views referred to, I have operated on 134 cases, and of these 129 have been successful. In two union failed entirely, and in three it was so partial and imperfect that I place them as unsuccessful. Forty-five of them have been performed in hospital practice. In a considerable number an aperture has been left in the hard palate, and much benefit has been derived in many such cases from the use of an obturator. Occasionally there is a round aperture left, with such scanty material that it cannot be closed by operation; but in such slits as these, if the roof be wedge-shaped (not horizontal), the soft parts may be separated from the hard, and brought down so as to approximate in the middle, and be held together by *stitches until they unite.*

I have never attempted the operation in infancy, and consider the circum-

stances most favourable at or above puberty; but I have frequently operated successfully at ages between ten and fifteen, where patients have been steady and courageous. I have never operated under chloroform, and whilst I do not deny the possibility of doing so, I am of opinion that, as a rule, it is absolutely requisite to have the patient conscious, so that he may facilitate the steps in a variety of ways.

This enumeration includes all kinds of cases, and refers chiefly to the cleft in the soft palate. In many instances of cleft in the hard palate it is utterly impossible to contend against nature; and even in the soft, the parts are occasionally so scanty that there is literally no material to work upon.

In as far as I know, the greatest success recorded before my own views were made public was that achieved by Mütter, of Philadelphia. In 1843 he had operated successfully in nineteen out of twenty-one cases; and J. Mason Warren, of Boston, had been successful in thirteen out of fourteen cases. These instances were of both hard and soft palate. What may have been their after-success I cannot say. It has been related of Roux, since his death, that he had operated on 120 cases, and that of these one in every three had failed. I attribute Roux's comparative want of success to the circumstance that the levator palati and back part of the palato-pharyngeus were left untouched; and I consider that Warren's success may have resulted from the free incisions which he made through the palate outside the pillars of the fauces. My own success, if I may so call it, I attribute chiefly to the division of the levator palati, and next to the relaxation which the wound for that division involves. For mere relaxation, the incision of Dieffenbach is probably the most perfect. I know that it has been particularly successful in Mr. Skey's hands, and in Mr. Pollock's; but, with all deference, I am still disposed, from all I know of the subject, to prefer a free incision above the soft palate, whereby the levator palati my be divided to a certainty. In addition, I look upon this wound as of great service in this respect—the lymph effused upon it acts as a splint, whereby the palate is kept fixed as a board until union in the mesial line is complete.

As to attitudes in this operation, the patient may sit or lie, as may best suit convenience. Latterly, I have made most use of the recumbent. I find that the head can be kept best on the same line in this position; and as regards my own views on the anatomy and physiology of the parts concerned, I deem the subject of some importance. For instance, if the patient sits with the head slightly thrown backwards, the palato-pharyngei, when irritated, pull the soft palate downwards towards the epiglottis, so as to leave a space between the palate and the base of the cranium; but if the head be thrown far backwards the axis of action is altered, and these muscles draw the soft parts upwards, or, in other words, bring the soft palate towards the base of the cranium, and thus add to the difficulties of the surgeon by limiting the space above the soft palate where he has to work with the needles in introducing sutures. Here, as in hare-lip, the surgeon has generally stood before his patient, but I invariably select his right side in preference to all other places.

The grand practical object of this operation is to improve the voice and articulation. Defective deglutition from this malformation is what attracts the mother's or nurse's attention in early life. The cries of infancy are in nowise peculiar in tone; but when definite articulation commences, or rather should commence, the value of an entire palate is then appreciated. The air and sound, in passing outwards from the larynx, escape in part through the nostrils by the split in the palate. A nasal twang is the result, and articulation as in the normal state of the parts is impossible.

Immediately after the operation, the modification on the voice can be at once detected. It is customary to keep those operated on from speaking for eight or ten days. It is, however, a needless restriction as regards my operation. In reality, few care, under the circumstances, to speak at all; yet I do not think that it would do harm. In the course of eight or ten days, when the fever or distress *following* the operation has gone, the tone of the voice *is at once perceived to be changed* for the better. Improved articula-

tion, however, comes more slowly. Years, many years, are required for distinct articulation when the whole organs are to all appearance in perfection; and after the most successful operation for cleft palate, months and years are required to alter defective sounds. Voice and speech have to be modified anew. With some the changes come slowly and sluggishly; but with others they are so rapid and perfect that in a few years the original defect cannot be detected except by a practised ear.—*Lancet, June* 25, 1864, *p.* 722.

45.—ON HARD NODULATED TUMOUR OF THE TONGUE, APPARENTLY OF A CANCEROUS NATURE, WHICH DISAPPEARED UNDER USE OF THE GALIUM APERINUM.

By F. A. Bulley, Esq., Senior Surgeon to the Royal Berkshire Hospital, Reading.

Jane C., a married woman aged sixty, residing at Ramsbury, in Wiltshire, was admitted into the Hospital April 5, 1864, on account of a hard, firm, somewhat circumscribed tumour of about the size of a boy's marble flattened, imbedded in the substance of the tongue, on the right side, about an inch from its apex, which had been gradually increasing in size since she first observed it, five weeks before, when it was about as large as a hemp-seed.

The upper surface was nodulated and uneven, and the swelling generally had the appearance and feel of a scirrhous formation in the organ. It had all along been extremely painful, so much so as entirely to prevent her sleeping at night; it was exquisitely tender to the touch when handled, and latterly she experienced a throbbing beating pain in it, which had induced her to think it was about to burst. There was no appearance of its having been caused by injury to the tongue through a decayed tooth. She had always been in the habit of living tolerably well, but had been suffering a good deal from general debility and languor for some time before the commencement of the swelling. Her countenance did not indicate any peculiar cachectic condition of the system, and there was no history of any hereditary cancerous taint in her family. The tumour had increased rather rapidly lately, and she was quite unable to masticate solid food on account of the pain it induced, which had added much to her original weakness.

She was ordered to have strong cold beef-tea frequently during the day for diet, with a pint of porter daily, and to take the following medicine:—℞. Extract Galii aperini solidi, ʒij.; aquæ ad. ℥jss. M. ft. extract fluid. Of this extract a drachm and a half was given twice a day in a wineglassful of water. She was also ordered to use the above mixture as a warm lotion to the mouth several times during the day, keeping it in the mouth for some time during each application.

A month after her admission she had completely recovered from the languor and debility under which she had previously been suffering; her face, instead of being pallid and sallow, had recovered a healthy and somewhat florid appearance, which was natural to her; the pain in the tumour had been gradually diminishing, and the tumour itself had become so much reduced in size as to be scarcely discernible to the touch, and as she was now able to take solid food without discomfort and with an appetite, she was, at the end of five weeks, discharged from the Hospital. A fortnight afterwards, having continued the remedies prescribed, she presented herself as an out-patient, when it was found that the tumour had entirely disappeared, and the tongue had recovered its natural structure and appearanee.

The galium aperinum or cleavers has long been employed as a popular remedy in cancerous affections. Some years ago Dr. Wynn directed the attention of the Profession to the remarkable effects he had observed from its use in the treatment of some inveterate cutaneous affections which had come under his notice, and related several cases in the *Medical Times and Gazette*, which clearly showed that it was a remedy of considerable efficacy in such diseases; and more recently Dr. Ogle has published some interesting cases of epilepsy in which its employment had been followed by the most successful results. Like many other recent vegetable juices, it appears to have the

power of correcting that peculiar dyscrasy of the blood which is found to prevail more or less in all cases of cancerous disease, whatever form it may assume, acting, I suppose, in the same manner as the same kind of remedy seems to act in scurvy, by altering and improving the disintegrated and broken down condition of the blood which always accompanies that disease, and restoring it to its natural healthy state.

I have for many years past been in the habit of employing this remedy in the treatment of cancerous affections of different kinds in my hospital practice, and have not failed to observe that in some cases it has seemed to favor the production of healthy granulations on the ulcerated surface, whilst in others complete cicatrisation has ensued, and having performed operations for the extirpation of some tumours under these favorable circumstances, I have reason to believe that in these instances the disease has not returned, at least to my knowledge, the patient having, of course, persevered in the use of the remedy for a considerable time after the operation, and although I do not consider that the remedy is capable in every case of effecting the resorption or removal of already existing cancerous deposits, I still believe, from what I have observed, that it has the power of suspending, or at least modifying, in some measure, the cancerous action going on in the system, and thus placing the patient in a more favorable position for the performance of an operation for the removal of the local disease, either by the knife or the application of caustic. It appears to me to be quite contrary to reason to expect that an operation can be certainly, or even commonly successful, when performed while the cancerous action is proceeding unchecked in the system; and as, according to our present knowledge on the subject, we are not in possession of any certain means of arresting its constitutional progress, it is not surprising that the disease should so frequently return, either in the part itself, in the neighboring glands, or in some organ at a distance predisposed by some accidental irritation to become the seat of a cancerous deposit.

That Nature occasionally exercises a power of arresting or suspending the cancerous action in the system for a very long period, and even altogether, the records of Surgery fully prove, and it is not too much to hope that sooner or later some remedy may be discovered which may be capable of imitating the operations of Nature in this respect, and that thus a class of cases hitherto deemed to be almost beyond the resources of our art, may ultimately become amenable to some rational mode of constitutional treatment.—*Medical Times and Gazette, July* 30, 1864, *p.* 117.

46.—ON ENLARGED TONSILS, AND THEIR TREATMENT WITHOUT CUTTING.

By Dr. Morell Mackenzie, London.

The effete practice of applying caustics, alteratives, or absorbents, to the enlarged glands has been abandoned, and a positive effect has been accomplished by the aid of escharotics.

The "London Paste" which I employ (consisting of caustic soda and lime, moistened with a little alcohol) is made for me by Messrs. Bullock and Reynolds, of Hanover-street, Hanover-square. In preparing it, it is extremely important to employ absolute alcohol. If spirits of wine be used instead, the bulk of the paste is very much increased, and its causticity proportionably diminished. In addition to this, the water in the spirits of wine causes the paste to dry up very quickly, and makes it necessary to moisten it with fresh spirit every time it is employed. The mixture of caustic soda and lime has a powerful affinity for carbonic acid, and if exposed, it rapidly absorbs it from the air, and loses its causticity. The paste should therefore be carefully kept in a stoppered bottle.

The compounds of the caustic alkalies and unslacked lime possess a power of destroying animal tissues which is unequalled by any substance that can be safely applied at *the back of the throat.* I must remind those of you,

however, who have been in the habit of employing Vienna Paste for surgical purposes, that whilst to make an issue it is necessary to keep the paste applied for ten or twenty minutes, neither it nor the London Paste *must remain in contact with the hypertrophied tonsil for more than five seconds.* The London Paste, as compared with the potash caustic, possesses, according to my observations, the following advantages:—

1. It tends to penetrate, rather than to spread circumferentially.
2. Its action, though less violent at the moment of application, continues for a much longer time.
3. It causes much less pain.

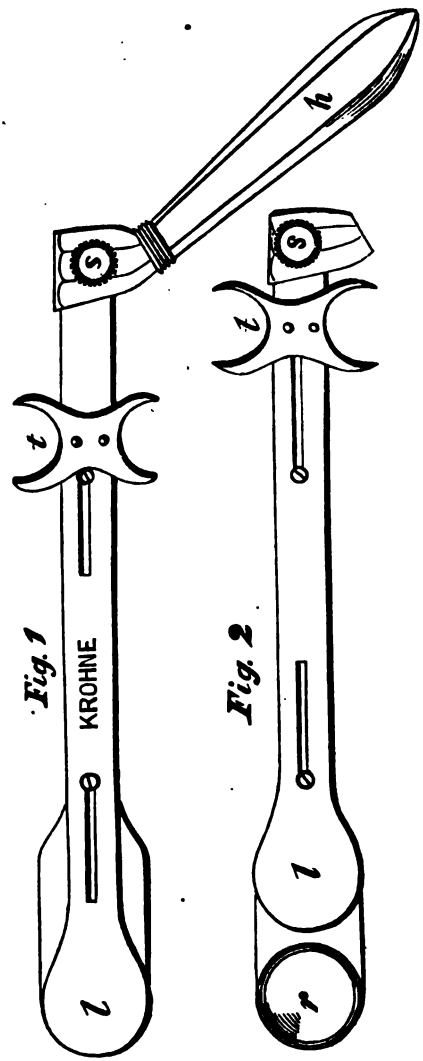


Fig. 1.

Fig. 2.

Fig. 1.—The instrument closed.
Fig. 2.—The instrument open.
(t.) Trigger by which the lid (l) is moved backwards and forwards.
(r.) Receiver into which the paste is put.
(l.) Lid, covering the receiver in Fig. 1, and drawn back in Fig. 2.
(s.) Screw, by which the horizontal portion of the instrument is fixed into the handle (h).
By this arrangement the instrument can be used for either tonsil.

For applying the London Paste to the tonsil, and for preventing its accidental contact with parts not meant to be touched, I have had a little instrument contrived which answers its purpose well. You see that it somewhat *resembles the guillotine* of Mr. Luke, though instead of a steel ring with a

sharp blade behind it, there is a kind of circular shallow spoon or receiver, into which the escharotic is put; the receiver is provided with a lid or cover, which, when the instrument is grasped in the hand, can be pushed forwards or backwards by the action of the index finger on a kind of trigger, or half-ring placed near the handle. The instrument is introduced closed, and when opposite the tonsil the lid is drawn back from the receiver, and the latter with its contained paste is pressed firmly against the hypertrophied gland for about five seconds. The lid is then pushed forwards over the receiver, and the instrument withdrawn. You see that the horizontal portion of the instrument is made of vulcanite—a substance which is not materially acted upon by London Paste. I call your attention also to the fact that the horizontal portion of the instrument is fixed into the handle by a screw in such a way that it can be reversed, and thus used for either the right or left tonsil. In employing the instrument, I generally introduce it with my right or left hand, according as I wish to operate on the patient's left or right tonsil; whilst with my other hand I press the receiver firmly against the enlarged gland. I also frequently employ the free hand for pulling the trigger backwards or pushing it forwards, that is to say, for uncovering or covering the paste. For operating on children it will be found convenient to employ a smaller instrument. The same handle will of course do for two or three "receivers" of different shapes and sizes.

The instrument I have described is made by Mr. Krohne, of Whitechapel Road. It renders the employment of a strong semi-liquid escharotic easy and safe. Whilst recommending its employment to others, I must confess that it is only quite lately that I have made use of it myself. In more than twenty cases, I applied the paste with a small glass rod, or the wooden stick of a pen-holder. The following was my mode of procedure. The paste was first prepared. Care was exercised that it should be neither too thin nor too thick. If too thin, it is apt to run about and burn parts which should not be touched. On the other hand, if the paste is lumpy, there is the risk of fragments being swallowed. The glass rod was then dipped into the paste, great care being taken that the latter did not cover more than an inch of the rod. The patient was then placed in the position for laryngoscopy, and the fauces illuminated. In applying the paste it will be found very convenient to throw a strong light into the throat, with the large reflecting mirror of the laryngoscope. This is not absolutely necessary, but the difference between the dull day-light—partially obstructed by the operator's head and hands—and the brilliant illumination obtained by reflected light, is so great, that the mirror has only to be employed once to ensure its always being used afterwards. Still those who are not accustomed to use the laryngoscope may safely apply the paste without artificial illumination. In this case the operator must choose a fine day, and should sit close to the window, with his back to the light, whilst the patient sits facing him.

To continue: The patient being ready, he was directed to open his mouth widely, and whilst with one or two fingers of my left hand, I pressed the tongue down firmly, with my right hand I applied the paste.

Directly the tonsil is touched with the paste a straining movement takes place in the throat and the tonsil is violently pushed out towards the median line. In this way, a large surface of the gland is exposed, and the paste can be applied with great facility. I stated just now that it was convenient to use a smaller instrument for operating on children than on adults. But it is so impossible to explain the object of an instrument to frightened children, that in destroying their tonsils, I generally apply the paste with the glass rod in the manner described. With reflected light at one's command, it is really astonishing how easily and how quickly operations at the back of the throat can be conducted, even on unwilling subjects. In employing the instrument which I have shown you to-day, as the paste is concealed, it is not necessary to press down the tongue with the forefinger of the left hand.

In making use of either London or Vienna Paste, it is convenient to have a little vinegar at hand, so that if the back of the tongue or any other part has been accidentally touched, the caustic action can be at once arrested.

To avail oneself of the neutralising property of vinegar however, it must be applied *immediately*. If its application is deferred, it only irritates the already inflamed part.

The application of the paste for a few seconds causes more or less pain. Some patients complain very much; others do not seem to think much of it. The pain lasts from five to ten minutes. Afterwards there is a feeling of a sore throat, and some discomfort in swallowing. This generally continues for two or three days. On examining the tonsils shortly after the application of the paste the mucous membrane appears of a bright red colour; streaks and patches of dark blackish blood, and white shreds of altered membrane, are seen at the back of the mouth. The next day, the tonsil is covered with a white eschar. The paste should be again applied after an interval of three or four days. The subsequent employment of the escharotic is generally attended with less pain than its first application. The largest tonsils can generally be reduced to a normal size in about a month. The paste should be applied about twice a week. The London Paste not only actually destroys a certain portion of the tissue, but it greatly weakens the vitality of the subjacent tissues. The cohesion of the gland is impaired, and it becomes soft, and very friable. In proof of this, I may observe that after applying the paste once or twice, it is easy to push a glass rod into and even through the gland.

Dr. Fournié has employed the Vienna Paste for destroying the elongated uvula, and I have used both it and the London Paste for that purpose. The application of escharotics, however, to the uvula is difficult and unsatisfactory. The extreme mobility of the uvula makes it impossible to apply the paste firmly and steadily, and hence it often happens that the upper part of the uvula is partially destroyed, whilst lower down it is little affected. Besides which, the application of the escharotic to the uvula causes much more pain than when the tonsils are destroyed in the same way. Though I have thus been able to get rid of the uvula in one or two cases, its removal with the knife is so easy and safe an operation, and attended with so little pain or hemorrhage, that it is certainly the preferable mode of treatment. I do not recommend therefore the use of escharotics for destroying the hypertrophied uvula.—*Med. Mirror, September* 1864, *p.* 522.

47.—GLYCERINE AS A VEHICLE FOR APPLICATIONS TO THE THROAT AND LARYNX.

By Dr. George Johnson, Physician to King's College Hospital.

Glycerine is a very convenient and useful vehicle for various local applications to the larynx. It is an excellent solvent for tannic acid. Tannic acid may be dissolved in glycerine in the proportion of two drachms to an ounce. This is a very useful topical astringent. Glycerine will also, by the aid of heat, dissolve as much as one-fifth of its weight of hydrochlorate of morphia. We seldom require so strong a solution as this, but a moderately strong solution of morphia in glycerine is a most useful soothing application in cases of irritable larynx. Glycerine as a vehicle for other remedies, has this advantage, that being viscid, it adheres to the surface of the mucous membrane, and retains there the astringent or the sedative which it holds in solution. On this account it may very usefully be mixed with a solution of nitrate of silver. A mixture of glycerine with a solution of perchloride of iron is a useful topical astringent. Glycerine may also be made the vehicle for *alum*. Whenever glycerine is applied to the larynx it must be done by means of a brush; it is too thick a liquid to pass through a syringe.—*Lancet, July* 30, 1864, *p.* 114.

48.—ON A SIMPLIFICATION OF THE EXTRA PERITONEAL OPERATION IN THE SEVERAL VARIETIES OF STRANGULATED HERNIA.

By Furneaux Jordan, Esq., Surgeon to the Queen's Hospital, Senior Surgeon to the Birmingham Eye and Ear Hospital.

It may be said compendiously that there are two classes of cases of strangulated hernia which require operative relief. In one the extra-peritoneal operation offers the greatest prospect of success; in the other it is necessary to open the sac. In the second class may be placed, 1, cases of gangrene, and, 2, cases that cannot be returned after the extra-peritoneal incision. All other cases may be placed under the first category. It is rare in Surgery to meet with so clear and unmistakable a rule of practice. In every case of strangulated hernia, except where gangrene is present, the Surgeon should first adopt the simplest method of relaxing the fibrous structures which strangle the protrusion, and apply the gentlest possible taxis. In the few cases in which this proceeding fails, a second stricture having been sought for (especially in inguinal hernia), but not found, the sac may be opened in the usual way.

My experience coincides with that of numerous observers to the effect that the presence of gangrenous bowel may be very certainly diagnosed. When inflammation reaches the coverings of the sac, as indicated by redness of the skin, infiltration (œdema) of the subcutaneous tissue, and loss of distinct definition of the tumour, with complete cessation of pain, the bowel is already sphacelated. Constitutional symptoms lend confirmation. Flickering pulse, physical and mental prostration, and the other phenomena of collapse are present.

Mrs. J., aged sixty, had constipation five days, and vomiting, which had been stercoraceous for several days. Unfortunately, the hernia was not discovered until too late. At the request of the Medical attendant I saw the case. I found in the right groin a flattened, hard, red swelling, extending from the pubes to the ilium, and one inch above and two inches below Poupart's ligament. The collapse was marked. Incisions disclosed black shapeless bowel. There were no fæces in the sac, but a copious flow followed division of the margin of Gimbernat's ligament. I gave a most unfavourable prognosis; she sank next day.

I would emphatically observe here that if there be a condition of the bowel, which, beyond all others, is irreparably injured by exposure and handling, it is where inflammation is far advanced and gangrene is imminent, without being actually present.

In irreducible hernia there is now no difference of opinion as to the impropriety of opening the sac. In these cases strangulation occurs not from constriction of the neck of the sac, but from the protrusion of additional bowel. Opening the sac in a large irreducible hernia is one of the most fatal of surgical proceedings, and the more direct the communication with the abdomen the greater the fatality, as in cases of irreducible umbilical hernia. Here to practise the simplest means of securing the return of the recent protrusion should be our highest aim.

In cases which are reducible prior to strangulation it is difficult to conceive that strangulation can occur in a certain number of hours from changes in the neck or contents of the sac. To conceive this we are required to believe that in an inflammation of a few hours' duration, developed under circumstances antagonistic to formative results, adhesions are formed so firmly as to be incapable of rupture on the application of the taxis subsequently to the relaxation of the firmer extraperitoneal structures. It is a significant fact (I do not claim for it more than it is worth) that strangulation never occurs in those rarer forms of hernia which pass through openings, devoid of fibrous margins, although such herniæ have sacs and the sacs have necks. Practically we find that cases (perfectly reducible prior to strangulation) in which reduction cannot be effected after the firmer structures cease to constrict are remarkably rare.

The following cases show that after many days of strangulation genuine adhesions do not form:—Mrs. F., aged thirty-eight, brought into Queen's Hospital five days after occurrence of strangulation, (and eight after parturition). Sac distended with dark fluid, and bowel claret coloured, but no adhesions or gangrene. Recovered rapidly after division of stricture. I was requested by a practitioner in the country to see Mrs. C., with similar phenomena and result. Another practitioner in the country called me to see M. H. with strangulated inguinal hernia of six days' duration. The bowel was sticky from inflammatory products, but no real adhesions were present. The patient died. These cases occurred before I performed the operation to be described. I believe, from cases in which I have myself operated, that in many instances where irreducibility continues after division of the less yielding tissues, it is the irreducibility of a small portion of the contents which has not been observed, but has existed before strangulation. Fresh protrusion, a noticeable tumour, and strangulation occur together.

When cases of strangulated (reducible) hernia occur which actually require the extra-peritoneal to be converted into the peritoneal operation, one, two, or three conditions will be found—constriction of the neck of the sac, adhesions within the sac, or increased size of the contents of the sac. Each condition is produced by processes occurring subsequently to strangulation.

I have so far spoken of the constricting agency as lying either in the non-distensible structures which surround the several outlets at which herniæ occur, or in the neck of the sac itself. The crural sheath does not constrict in femoral hernia, or the fascia transversalis (except at the inner ring as a special structure) in inguinal hernia; the fascia transversalis is never the seat of strangulation in umbilical hernia; the subperitoneal fatty fascia is never a constricting agent in any variety of hernia. All these tissues are, of course, compressed into a small space within the several fibrous outlets, and, after death, such compressed tissues present considerable rigidity; during life, however, they are not themselves compressing agents. Why, then, should these tissues be divided if the really compressing structures lie externally to them.

It is the object of this paper to show that all that is necessary in practice is to relieve the tension of Gimbernat's ligament in femoral hernia, of the conjoined tendon or fibrous apertures in inguinal hernia, and of the linea alba in umbilical and the more common forms of ventral hernia. As a rule, these structures can be reached and relaxed, the cutaneous tissues only being divided prior to the use of the hernia knife.

The great excellence of Mr. Gay's, contrasted with the previous extra-peritoneal, operation consisted in this:—The incision was much more limited, and was made at a distance from the tumour. In Mr. Gay's operation, however, the subperitoneal tissue was reached both in the operations for femoral and for the other varieties of hernia; it was, nevertheless, an immense stride in the progress of hernial surgery.

I have now frequently in cases of strangulated hernia, especially in femoral, the variety which most commonly requires operative relief, performed, with strikingly satisfactory results (especially in the extreme shortness of the convalescence), an operation of which the following are the chief characters, and which, since it aims at keeping as far as possible from the peritoneal structures, I would suggest might be termed the pretaxoid operation. A short incision is made through the skin and superficial fasciæ near the neck of the tumour, and sufficiently large to admit the end of the finger. The superficial fasciæ are divided (not necessarily completely) to an extent which will enable the finger nail to detect the interstice or boundary line between the firm, immovable, unyielding apertural margin and the more movable and yielding tunics of the hernia. For example, in femoral hernia it is not necessary to cut through every flake of connective tissue down to Gimbernat's ligament. It is enough if the finger nail discovers with ease and certainty the margin of that structure. As the hernia knife (blunt pointed, but cutting to the point) is insinuated along the finger nail, close to the firm *constricting margin*, it may advantageously carry before it a little connective

tissue; thus all tissues are divided to the least possible extent, and the knife is much less liable to perforate the peritoneum. The practical rule, then, in all cases of hernia is to divide the skin and just enough of the subcutaneous connective tissue to permit the tense margin to be felt which it is proposed to relax. Anatomical knowledge is applied here not to determine how much tissue shall be divided—that is best decided by the touch—but to determine the precise direction in which and the precise point to which the finger nail is to be carried. I regard it as an important feature of the pretaxoid operation that a lamina of tissue (crural sheath, fascia transversalis, &c.) shall, if possible, come between the peritoneal structures and the hernia knife. The herniatome is next used in the most sparing manner, not only in the very limited application of the edge, but, which is even more important, in the limited introduction of the point, for the peritoneal structures are stretched thin and closely applied to the abdominal aspect of the apertural structures. In few cases should the herniatome pass beyond the finger nail, and in umbilical hernia not at all. Incision of the tense margin is followed by pressure with the nail and tip of the finger in the direction in which the incision was made. Pressure must on no account be made towards the abdominal cavity; neither fingers, directors, nor knives should be carried in this direction. If the finger be passed into the abdomen before the margin is relaxed there is no strangulation, and no operation should have been performed; if strangulation be present, the introduction of the finger after the limited use the herniatome is more likely to injure, and seriously injure, the bowel and peritoneum than to effect any useful dilatation. Gentle and momentary application of the taxis to the unwounded coverings completes the operation.

It is not inopportune to remark here parenthetically, that certain points in the anatomy of hernia, even at the present time, give rise to great difference of opinion. These differences of opinion disclose on both sides the adhesion to a school of anatomy which is fast becoming antiquated, if not obsolete. The philosophic teachings of Reichert—in the main accepted by our most reputed histologists—applied, as they assuredly ought to be, to Surgical anatomy, show most clearly that, say in femoral hernia, there are, as separate entities, no ligaments, no sheaths, no fasciæ, no tendons, but there is one single indivisible mass of connective tissue, firm here and loose there, having one disposition here and another there, and being directly continuous with the skin in one direction and with the bone or an epithelial surface in another. With these firmer and looser tissues their forms and localities, it behoves the practical Surgeon to make himself—there is only way to do it—minutely familiar. It is perfectly idle to dispute whether the femoral sheath is a prolongation of the transversalis and iliac fasciæ, on the one hand, or the fascia lata on the other, or whether the so-called saphenous opening does or does not exist, seeing that the femoral sheath, the fasciæ lata, the cribriform, iliac, and transversalis fasciæ, Poupart's and Gimbernat's ligaments, and the aponeurosis of the external oblique are all one tissue—a tissue without attachments, without junctions, without lines of demarcation.

There is one fact in modern histological anatomy of immense importance in relation to Surgical pathology. A basement membrane has no existence. The epithelium of surfaces is placed immediately on connective tissue, which is very dense underneath the epithelium, but becomes gradually less dense as it passes deeper. Now, in inflammation of all surface membranes, it is this less dense connective tissue which is the seat of the greatest pathological activity. Erysipelas, bronchitis, pleurisy, and peritonitis are illustrations of this remark. It is one of the principal objects I have in view in the pretaxis operation to avoid injury to that inflammable portion of the peritoneum which is called subperitoneal fatty fascia.—*Medical Times and Gazette, June* 11, 1864, *p.* 641.

49.—ON THE EXTRA PERITONEAL OPERATION FOR STRANGULATED HERNIA.

By Fuenbaux Jordan, Esq., Surgeon to Queen's Hospital, and Senior Surgeon to the Birmingham Eye and Ear Hospital.

[After some preliminary remarks, Mr. Jordan describes a few details of the operation as applied to the several varieties of strangulated hernia.]

In every variety the superficial incision is best made on that side of (never over) the neck of the hernia at which it is intended to make the deeper incision. It may be made in any direction, vertical or transverse, as its object is simply to admit the finger; perhaps more freedom of movement for the finger with greater distance from the tumour is obtained by making it a right angle to the deeper incision. Hence, in femoral hernia, it will be vertical to the inner side of the neck of the sac; in inguinal, umbilical, and ventral hernia, it will be transverse and above the neck of the sac. In each case it may be made about an inch from the neck of the sac. In femoral hernia, the incision is made over the pectineus muscle, which lies at some distance from the surface, and is covered by a large quantity of connective tissue in firmer and looser strata, which may with sufficient accuracy be called by their well-known names. This superabundant tissue may be divided downwards towards the pectineus until the finger can be carried outwards, upwards, and backwards to the edge of Gimbernat's ligament. The hernia knife is carried along the finger nail, and a line or two (according to the amount of tissue carried before the knife) beyond; the edge is turned to Gimbernat's ligament, and a very sparing incision made transversely inwards. The tip of the finger now makes firm pressure towards the spine of the pubes. The thigh being flexed and thoroughly rotated inwards, gentle and momentary taxis (it should be nothing more) effects reduction of the tumour. If the finger nail cannot be carried easily to the margin of the ring, the fasciæ have not been sufficiently divided, or, in quite rare cases, the upper horn of the saphenous opening, instead of being blended with Gimbernat's ligament, may be brought lower down. In either case incisions carried down more completely to the pectineus will remove all difficulty. Gentle pressure outwards on the movable sac will always throw much light on the precise locality of the immovable constricting margin.

I was requested by a Medical Practitioner in Birmingham to see Mrs. H., aged thirty, with strangulated femoral hernia of thirty hours, which, under the complete influence of chloroform, resisted all justifiable use of the taxis. I carried the point of the hernia knife by an almost subcutaneous incision to the margin of Gimbernat's ligament. Pressure on the incision, followed by momentary application of the taxis, effected reduction of the tumour. The next day the patient laughed and chatted with her friends, and was kept in bed with difficulty. I was called by a Surgeon in the country to see Mrs. R., aged forty-seven, with strangulated femoral hernia of two days' duration. There was extreme depression, but no sign of inflammation in or around the sac. I performed the operation I have described, and a rapid and excellent recovery followed. The two cases just recorded are simply illustrations of a very frequent form of hernia. The next case is more unusual:—Mrs. D., aged eighty, was admitted into the Queen's Hospital under my care. Collapse was so extreme that she was supposed to be dying. I performed the simple operation described above, and made pressure on the incision. On applying the taxis there was immediate reduction of almost the whole of the tumour. The sac, however, was not completely empty, and I consequently extended the incision over the coverings and opened the sac, which contained a small portion of omentum fastened to the sac by one old small adhesion. She never rallied from the shock. After death there was no trace of inflammation anywhere.

In inguinal hernia, the careful division of the superficial fasciæ will enable the finger to pass along the neck of the tumour to the seat of strangulation, *when a limited* incision upwards, pressure upwards, flexion of the thigh, and

taxis will mostly effect the desired object. The operation is especially applicable to umbilical hernia, as division of the cutaneous structures brings the finger so easily to the margin of the opening. It is most imperative here to carry the hernia knife as far as the nail only; where the margin of the linea alba projects after the manner of a short tube (as in a large irreducible strangulated case which occurred to me recently, in which convalescence followed the pretaxoid operation, but which ultimately terminated fatally in consequence of the grossest imprudence), the nail must precede the point of the knife step by step. The linea alba is thin; the fascia transversalis is extremely thin; the subperitoneal tissue is scanty, and, with the peritoneum, is pressed close to the margin of the outlet. In irreducible umbilical hernia it is of immense importance not to open these thinner structures except the linea alba.

All the advantages of the extra-peritoneal operation may be, with still greater emphasis, claimed for the simpler form of operation. There is less danger of hemorrhage, and should it occur it will be external and visible. Should I meet with an abnormal distribution of the obturator artery, and accidentally wound it (an accident less probable in this operation than in any other), I should arrest hemorrhage with wire pressure applied at the proximal side of the arterial wound by means of a curved needle. The bowel is not exposed, handled, or wounded either below or above the stricture. (See James, of Exeter, on the liability to the latter accident in the ordinary operation, even in the most skilful hands.) Inflammation, ulceration, and incipient (speckled) gangrene are repaired more rapidly and more completely in the subcutaneous condition in which the abdominal organs are naturally placed.

If the operation I have described be performed with extreme promptitude in cases of reducible strangulated hernia, it is, *per se*, so free from danger it is impossible that the result should be unsuccessful, save in exceptional cases where the shock of the strangulation itself is sufficient to cause death, as in extreme old age, or in cardiac or pulmonary disease. I willingly grant that earliness is even more important than the mode of operating. Nevertheless, other things being equal, both experience and statistics show that the operation of not opening the sac is incomparably superior to the older method. I find in Mr. Bryant's excellent clinical memoir on the experience of eight years of strangulated hernia at Guy's Hospital, that in the cases which died after the extra-peritoneal operation during the period referred to, not in a single case could the cause of death have been obviated by opening the sac. It is to be feared that the older and severer operation of opening the sac indiscriminately is still so frequently resorted to because of the ease with which it is performed. To cut down until the bowel is found, to discover the hole through which it escaped, to cut that hole larger with a knife, and thrust the intestine back into the belly, requires no anatomy, and but moderate caution.

Perhaps the greatest changes in the modern Surgery of hernia are seen in the "prior" and "after" treatment of strangulated hernia. In the prior treatment there is but one proceeding which deserves the confidence and the expenditure of the time of the Surgeon—namely, the complete administration of chloroform, and under its influence only, the application of the taxis for a very few minutes if the hernia be femoral and small (in certain cases not at all), a few more if inguinal and large. Before chloroform is given the Surgeon should be prepared, if necessary, to adopt operative measures. Bleeding, antimony, tobacco, the warm bath, prolonged taxis, and taxis without chloroform, are now, or ought to be, of the past. Ice locally, and opium internally, are justifiable only in exceptional circumstances, as when a House-surgeon waits for a Surgeon, or one Surgeon waits for another. The after treatment is equally clear. Rest to the contused, exhausted, and inflamed bowel must be secured by means of opium, and the persistent avoidance of purgatives by the mouth. After many days, if the bowels do not act, one or two enemata daily will in time bring about the desired result.

In conclusion, I would *again urge the* proposition that the simplest form of

extra-peritoneal operation is desirable in all cases where reduction by the taxis is desirable, but cannot be effected. Too frequently Surgeons apply vigorous and prolonged taxis, and, if it fails, immediately proceed to the severer operation of opening the sac. Now, either it is improper to apply the taxis, or it is improper to open the sac before resorting to a simple measure which renders the taxis successful.—*Medical Times and Gazette, July* 2, 1864, *p.* 5.

50.—ON THE CAUSES OF HERNIA.

By J. A. Kingdon, Esq., Surgeon to the City of London Truss Society.

[This paper is an abstract of one read before the Royal Medical and Chirurgical Society.]

The object of the paper was to call in question the accuracy of the prevailing opinions concerning the causes of hernia. Instead of its being due almost entirely to mechanical causes, as most writers since the time of Scarpa have held, the author endeavoured to prove that hernia was the result of an abnormal condition of the peritoneum in its entirety, either from congenital malformation or inherent disposition to be relaxed. Without disputing the majority of the arguments that favoured the mechanical theory, the author attempted to show that mechanical causes could not operate without antecedent derangement of the peritoneum. So long as the mesenteries remained unrelaxed, in healthy tone, and with their normal attachments, the author held that hernia not only did not but could not occur, and that there was no disposable force that could stretch the mesenteries and dislodge the viscera. But when, from congenital defect or subsequent derangement, the mesenteries allowed the intestines to descend in the cavity of the abdomen below their proper sphere, then mechanical causes could act—then the arguments of those who advocated the mechanical theory would apply; protrusion would then be due to loss of equilibrium between the muscular parietes and the rings, but not till then. The author attempted to show that hernia was fundamentally an affection of the peritoneum generally, by pointing out that the condition of the parietal layer determined the kind of hernia—i. e., the aperture through which it escaped. The facts and arguments in support of these views were set forth in the paper.—*Lancet, July* 23, 1864, *p.* 95.

51.—PERSULPHATE OF IRON IN HEMORRHOIDS.

Dr. George S. Cartwright, Assistant Surgeon, U. S. V., highly extols (Cincinnati Lancet and Observer, May, 1864) the efficacy of the persulphate of iron employed as an ointment in the treatment of hemorrhoids. It is especially beneficial, he states, in ulcerated hemorrhoids, or in those whose constitutions are debilitated from diarrhœa, long marches, and excessive fatigue of any kind.

Of several cases which he relates, illustrative of the advantages of this remedy, we select the following:—

"Major ——, U. S. A., of full habit, has been the subject of slight hemorrhoids for several years. For the last twelve months has been obliged to travel a great part of the time in a rough vehicle. Applied to me December 5th, 1863. On examination found a small tumour, external to the sphincter, about the size of a large pea; when at stool, it would protrude to the size of a small walnut, and would with difficulty be returned.

"*Treatment.*—Lead water freely applied to the part, and ℞. ferri persulphas ℥ss., cerate simplex ʒj. Rub well together, and apply on retiring at night. The effect of the persulphas was almost immediate, relieving pain and cauterising the part.

"*I would state that* he had previously used ointment of galls, tannin, opium, *&c., with only temporary* relief. The effect of the persulphas is permanent,

and in the above case he was able to ride on horseback, or take active exercise, within two weeks after commencing the use of iron, without the least inconvenience. It is now two months since he first commenced the use of it, and has not had any return since."

Dr. C. sometimes employs the ointment with double the proportion of the persulphate used in this case.—*American Journal of Medical Sciences, July*, 1864, *p.* 278.

ORGANS OF URINE AND GENERATION.

52.—ON LITHOTOMY IN CHILDREN, AND ON LITHOTRITY.

By William Fergusson, Esq., F.R.S., Surgeon Extraordinary to H. M. the Queen.

[Mr. Fergusson does not consider that a "wholesome idea" of the operation of lithotomy in young children exists at the present time. Certainly, when satisfactorily accomplished, the operation is almost certainly successful; but we often hear of difficulties and great mishaps, which, however, are seldom made public. We occasionally hear of an operation being performed and no stone found, although it had been previously detected by sounding. In such a case the bladder has never been reached at all.]

After many operations on the adult and on the young subject, I had in a manner forgotten my early knowledge of the position of the bladder in children, and not only was content to make the incisions with a simple scalpel, but had in a measure got careless about some matters of great importance. On the 17th March, 1849, I had to operate on a boy four years of age, at King's College Hospital. I used a scalpel, as I had often done before, and made the ordinary incisions for lateral lithotomy. A grooved staff with a large curve was the director into the bladder. In making the deepest part of the incision, I purposely used the cutting instrument as lightly as possible, with a view to open only a part of the membranous portion of the urethra, and notch the prostate and neck of the bladder. These objects being effected, the point of the forefinger of my left hand was, as usual, placed on the staff, and pushed gently towards the bladder. The finger went on, but I was aware that it had not got between the urethra and the staff. With an insinuating movement (much to be appreciated by the lithotomist who, as I do, professedly makes a small incision in this locality), I endeavoured and hoped to get its point as usual into the urethra and neck of the bladder. But here I felt conscious that I had failed. I was aware that the finger was getting deeper as regarded the depth of the perineum, but that I was not materially nearer the bladder. I could feel a considerable space at the point of my finger, and was convinced that the upper part of the membranous portion of the urethra, as well as the sides above the wound, had given way to the pressure of the point of the finger, and that now, as the latter was getting deeper into the wound, I was only pushing the prostate gland and neck of the bladder inwards and upwards. These parts seemed to recede before the smallest imaginable force, whilst I felt that I could in a manner make any amount of space around the bare part of the staff. I had no difficulty in distinguishing between the surface of this space and that of the mucous membrane of the bladder. Moreover, I knew that I had never crossed that narrow neck which is always felt as the finger passes into the bladder when a limited incision is made. An impression came over me that I was about to fail in getting into the bladder, and I had the idea that unless I could open the urethra just in front of the prostate more freely, I should possibly never reach the stone. Any additional use of the forefinger of the left hand only endangered the further separation of the prostate and neck of the bladder from the pubes, and I was conscious that the only safety lay in cutting a little more freely on the groove of the staff. This I effected with great caution, and then I could appreciate the passage of the finger as usual through the prostate and neck of the bladder. The stone was thereafter easily touched and removed; but when all was fin-

ished, I was forcibly impressed with the idea that I had nearly failed in the performance of the operation. An onlooker might not have been able to perceive the cause of this emergency, but I was myself conscious that I had not reached the bladder, even at a time when the finger seemed deep in the perineum. Happily, the patient recovered, though slowly, in consequence of the lacerated condition of the wound and the formation of an abscess in the left testicle.

Until this date I confess I had never fully appreciated this danger and difficulty in lithotomy in young subjects. I had read of the slipping of the gorget in this operation, and become acquainted with the fact that futile incisions had often been made, and with the supposed mistake of the surgeon in cutting when no stone was present; but now a new idea flashed on my mind, and from that time I have never lost sight of it. I have never performed lithotomy on children in public without referring to it. I have observed, since that time, that the subject has been alluded to by certain surgical authors, but I am not aware that it had ever been specially noticed before.

From all my experience I feel justified in stating my conviction that most of the cases heretofore related as instances where the incisions for lithotomy have been made and a stone has not been present, have been examples where the surgeon has failed to reach the bladder from the cause just narrated. Since I have been impressed with this view I have known of cases where, death having followed the incisions, the stone has been found in the bladder at a post-mortem inspection; and I have also heard of others where the stone has been successfully extracted at a second operation, after the first wound has been allowed to heal.

The mishap is much more likely to occur than most surgeons imagine, and my opinion is founded upon the following grounds:—The size of the wound is necessarily limited, so that the forefinger of the left hand in a manner fills it. The perineum is much deeper in proportion in the child than in the adult, and, in addition, all the material is loose, lax, and loaded with fat. The circumstance that the bladder is more abdominal than pelvic in early life has been greatly overlooked. The slender tissue of the membranous portion of the urethra, the narrowness of the tube (both contributing to the facility with which the circumference may be torn through), and the small size of the prostate (rendering its discrimination difficult), all constitute peculiarities which are not conspicuous in the adult. In the latter there are room, development, bulk, mass, and a final wall of prostate and bladder, which may be all said to be deficient in the child.

From these data I have long since come to the conclusion, and have long taught in my lectures, that lithotomy in children, whilst comparatively free from danger as regards the final result, is by no means so free from difficulty or the risk of failure. The safety of result has been mistaken for simplicity of execution; but I hope that what I have now stated may be a warning to the young lithotomist. It may naturally be asked how the danger referred to may be avoided. My answer is, that more care than is usually given should be devoted to the operation, and that as the surgeon cuts into the membranous portion of the urethra and neck of the bladder he should never push the point of his forefinger onwards, unless he feels certain that he has it between the staff and the wound.

To show that I do not now speak without a fair share of experience, I take the liberty of stating that of one hundred and fifty-nine patients on whom I have performed lithotomy, fifty have been under the age of puberty—that is, under fifteen. Experience, instead of diminishing my anxiety on such occasions, has rather increased it; for as numbers have enlarged, I have been more and more impressed with the views above given. Of the fifty cases I have lost two—one was the third child I operated on, the other was the forty-eighth; so that of the whole number, I cut consecutively forty-four without losing a case, or forty-seven losing only one. Of the two lost, one died on the twelfth day after, from unhealthy inflammation; the other on the second *day, from hemorrhage and* shock, the bleeding being probably the principal

cause. Six of these were done in private practice, the rest in public—one (a fatal case) in the Royal Infirmary of Edinburgh, the others (forty-three in number) in King's College Hospital. I am not aware of such a list having been published before; and to myself, as I believe to the profession generally, it would be interesting to hear the results of the practice of those who have had greater experience than I pretend to in lithotomy in children.

[Mr. Fergusson has now treated between 250 and 300 cases of stone in the bladder by lithotomy and lithotrity.]

In a large majority of my own early cases, I was content with crushing the stone, and waiting for the gradual and spontaneous egress of the fragments. I did make use of the so-called scoops, the large-eyed catheters (double and single), injections and currents of water, single or continuous; but with results so unsatisfactory that I had no confidence in any way of getting rid of fragments excepting by forcible disintegration and chance. In some cases nature and chance did all that was expected: the fragments came in good time, and the case was complete. In others, however, there seemed no end to the disease, which, in reality, appeared rather increased by the communition of the stone. In one instance during the early days of anæsthesia, the patient bore lithotrity as if nothing had been done. Whatever roughness there might have been in the operation, he was under the influence of ether, unconscious, and he bore his condition afterwards most manfully. His malady, notwithstanding, seemed rather to increase than diminish; and his sufferings at last from the fragments were such that he requested to be relieved by lithotomy. I myself, wearied with the repeated unsatisfactory results, willingly complied. Lithotomy was performed. I extracted ten small stones by one swoop of the forceps. The operation did not last a minute; and I do not think that I ever performed lithotomy with less injury to the structures implicated, or with more temporary satisfaction to myself. Yet the patient, a fine healthy man in all other respects, died within a few days.

Although unfortunately experienced in the mysterious way in which patients die after both lithotrity and lithotomy, I was greatly struck by the rapid result in this instance. He had borne with comparative impunity more than the usual amount of irritation and suffering after lithotrity; but he sank under lithotomy, like one with the powers of life already exhausted to the last degree.

This case made a strong impression upon me. I wondered how a man could bear so much from lithotrity, and so little, comparatively, from lithotomy. As he stood the use of the lithotritic apparatus so well, I asked myself how it might have been if I could have removed all the fragments which would not come away, and which by their irritation kept upon him all the sad sufferings from stone.

Some years afterwards my attention was further attracted to this subject. I felt dissatisfied with my experience of extracting stone from the female bladder, either by cutting or dilating. In July, 1854, a case of stone in the female came under my notice. The patient was three years old, and wishing to avoid dilatation, I used a lithotrite made for the purpose by Weiss, smaller than any which he had yet constructed. With this, and a scoop of the same size, I cleared the bladder in two operations under the influence of chloroform, and the cure was rapid and perfect in all respects. I crushed first, and with the scoop extracted the fragments. With this I either extracted a fragment at once and entire, or jammed it so between the blades that I could withdraw the instrument (the blades containing the fragments) readily without serious damage to the urethra.

I was so much pleased with this practice and the instruments, that I resolved to try further, on the first opportunity, in the adult. In December of the same year, a gentleman with a moderate-sized stone came under my notice. On the 6th the stone was crushed under chloroform. On the 13th, several fragments having passed in the interval, crushing those remaining was resorted to. On the 19th a few had passed, but not all that were expected, and in consequence the small scoop was introduced, and the bladder was, after several *manipulations*, seemingly cleared. The next day the pa-

tient was so well that he left town to go a considerable journey. In May, 1858, nearly four years after, this patient came to town with some irritation like his former early symptoms. I examined, and detected stone. Having become in the interval, from further experience, more familiar with the use of the little scoop, I applied it here, and, without much trouble to myself or distress to the patient, extracted three small calculi, each about the size of a flattened pea. In a few days all irritation had ceased, and thus a cure was effected without even crushing, but simply by extracting. In July, 1859, I removed from the same patient an entire stone, about half an inch in diameter, with the same instrument, and from that time he has remained free from disease.

Since the above dates I have almost invariably used these instruments in the process of lithotrity in the male. I have generally, as a first step, introduced a lithotrite of considerable size, equal to a No. 10 or 11 bougie, and broken the stone into various fragments. Next, I have taken the smaller lithotrite, above referred to, attacked these fragments, and then have used the small scoop with the object of removing several fragments, so that the patient might have satisfactory evidence that the stone had been crushed. In a few days after the small crusher and scoop have again been used—particularly the scoop, wherewith the fragments which have been found sufficiently small have been extracted singly, or two or three at a time. Thus, instead of waiting for the spontaneous escape of the fragments, a process usually both uncertain and tardy, the stone has been got rid of by direct and precise surgical interference.

If this practice be judiciously carried out, it will, under ordinary circumstances, prove an immense advantage; and in many instances a stone may be removed with a rapidity little short of the time needful for lithotomy, with the advantage that the patient need not be confined to his bed for a single day.

My chief object in this portion of my lecture is to draw attention to this subject. It is comparatively little known; and, moreover, a very general impression prevails that it is incorrect to extract fragments. The various instruments and mechanical devices which have been from time to time recommended or used for this purpose, or to facilitate their escape, have generally proved of so little service that they have been in a manner overlooked or laid aside by the practical lithotritist. It has even been taught that no attempt should be made to extract fragments; and, as I have already stated, so strong is the feeling in this respect, when stone in the bladder is conjoined with paralysis of that organ, that the operation of lithotrity is considered highly objectionable, if not impracticable, because there is no likelihood of the fragments passing away, excepting through a large-eyed catheter or scoop made specially for the case.

With the instruments which it is my object to recommend, the process of lithotrity, and I believe the distress of the patient both bodily and mental, may be considerably abbreviated; and instead of paralysis being objectionable, it is perhaps the condition most favourable to the operation, as it generally happens that with paralysis there is a callousness of the mucous membrane of the bladder which permits a freer use of the blades than under ordinary conditions.

In early days a large instrument was thought essential for the due performance of lithotrity. The risk of bending or breaking was deemed considerable and serious, and on these grounds the largest instrument which the urethra would admit was selected for use. If a catheter or so-called scoop was used afterwards, its magnitude was thought of equal importance; and to give every advantage in this respect, it was recommended that the urethra should be dilated, and, if needful, the orifice in the glans enlarged by incision, prior to the use of lithotritic instruments.

As to the advantage of a larger urethra there can be no doubt; but I believe that large instruments are by no means so essential. Indeed I feel assured *that comparatively* small-sized ones are an advantage, and in certain stages *of the treatment I believe* them of great value.

The modern lithotrite, particularly of the best English makers, is a very superior instrument to those in former use; and less force is required to break an ordinary stone than was imagined. We seldom hear of an instrument of the kind either bending or breaking; and in the course of time I have come to the conclusion that one of small diameter permits of more ready manipulation than where the size is such as to fill, and be in a manner grasped by, the urethra. Hence, then, I have for many years employed lithotrites of smaller diameter than those in common use. An objection to small sizes has been made, because when the bladder is irritated to spasm the water escapes, whereas it would not if the urethra were filled (plugged) by a large instrument; but the escape can readily be prevented, if desirable, by grasping with the fingers and thumb. Even in regard to the quantity of water required in the bladder during the operation, I believe that there is no such necessity for a large amount as some have imagined. When the organ is distended with eight, ten, or twelve ounces, the stone is usually more difficult to be caught than when the quantity is smaller. The slightest touch causes it to move where there is a large amount of fluid, and fragments are certainly much more difficult to be detected and seized. No doubt it is unwise, even dangerous, to open the blades of a lithotrite freely in an empty bladder, or one with very little water in it; but I have long been convinced that there is no need for the presence of so much fluid as some have thought.

The chief objections that I myself have found to very small-sized lithotrites or scoops are, the comparative difficulty of passing them into the bladder, and, especially, the comparative difficulty of sounding for fragments. In the finer manipulations of the sort it is hard to say whether the sense of touch or of hearing is of the greatest value. I am almost inclined to give the ear the preference; but even with this it is of the greatest consequence that the utmost facility of movement should be permitted, and hence I am averse to the use of the sounding-board, as recommended by Mr. Brooke and others, as also to the use of instruments which fill up the urethra to such an extent as to prevent the free movement of the point of the instrument within the bladder.

I was so early impressed with the necessity of having a narrow shank for a sound, so that every facility should be given to movement, that I had Heurteloup's sound, as it is called, modified accordingly. The shank was made of smaller diameter by several sizes than the curved end, and thus the latter was a sort of lob, which could be turned about readily in all directions, while its comparatively large size gave more certainty to both hand and ear. An instrument of the kind (Fig. 1) has been depicted in the later editions of

Fig. 1.

my work on Surgery, and is now in considerable general use. A sound of this sort is of far greater value than one with a thick stem, or with a slender stem and equally slender point; and in accordance with my views on these matters, Mr. Matthews has lately constructed, on his own devise, a lithotrite with a narrow stem and lob-point, which, in my opinion, goes far to facilitate the effective use of the lithotrite in ordinary cases of stone. This figure (Fig. 2) gives a representation of it. The force is applied by the rack and pinion, in accordance with my own views on this subject, but of course the same *shape will answer for the various* ways in which the screw force has

been so admirably applied in modern times by Messrs. Weiss and Mr. Coxeter. The figure shows the blades open, but when closed there is a lob-end, like that of the sound in Fig. 1. The narrow shank permits free movement both in the urethra and bladder, and the increased bulk at the end intended to touch the stone, gives development to both touch and sound.

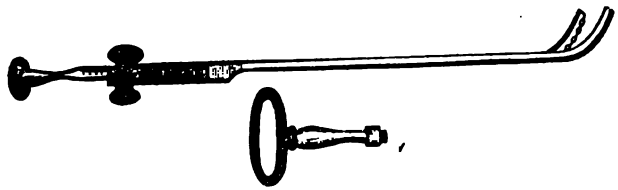


Fig. 2.

But these instruments (Figs. 3 and 4) are those to which I am most desirous of drawing attention. Fig. 3 shows the diameter of a lithotrite which I

Fig. 3.

often use in dealing with fragments, and Fig. 4 shows a scoop of still smaller diameter. The small size must appear remarkable even to those familiar with the lithotrites and scoops hitherto known. I am not aware that any so small as Fig. 4 has ever before been made. Mr. Matthews, the instrument maker, informs me that there were none in the recent International Exhibition, and certainly none came under my own observation.

I have instruments of the kind in my possession varying in size from No. 3 to No. 6 bougie or catheter sizes, and with them I am in the habit, as I imagine, of diminishing the period usually devoted to the completion of lithotrity.

Generally when a stone is crushed the fragments are left to chance. So-called scoops and big-sized catheters have been introduced, and by the natural stream, or the force induced by injections, the fragments have been extracted, and have occasionally appeared in quicker time than if left solely to chance; but in the majority of instances the surgeon and patient have waited for the chance escape of the fragments without instrumental aid.

At this date, there is ample experience to show that after a stone is crushed by one or repeated sittings, the fragments do come away, and a perfect cure is the result. But in many instances the last fragment is tardily discharged, and many weeks or many months elapse ere the cure is complete.

It may be justly said of successful lithotrity that neither weeks nor months are long in comparison with the advantages gained; but, for the credit of surgery, it may be claimed as an advantage to do that in a few minutes which nature might take weeks or months to accomplish.

The chief object I have in view in these remarks is to establish this practice.

Supposing a stone crushed and its fragments comminuted, my proposal is, that these should be removed at the same time, or as soon thereafter as may be. The lithotrites generally in use are so large that they can be withdrawn *from the* bladder only when shut. If a fragment rests between the blades, it *must be comminuted* before these can be closed. The same remarks are

applicable to ordinary scoops. Comminution must be so effectual that the blades may be closed, or nearly so, and all that can be brought away is the small bruised portion held in the hollow between the blades.

Fig. 4.

With a small lithotrite and scoop (Fig. 4) such as I am in the habit of using, a fragment of considerable size may remain betwixt the blades, and yet the united size or diameter may readily pass or be drawn along the urethra.

In the last sixty cases I have adopted this practice generally, and, with few exceptions, have had every reason to be satisfied. Occasionally, when over anxious for a rapid cure, I have extracted fragments rather too large to come readily along the urethra, particularly in the prostatic or membranous portion, or at the triangular ligament. In some, when the urethra nearest the neck of the bladder has been rather roughly used, there has been considerable irritation; in others, even under such circumstances, there has been no irritation whatever; and in many instances I have been able to effect in one or two operations within ten days what, according to custom, would take weeks, or possibly months. I have done, in fact, by a precise surgical manipulation, that which according to ordinary rule is left entirely to chance. Experience has taught me that it is almost hopeless to trust to chance in all such cases; that in many instances the fragments may be weeks, or months, in coming away, even with attempts to coax them through catheters with large eyes and other instruments devised for the purpose. Here are the fragments of stone crushed in a man whose bladder acted regularly, yet only a few of these passed spontaneously; all the rest were removed by the scoop in question in three or four operations. From first to last there was not a single bad or even troublesome symptom. The patient was detained scarcely an hour in bed beyond his regular period of rest. I never saw one suffer less distress; yet, before he came under my notice, he had been strongly urged by an eminent surgeon to submit to lithotomy.

It is considered an extreme misfortune for a patient to have stone in the bladder and paralysis of that organ at the same time; and it has been well-nigh a rule in surgery that lithotrity is scarcely eligible in such cases, on the ground that the fragments are not likely to be expelled or carried off by the stream of urine through the urethra or a catheter. Of course I am aware that there have been successful cases of the kind; but the success has been more from lucky chance than from skilful surgical interference. And my object in these remarks is to claim for surgery a direct, special, and precise mechanical movement whereby that which has heretofore been left to chance is converted into a certainty. Here are the fragments of a large stone from the bladder of a patient who could not pass a drop of water without the aid of a catheter. All of them, with a few minor exceptions, were extracted with a small scoop. Here are the fragments—from a similar case, where, however, the urethra was unusually large—which were all extracted by a small scoop by my friend Mr. Henry Smith. The largest of these fragments is about three-quarters of an inch by about half an inch wide, and it was extracted without any laceration or marked distension of the urethra; yet through the neck of the bladder, or a urethra of this size, even the urine would not flow.

The mechanical development, *if I may so* call it, of stone, shows the un-

certainty of its spontaneous expulsion from the bladder. Most calculi have each a nucleus far smaller than the diameter of the urethra, but how few such nuclei pass? At all events, stones larger than the diameter of the urethra—all the instances in which lithotomy and lithotrity have been performed—are examples where small round bodies (the nuclei) have not passed away spontaneously.

Now, I claim for surgery the power of taking away such bodies. I do not profess originality in this respect, for we all know what was done by Sir Astley Cooper in this way, but the instrument he used in removing small calculi—gravel, we might say—is of inferior mechanical powers to the modern lithotrite. It would be of little use in dealing with fragments in lithotrity; nor would the ordinary lithotrite or scoop be of much use in what I now speak about.

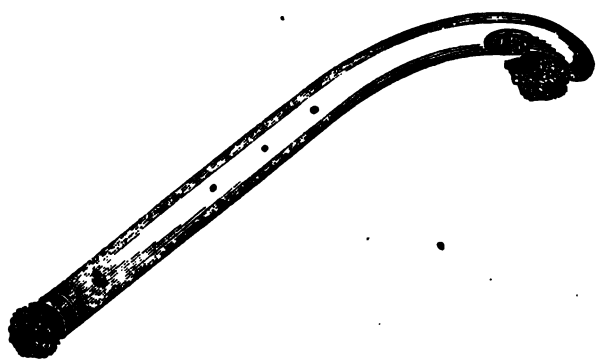
The small instruments which I now show are essential to the practice which I advocate, and with these I maintain (as I have amply tested in numerous cases) that lithotrity can be abbreviated and brought to a certainty such as has not been claimed for it hitherto.—*Lancet, July* 2, 1864, *p.* 1.

53.—A NEW TUBE FOR USE AFTER LITHOTOMY.

By Jonathan Hutchinson, Esq., Surgeon to the London Hospital, and Assistant Surgeon to the Royal London Ophthalmic Hospital.

Many successful lithotomists have attached great importance to the introduction of a tube through the wound, and its retention in the bladder for several days afterwards. I have myself used it in several cases, and am inclined to think that it is of great value, *first*, in adults (in whom the wound is long); and, *secondly*, in those cases in children in which the operation has, for any reason, been prolonged, and the cellular tissue may probably have been considerably disturbed. It has often occurred to me that the tubes commonly used for this purpose, which are straight, and have a hole only at their ends, are not so well adapted as might be. In a recent case—that of an old man from whom I had removed three large calculi—I employed for the first time the one I now show you.

You will observe that the tube is curved, so that it may dip into the lowest part of the viscus, behind the prostate. It is of silver, and of as large a bore as possible. A strip of sponge is passed through it from end to end, projecting at each. This sponge is intended to take up the urine by capillary attraction, and convey it through the tube. Then, again, over the end of this tube



Lithotomy tube (half-size).

we may fasten an India-rubber receptacle for the urine, and thus prevent the *wetting of the bed* and the consequent irritation to the patient's skin. The

tube is perforated with holes, through which a ligature may be passed in order to secure on its exterior pieces of sponge whenever the occurrence of hemorrhage may make plugging of the wound needful.

It served its purpose very well in the case in which we used it, and the patient made good recovery.

With the straight tube, with a hole at its end, it is clear that, supposing the tube to project into the bladder, it will be requisite that the urine should rise to a certain height before it can gain entrance to the tube. Usually, therefore, I believe the urine finds its way by the side of the tube, and not through it. With mine, in the case mentioned, it was quite certain that urine flowed *through* it.—*Clinical Lectures and Reports of London Hospital*, 1864, *p.* 93.

54.—ON THE BEST METHOD OF DETECTING SMALL QUANTITIES OF ALBUMEN IN THE URINE.

By Dr. Andrew Clark, Assistant Physician to the London Hospital.

It is of great clinical importance to possess a simple and certain method of detecting small quantities of albumen in the urine. After a great variety of experiments, I have come to the conclusion that nitric acid used in the manner about to be described—a modification of the plan suggested by Heller—is by far the most sensitive, reliable, and handy agent that can be used for this purpose by the physician.

Pour not less than half a drachm of *fuming* nitric acid into a test tube; incline it, and then let a like quantity of the suspected urine trickle down *very slowly* to the acid, over the surface of which the urine will float without the slightest admixture. If albumen be present, a milk-white, sharply-defined, tolerably tenacious film will form at the exact point of junction of the two fluids. This film is never, at first, thick; and when the amount of albumen in the urine is extremely minute, it may be so thin as to become visible only by reflected light when the test tube is inclined. Occasionally, when very thin, the albuminous film is dissolved in the course of a few hours. Commonly, however, it increases in breadth, diminishes in density, becomes yellow or yellowish-green at its under surface: and throws off minute coagula, which fall through the acid to the bottom of the tube.

Nitric acid used in this manner as a test for albumen is also a test of the presence of uroxanthine, or bile—either or both of which are not unfrequently present in temporary and functional albuminuria.

If, in immediate contact with the acid, a ruby or violet ring is developed, uroxanthine is present;—and bile also, if in addition to a red or violet there is formed likewise a green-coloured ring, which remains for some time.

Two feasible objections are urged against depending solely on the employment of nitric acid in the manner described, as a test of the presence of albumen; and I have myself noticed a third; but a careful examination of their force leads me to the conclusion that they are more theoretical than real.

When urine, rich in uric acid or its salts, or containing much scaly epithelium, is poured over cold nitric acid, a general turbidity arises, which is said to be undistinguishable from that produced by the presence of albumen.

But if the proposed test for albumen be properly applied, no turbidity will be produced by the presence of that substance, unless urates are also present. And then the white film of albumen is separated from the superimposed turbidity by a thin stratum of clear urine.

The turbidity produced by uric acid or epithelium is general, granular-like, and without any approach to coherence.

The turbidity produced by urates is sometimes abruptly defined below by an opaque, ring-like border, sometimes coloured, sometimes not; but a stratum of clear urine intervenes between this ring and the surface of the acid, and, as above, the turbidity has *no cohesion* of parts. Besides this, the tur-

bidity produced by lithates may be immediately dissipated by heat; and, if not in great excess, even by the heat of the hand closed around the tube.

The film produced by the contact of nitric acid with albuminous urine is quite different from any kind of turbidity. Confined to the layer of urine resting upon the acid, white like a disc of compressed cotton, tenacious, and when shaken with its associated fluids, breaking into flaky fragments, it seems improbable that any but the merest tyro should mistake it for anything but what it is.

In testing for albumen by means of heat and nitric acid, there may be no immediate response indicative of its presence; and yet after a few hours, a flocculent precipitate may form and fall to the bottom of the tube.

A specimen of urine examined within an hour after extrusion from the bladder may yield unequivocal evidence of the presence of albumen, and cease to do so after twelve hours.

Small films of coagulated albumen produced on the surface of nitric acid occasionally disappear within a few hours from the time of their formation.

Little importance is to be attached to the presence of small quantities of albumen in the urine of women a day or two before or after menstruation. It is common without any disorder of the kidney or any sensible discharge from the vulva.

Small quantities of albumen are often present in the urines of women with leucorrhœa, and of those who have recently had fits of hysteria.

One is not justified in asserting the absence of albumen in the urine upon the result of one or two examinations. I knew a case in which albumen occurred in the urine daily for several months; but it was present only in the urine first passed after breakfast, and was never, to the time of its departure, present in the urine passed at any other time.

Men sometimes discharge a thin whitish glairy fluid with the closing stream of urine in the act of emptying the bladder. This fluid is said to be seminal; but in none of the examples that I have examined were any spermatic filaments present. From its containing mucin, and young cell particles, I look upon it as an augmented and slightly altered secretion of the glands opening into the urethra. When discharged in any quantity, the urine containing it responds to all the ordinary tests of the presence of albumen.

Albumen in small quantities and unoccupied by casts, may be present in the urine daily for three years, and at last permanently disappear. This occurred in a case under my observation. The health which had previously been bad, rapidly improved after the disappearance of albumen from the urine, and became ultimately very good.

Mere hepatic congestion is sometimes the cause of slight functional albuminuria. I had under observation for some time a lady whose "liver attacks" were invariably preceded by the appearance of small quantities of albumen in the urine. With the free purgation which was found necessary for the removal of these attacks, the albumen disapeared. I remember also the case of a gentleman who was subject to somewhat similar attacks. In his urine, however, free uric acid was associated with the albumen, and both stayed several days beyond the subsidence of acute disorder. But he was a wilful patient, and chose to live well even at the cost of being ill.—*Clinical Lectures and Reports of London Hospital*, 1864, *p.* 224.

55.—ON STONE IN THE FEMALE.

By Dr. G. M. Humphry, F.R.S., Surgeon to Addenbrook's Hospital, Cambridge.

Having seen a case of attempted lithotomy in the female, where the urethra was incised towards the pubes, and the instruments for extracting the stone missed the bladder and passed into the cellular tissue in front of it, the girl dying next day; having also seen the difficulty and pain attendant on the use of metallic instruments to dilate the urethra; and having also heard that my predecessor, Mr. Okes, employed catgut for the purpose, I took the hint, *and have found it* answer remarkably well. My plan is to select pieces of

catgut bougie, about four inches long, and pass through the urethra as many as it will hold, securing them in their position by a thread tied round them and attached to a bandage which, passing over the os externum, has a slit in it to admit the bougies, and is tied at either end to another bandage round the waist. Usually I insert a silver catheter into the bladder first, and pass the bougies beside it. It allows the urine to flow off, and affords a *point d'appui* for the bougies. It is not, however, necessary, for there is usually sufficient room for the urine to escape between the latter. In a short time, the bougies, becoming swelled, dilate the passage in the gentlest and most gradual manner. After three or four hours it is quite easy to introduce one, two, or three more bougies, which begin to swell and further stretch the canal. Additional bougies may be added in such numbers and at such intervals as is found to be desirable; and the dilatation of the urethra is thus conducted in a safer and less painful manner than by any other device that I have known. Sometimes, on visiting the patient, I find all the bougies expelled by the straining expulsory efforts of the bladder. This is of little consequence: I merely fill the urethra again with fresh pieces, and proceed as before. It is the best plan to visit the patient frequently, and add one or two bougies every second or third hour, taking care to introduce them gently, oiling them well, and inserting the fresh ones between those that are already there: by which means the sides of the urethra are less likely to be injured. In twenty-four or forty-eight hours the passage is sufficiently dilated to admit the easy introduction of a pair of forceps commonly used in lithotomy. The size required can be estimated pretty accurately through the information derived by the finger in the vagina, and had better be a little over than under the necessary mark. They must be used carefully; and, when the stone is in their grasp, the finger in the vagina will tell whether it is in the most favourable position, or will assist to bring it so. I take care to conduct the extraction very slowly, and with as little force as possible, giving the urethra time to dilate still further under the pressure of the forceps and the stone. The patient is under the influence of chloroform, and my object is to avoid lacerating the urethra rather than to shorten the time occupied in the operation. It is astonishing how wide the urethra may be dilated, without tearing, if only sufficient time be allowed; and, when it is torn in the extraction of large calculi, it very soon recovers, and the patient is able to retain the urine, which is shown by the following cases:—

Mary A., aged fifty-three, had symptoms two years. The stone was readily felt with the sound; and the finger in the vagina detected a lump, of the size of a chestnut, lying on the right side of the fundus. At eight a. m. I commenced dilatation, by inserting three catgut bougies. At eleven she was suffering from the distension of the bladder, which was relieved by the addition of a catheter. At two p. m. another bougie; the same at five; and two more at nine. She slept a great part of the night. At eight next morning I found that the bougies had just escaped from the bladder. The urethra was well dilated, admitting with ease a small pair of lithotomy forceps, with which an oval lithic-acid stone, weighing two drachms and ten grains, was grasped in a favourable direction, and dragged slowly through the urethra without much difficulty. In three days she could hold her water quite well.

Susan J., aged three. Attempts had been made to extract the stone by the surgeons of her native place, but they found it to be too large to be drawn through the urethra. I could not feel it from the rectum. At twelve o'clock dilatation was commenced by two bougies; at five a third; at nine a fourth; at seven next morning, a fifth; they were passed with little difficulty, and the child had not suffered much pain, sleeping a good deal. At eleven a. m., when she was under the influence of chloroform, a small pair of lithotomy forceps were introduced, the stone was quickly seized, and gradually drawn through the urethra, dilating and perhaps tearing a little as it passed. It weighed two drachms and twelve scruples. There was incontinence of urine for two days; on the fifth, she could retain the water four or five hours; and in two more days was discharged cured.

From a girl aged four, I extracted by the same process a stone weighing

three drachms and a half; from another, aged five, a stone weighing two drachms; and from a third, aged twelve, a rough stone weighing an ounce and a drachm, measuring in its smallest circumference three inches and three-quarters, and in its largest four inches and a half. They all recovered quickly and completely.

The conformation of parts in the female is extremely favourable for lithotrity. I have had recourse to it in some cases with excellent result. But where the stone is large, and especially in the child, I think it is better to effect the extraction at once. Under such circumstances, and in such delicate patients, the ureters, pelves, and calices of the kidneys are liable to be dilated and to take on inflammation if subjected to slight additional irritation, with the stone, or parts of it, remaining in the bladder and more or less impeding the flow of urine. The following case, which occurred early in my practice, shows the importance of being prepared to complete the operation of extraction at once. Had I done so the result might have been different.

A rather delicate girl, aged four, had symptoms of stone from birth. On Nov. 9th I commenced dilating the urethra with catgut bougies, and continued it gradually and carefully. They were several times expelled from the bladder. On the 11th, the urethra admitted a pair of forceps without difficulty, and I grasped the stone, which was evidently large. The forceps slipped from it, and I had some difficulty in seizing it again, but at length succeeded. I felt something give way; and, the forceps slipping again, I found one of the blades almost broken through near the middle. The child, having been on the table some time, and subjected to a good deal of pain, was sent to bed. Next day she appeared to be comfortable. On the 13th she was chilly and ill, and the urine rather thick. Some diarrhœa followed, with fever, an ulcer upon the palate, torpid languid state, and death on the 20th. The muscular coat of the bladder was somewhat thickened; the mucous membrane showed one or two spots of slight bruising. The urethra was a little congested, but showed no signs of laceration or ulceration. The stone was as large as a bantam's egg. The ureters were dilated to the size of my finger. The pelves and calices of the kidneys were large, and, together with the ureters, were distended with thick purulent urine. The substance of the kidneys was studded with small spots of pus. There were, besides, pneumonic patches with commencing suppuration in the lungs, superficial ulcers about the isthmus faucium, redness and prominence of Peyer's glands, and red patches in the bowels.

Another girl, aged twelve, with symptoms of long standing, got wet and caught cold on her way to the hospital on the 14th November. She had stillicidium urinæ and prolapsus recti, and was feverish. On November 23rd she was sounded when under the influence of chloroform. The stone was evidently large; and her pallid emaciated appearance, quick feeble pulse, glazed lips, and sunken eyes, forbade any attempts at its extraction. She continued to decline, and died on the 26th. The stone, larger than a walnut and of phosphatic composition, was roughish, and was grasped by the bladder, the coats of which were much thickened. The ureters were as large as my finger, but at the entrance into the bladder were of the natural size. The pelves, infundibula, and calices of the kidneys were much dilated and filled with urine, the cortical substance of the kidneys being reduced to very small proportions. On the right side the urine was turbid and purulent, and there was interstitial suppuration throughout the substance of the kidney.

Bone, teeth, hair, &c., extracted from the bladder.—Rebecca B., aged thirty-eight, sent to me by Mr. Wright, of Chatteris, in July, 1852. A healthy person, with the usual symptoms of stone. The urine alkaline, containing pus and blood corpuscles, and crystals of triple phosphate. Said that ever since her last confinement, seven years ago, she had been subject to pains in the left side of the lower part of the body, which were sometimes very severe. During the last year she had observed frequent "settling in the water, of red colour, with pieces of flesh." For the last few months only had experienced pain in passing urine. She said also that she had occasionally *voided pieces of chalky* substance, and once something much like a tooth.

The sound came in contact with a foreign body, which was judged to be a stone. I accordingly proceeded to dilate the urethra in the manner above described. After twenty-four hours a small pair of lithotomy forceps were introduced, and soon seized a foreign body, which broke; a portion coming away in the forceps proved to be a tooth, like a bicuspid, in a socket of bone. I subsequently seized a larger mass, and having discovered it to be free, broke it with the forceps, and extracted several large fragments of bone, teeth, and hair. She recovered quickly, and in a fortnight returned home, stating that she was quite well, though there was still pus in the urine. The bone contained the usual corpuscles. The hairs, of a reddish-brown hue, were mixed up with the bone and included in it, as if the bone had been formed round them; possibly they may have become insinuated into the smaller orifices of the bone which they occupied. The teeth were like bicuspids and molars; the former with bent, the latter with imperfect fangs, and more or less completely enclosed in the bone. They were not arranged in a natural order, nor did the bone present all the appearances of a maxilla. The fragments are preserved in the museum of the University of Cambridge.

After several communications with Mr. Wright, I went to Chatteris, on March 23rd, 1854, to extract further accumulations from the bladder. I learned that soon after her return home she began to pass from the urethra pieces of bone, teeth, and masses like chalk. One of the latter, which was shown me, weighed a drachm and a half, and measured nearly two inches in its smallest circumference; it was of irregular shape, quite smooth, with a pinkish exterior. Cutting into it, we found it to consist of an outer friable earthy crust, enclosing a fragment of bone with teeth projecting from it. The woman had lately suffered a great deal of pain, and was much thinned. Mr. Wright had succeeded in dilating the urethra by means of the catgut bougies; so that I had little difficulty in introducing my finger into the bladder. Directed to the left side, it entered a sac communicating with the bladder by a circular opening just large enough to admit the finger, having a soft defined edge. In this sac I could feel a large rough stone. With some difficulty I passed the forceps through the bladder into the sac and grasped the stone. It broke, and repeated introduction of the forceps was necessary to clear the fragments from the bladder. Then the finger found a quantity of the broken stone in the sac, which I contrived, with a great deal of trouble, to bring away by means of the scoop. At length I was satisfied that the sac and bladder were emptied, and washed them out thoroughly with water by means of a syringe. The lining of the sac appeared to be smooth and soft, but uneven, not unlike that of a bladder with rugæ. She recovered without an unfavourable symptom, soon regained the power of retaining her urine, and has remained quite free from any disorder of the bladder. On Sept. 25th, 1855, she was delivered of a very fine child.

There were a great quantity of hairs in this stone, especially in one portion, which had probably formed the centre; but no bone or teeth. Mr. Warner ("Philosophical Transactions," vol. xlvii.) and Sir Benjamin Brodie each met with a very similar case; and the most probable explanation of them is afforded by the supposition that one of those ovarian cysts which are known not unfrequently to have bone, teeth, and hair growing from their walls, had ulcerated and discharged its contents into the bladder; that phosphatic deposit from the urine upon some hair remaining in the cyst formed the stone last removed; and that the clearing of the cyst on that occasion led to the cure. This view is confirmed by the dissection of another case by Dr. Phillips, given in the ninth volume of the "Medico-Chirurgical Transactions." There was a tumour of the ovary, containing cream-like substance and a quantity of hair, also a tooth attached to its wall; and "the bladder was distended with a substance similar to that contained in the ovarian tumour. Here, also, was discovered another large tuft of hair." I presume these contents had passed from the ovarian tumour into the bladder through some ulcerated communication.

The origin of the hairs which have in a few cases been observed to pass from the urinary *passages of the male* is somewhat more obscure. There are

so many sources of error that it is first necessary to substantiate the fact with great care. Dr. Henry ("Medico-Chirurgical Transactions," vol. x.) satisfied himself, by the most careful investigation, that a quantity of short hairs, which a middle-aged gentleman observed in his urine, had their origin from some of the urinary passages. Sir Benj. Brodie also attended a gentleman who laboured under calculus of the bladder and disease of the kidneys, in whose urine he every now and then detected some very minute hairs, which he suspected to have been of renal origin. Two years ago I had occasion to remove from the perineum of a man, aged forty, one stone weighing five drachms, quite smooth, and composed apparently of lithate of ammonia, investing a soft putty-like nucleus; also several other smaller stones, made up of phosphate of lime and a quantity of short fine hairs, which were most numerous in the interior of the calculi. I could discover no hairs in the larger stone. The sac in which they were contained had an external fistulous opening, communicated with the urethra just in front of the prostate, and had a soft mucous lining. I examined the latter carefully, but could discover no hairs growing from it. During his convalescence, I repeatedly found short hairs in the urine which passed through the fistulous tract; whereas the urine taken from the bladder by a catheter, which I introduced several times for the purpose of this investigation, did not appear to contain any, or so few that they might have been accidentally missed. I concluded, therefore, that they had their origin in the fistulous tract. This man had been lithotomized by Mr. Okes, sen., when he was four years old; and a second time, a year afterwards, by Mr. Okes, jun. In the first instance, a cystic oxide calculus weighing four drachms; in the second, a fusible calculus weighing two scruples, had been removed. He recovered quickly each time; but was "loose-watered" after the last operation, rendering it necessary to wear an instrument for the purpose of retaining the urine. The fistula had originated in an injury to the urethra by a sprain, causing urinary abscess ten years previously; and he had several times passed calculi through it. In the case related by Mr. Paget, of Leicester ("Medico-Chirurgical Transactions," vol. xxxiii.), where a ring of lithic acid, with a fine hair in its axis, was removed through the persistent urachus, it is probable, as he surmises, that the hair was one of the pubic hairs, which had found its way through the umbilical opening.—*Lancet, July* 30, 1864, *p.* 114.

56.—ON THE TREATMENT OF STRICTURES OF THE URETHRA.

By Barnard Holt, Esq., Senior Surgeon to the Westminster Hospital.

Mr. S., an officer in the navy, consulted me, by the advice of Mr. Wilson, of St. Leonard's, for an obstinate stricture of the urethra, situated in its spongy portion, about three inches from the meatus. Mr. S. attributed his stricture to an injury, attended by profuse hemorrhage; for it was shortly afterwards that he noticed a diminution in the stream of urine, which became rapidly worse, and eventually could only be passed guttatim. Various attempts had been made previously to my seeing him to introduce a catheter, but without success; and the surgeon under whose care he then was, failing to pass the catheter, *blistered* the upper part of both *thighs*, the blisters being kept open for some time, but, of course, without the slightest benefit. Otherwise a perfectly healthy person, he now suffered from great frequency of micturition and distress from violent straining. Upon examination, I found it quite impracticable to pass any instrument, indeed not even a fine probe, through the impediment. In the following week he came to London, and, after a very prolonged trial, I succeeded in passing a ½ No. 1 catheter through the stricture. This was retained in the hope that the contraction might yield and admit a larger size; but the pain it occasioned was so great as to compel him to remove it before my second visit. Seeing it would be quite useless to persevere with that method, he was subsequently placed under the influence *of chloroform* when, after great difficulty, the dilator was pressed through,

and the stricture was immediately split with a No. 9 tube, the urethra being preternaturally small, and not admitting a larger size. The quinine-and-opium was administered, and a few hours afterwards the urine was removed with a No. 8 catheter, which passed freely into the bladder. This gentleman never had an unfavourable symptom of any kind, and he at once passed his water without difficulty. The after-treatment was continued, and he has now rejoined his ship, micturating naturally both as to stream and frequency, the urethra admitting a No. 8 without difficulty.

J. R., a policeman, was sent by Sir John Fisher into the Westminster Hospital, to be under my care. His case was in every respect similar to the above. Without the aid of chloroform I could not pass the smallest probe, but with the assistance of that agent I succeeded (but only by great perseverance) in introducing the smallest dilator, and rupturing the obstruction with the No. 10 tube. This patient never had an unfavourable symptom of any kind, and described himself on the following day as passing his urine as well as he ever remembered. The No. 10 was introduced three or four times afterwards, when he returned to his duty, with directions to have an instrument of the same size passed by the divisional surgeon at increasing intervals.

Mr. W., a commercial traveller, was sent to me by Mr. Reeves, of Carlisle, in consequence of an obstinate stricture of the urethra, complicated with fistulæ-in-perineo. He had suffered more or less from stricture for some years, which, in 1860, resulted in an abscess in the perineum and the establishment of a fistulous aperture, through which the greater part of the urine flowed, At this time he consulted a well-known surgeon in London, but, from pressure of business, he was unable to continue under his care. The stricture gradually became worse, and in December, 1863, another abscess formed behind the former one, leaving another fistula, so that at the time of my seeing him a large part of the urine was ejected through these two openings. Upon examination, a tight unyielding stricture was detected at the bulb, which resisted the first three attempts to pass even the smallest catheter. Upon the fourth trial, the catheter reached the bladder, and was retained for two hours, the pain being too great to permit its remaining longer. Subsequently the dilator was passed fairly through the obstruction into the bladder, and the stricture, which was very dense, was ruptured with the No. 10 tube. Although he had previously suffered greater when the catheter was passed, he described the pain of the operation by rupture as trifling. The No. 10 catheter was passed directly afterwards, and the urine removed. Quinine-and-opium was administered, and the urine was again drawn off in the evening. This patient never had a bad symptom of any kind whatever. The No. 10 bougie was passed at the prescribed intervals, and he now (three weeks after the operation) voids his urine in a full stream, the fistulæ having nearly healed; his general health also is much improved.

Dr. Hamilton Roe requested me to undertake the case of Mr. B., a member of our own profession, who had long suffered from stricture. Mr. B. informed me that for twelve years he had suffered from the effects of stricture, and that twelve months since he injured his perineum, which greatly increased his difficulty in micturition, which had now become so frequent as to destroy his rest, and thus materially influence his health. His urine contained a large quantity of ropy mucus, and was ammoniacal. With great difficulty I succeeded in passing a No. 2 catheter; but on his second visit I found it was impracticable to introduce any catheter at all. I therefore requested him to take lodgings near me, for the purpose of having my operation performed. In the following week, after a prolonged trial, I succeeded in passing a No. 1 gum-elastic catheter, which was retained for twelve hours. The dilator was then introduced, and the strictures—one at the bulb and the other at the membranous portion of the urethra—were at once enlarged by the passage of the No. 12 tube. Although he had previously experienced considerable pain when the passage of the catheters was attempted, he did not evince the slightest manifestation of pain during the operation, which he described as of the most trifling character. The No. 12 catheter was now passed, and the urine removed; quinine-and-*opium was taken*; and in the evening the urine was

withdrawn by the catheter. This gentleman never had a bad symptom of any kind whatever; he was out on the following day; and a No. 13 bougie can be introduced without the least difficulty. He now passes the night without requiring to relieve himself, and only micturates three or four times during the day.

Colonel —— consulted me in July, 1863, having suffered from stricture for twenty-five years, for many of which he had been under treatment by the ordinary dilating process, without, however, deriving any considerable advantage, no size larger than a No. 5 having been attained. He is now obliged to strain violently to expel his urine, which requires to be very frequently passed both day and night, and the urgency is generally so great that he has hardly time to reach a convenience before a small quantity will escape. Upon my first examination I found it impracticable to pass any catheter; I, however, eventually succeeded in passing a No. 2, which with difficulty was repeated upon three or four occasions; after which his military duties prevented his regular attendance, and I lost sight of him for some months. At the commencement of the present year he again consulted me, being then worse than ever. After great difficulty I succeeded in passing No. 2, and on the following day introduced the dilator and split the stricture with the No. 12 tube; the urethra was, however, so long that I did not get the full power of the instrument, and consequently could only immediately afterwards pass a No. 8. The pain was very trifling, and the hemorrhage very slight. The quinine-and-opium was prescribed, and in the evening the urine was drawn off. This gentleman never had, beyond a slight scalding, the least inconvenience whatever. He was out on the following day; and now (rather more than two months from the operation) I can pass No. 13 with ease; his irritability has entirely ceased; he passes the whole night without requiring to make water, which he now accomplishes without straining, and he can eject it a considerable distance from his body.

Colonel —— was sent to me by Dr. M'Ewen, of Chester, having been the subject of stricture for more than twenty-five years, during which time he had been under the care of various surgeons; at one time residing in the house of a surgeon in London for his more effectual treatment; and at another time was long subjected to the treatment of dilatation by retention of a catheter in the bladder. His stricture, however, invariably returned; and at the time of his consulting me he could pass a No. 5 catheter, but its introduction produced so much irritation that it was always followed by retention of urine and more or less irritability, which continued until he was compelled to introduce his catheter again. It was under these circumstances that he consulted me, hoping that by my treatment his urethra might be so enlarged as to prevent the irritation caused by the passage of the catheter. In answer to my inquiry what would be the result if he did not pass a catheter for some weeks, he informed me that the urethra then became so contracted that he had great difficulty in passing any instrument at all. Having taken lodgings close to my house, I passed the dilator and split the stricture with the No. 12 tube, and immediately removed the urine. In the evening the catheter was again introduced; but the following morning, although he had passed a perfectly quiet night, after returning from a cold water-closet, he had a rigour, which however did not last more than five minutes, but which for two or three days made him feel very ill, and rather interrupted the ordinary treatment. Beyond the rigour he never suffered any inconvenience; the passage of the catheter did not create any irritation, and was not followed by the slightest annoyance; but, on the contrary, his irritation subsided, and his urine was voided in a much more voluminous stream. In a fortnight he returned to the country; and in a letter I shortly afterwards received he stated: "I yesterday morning passed the instrument with more ease than I ever did before, and with no bad consequences afterwards. I sleep well through the night, being seldom obliged to get out of bed till five or six o'clock in the morning. If you at any time wish to refer a timid man to one who has experienced your treatment, you have perfect liberty to send him to me."

Mr. C., an officer in the Hussars, came from India for the purpose of having

my operation performed. He stated that for several years he had suffered from stricture, which had latterly become so bad as to require him to obtain leave of absence for the purpose of having it remedied. At present he is suffering from irritability of the bladder, frequent micturition, great straining, and the urine occasionally only passing guttatim. On examination of the urethra, I ascertained he had two strictures, through which a No. 1 catheter could with difficulty be passed. Having taken lodgings in my neighbourhood, on the second day from his first consulting me, I succeeded in passing No. 1 into the bladder; and having retained it for two hours, it was removed, and the dilator substituted, the urethra being immediately enlarged with the No. 12 tube. As in the former cases, the urine was withdrawn, both at the time of the operation and subsequently in the evening, by the No. 11 catheter. Quinine-and-opium was prescribed. This gentleman never had a bad symptom of any kind whatsoever; he was out on the following day; and he now passes his urine in a full stream without the least difficulty or irritability. A No. 11 bougie can be passed with the greatest ease.

Dr. ——, a medical practitioner in Ireland, consulted me in consequence of stricture, the symptoms of which very much resembled the preceding case —viz., frequent micturition, great straining, inability to propel the urine from the body, broken rest, and general debility. Upon his first visit, I succeeded in passing a No. 2, which was held with great tenacity, so as to require slight force to remove it. On the following day I passed the dilator, and enlarged the urethra with the No. 10 tube, afterwards removing the urine with the No. 10 catheter, and again passing it in the evening. He never had a bad symptom of any kind whatsoever; he went out the same afternoon, and in a fortnight returned to Ireland, passing his urine as freely as he ever remembered, a No. 11 bougie slipping into the bladder with great ease. I have lately had a letter from this gentleman, expressing his thanks for the great relief that had been afforded him, as well as an assurance that he will now perform my operation upon any cases of stricture that may come under his care.

The above cases have been but very briefly described. I have not considered it necessary to enter into every detail as to the exact situation or number of the strictures, &c., but have contented myself with recording them as they have occurred; and did I not fear to occupy too much space of the *Lancet*, I could describe many more—some more severe, some less,—in all of which the same result has been attained.

In conclusion, I would venture to remark that while it is perfectly optional with any surgeon *acquainted with my method of treatment* to employ it or not, as he sees fit, it is not desirable that practitioners who have never *either read my observations or seen the operation performed* should endeavour to persuade their patients not to submit to it, upon the plea that its performance *must* be attended with serious results. In two of the above cases the patients had been strongly urged not to have my operation performed, but as their surgeons had nothing to offer but the continued introduction of bougies (which were found to be quite useless), they preferred exercising their own common sense, and be subjected to a treatment which immediately relieved them from the continued torture they had previously undergone. In each of these cases the patient declared the pain of the operation was nothing as compared with what they had suffered from the passage of bougies. I again repeat, if surgeons will only ensure the dilator being passed *fairly through the strictures*, and not *through the side of the urethra*, as has been done, into the bladder, that the operation may be performed with as great safety as the most trifling operation in surgery. The pain is insignificant, and the urethra is immediately enlarged to its natural size.—*Lancet, Aug.* 20, 1864, *p.* 206.

57.—ON THE TREATMENT OF ACUTE ORCHITIS BY PUNCTURING THE TESTICLE.

By Henry Smith, Esq., Assistant-Surgeon to King's College Hospital.

In July, 1863, a young man presented himself amongst the out-patients of King's College Hospital with gonorrhœal orchitis in a very acute form. The pain was unusually severe; and, on examining the organ, it appeared to me that suppuration had taken place, the sense of fluctuation being, as I thought, distinct. With a view of evacuating the pus, I took a bistoury, and made a free and deep incision into the supposed abscess; but, to my astonishment and dismay, not a drop of matter escaped—only a little serum and blood. The tubes, however, of the testicle shot out, as it were, from the wound, forming a protrusion the size of a nut. Some pressure was applied by means of strips of plaster, and the patient was sent away.

Two days afterwards the man presented himself; but in a very different condition. He was quite free from pain, all the redness and most of the swelling had disappeared, and, on taking off the strapping, it was found that the protrusion of the tubes of the testis no longer existed.

This case, which was somewhat annoying to me at the time, suggested some serious reflections in reference to the speedy relief which had resulted from a practice which in reality was the effect of an error of diagnosis on my part. Was the sudden relief here a mere accident?—or, if not, to what could it be due? The quantity of serum and blood abstracted was so small that the cessation of pain and diminution of swelling could hardly be due to this cause; but it struck me forcibly that the free division of the fibrous tissue enveloping the body of the testis, and the consequent removal of tension from the organ, was the secret of the success, provided it was not a mere accident.

Influenced by this reasoning, and by the result of this case, I determined to try the effect of puncturing the testis in similar cases; and in the next case of acute orchitis which presented, I made a deep and free incision with a sharp narrow bistoury, emitting about half a teaspoonful of serum and several drachms of blood; and no other treatment beyond a little of the common aperient mixture was supplied. The result here was as successful as in the former; and as cases presented themselves, I adopted the same plan of treatment, reserving it, however, especially to those instances where the swelling and pain were very great. After the trial in a few cases, it was found that the success attending this practice was such as to lead me to adopt it as the usual treatment of acute orchitis; and during the last twelve months I have probably treated in this way upwards of twenty cases, with results that have astonished both myself and those numerous pupils who have witnessed the practice.

In nearly every case so treated—and I have purposely selected the most acute—the patient has experienced the most striking relief before he has left the out-patients' room; and on the next visit, forty-eight hours afterwards, the contrast presented is so remarkable that the superiority of this plan over the old-fashioned modes of treatment is at once impressed forcibly upon the minds of those even who would naturally be prejudiced against so apparently heroic a treatment. The speedy subsidence of all the acute symptoms is due entirely to the puncture of the swollen and inflamed organ, for I have taken especial care not to prescribe anything else except a little of the common white mixture, or perhaps the use of the ordinary lead lotion, and this chiefly to please the patient.

We all know what a terrible ordeal of violent remedies a patient with acute inflammation of the testicle has to undergo. In the first place, he is obliged to lie in bed for several days; a large number of leeches or the constant application of ice are necessary to relieve the pain; and at the same time the unfortunate wretch is compelled to undergo the process of severe purging and continued nausea, by repeated doses of salts and tartar emetic, before any decided mitigation of his symptoms ensues; and two or three days *mostly elapse before* he recovers from the depressing influences of these sev-

eral remedies. Lastly, the unfortunate organ has to be submitted to the tender mercies of a dresser, who, however skilful he may be, cannot help putting the owner of it to severe and prolonged torture whilst he is obeying the injunctions of his superior to "strap testicle."

Now for all this I venture to submit the plan now proposed, and one which I should call a "new" one; but it is venturing on dangerous ground to call anything new now-a-days. Moreover, my old assistant and our present house-surgeon, Mr. Richmond, informs me that when he was in Paris about two years ago he saw the same method of treatment adopted there; but I never heard of it before I resorted to it, and the practice in my hands was entirely due to the accident I have related before.

Of course several of my friends and pupils have urged objections against this plan of treatment, and suggested serious results, in the form of suppuration of the organ, impairment of its function, hernia testis, and fistulous sinuses; but none of these have I witnessed. It is very natural and proper to make these objections, for we have always carefully avoided the possibility of a puncture of the testicle when using a trocar for paracentesis of the tunica vaginalis, and, indeed, I have witnessed violent suppuration of the testis speedily ensue from this accident; but it must be borne in mind that wounding of a healthy testicle with a large and blunt instrument like a trocar is a totally different thing from a careful incision made into the highly inflamed organ by a thin sharp blade.

The only inconvenient result I have witnessed from this treatment was the following:—an incision was made into the testicle of a middle-aged man, with the usual relief, but in a few days the scrotum began to swell, great pain was experienced, and the man was taken into the hospital. The objectors to the mode of treatment suggested all sorts of disasters, in the shape of suppuration of the testicle, &c., but on careful examination it was ascertained that the swelling consisted of a large and rapid effusion of fluid into the tunica vaginalis, which was at once evacuated, with speedy relief to the patient. In another instance I made the incision much deeper than necessary, carrying the point of the knife nearly to the back of the organ. As much as ten ounces of blood were lost, but the testis was violently inflamed and swollen, and the only effect of the accident was to make the patient somewhat faint, but at the same time to give more speedy and effectual relief than usual.

This circumstance may lead one to the belief that the relief is solely due to the escape of blood from the puncture; but this view is inconsistent with the fact that great relief is given when only a few drachms of blood, mixed with serum, are discharged. Doubtless the direct withdrawal of blood from the highly inflamed testicle is of service, but my own view of the matter is, that the relief is in a great measure due to the withdrawal of the tension from the body of the testis by free division of the tunica albuginea.

Whatever may be the precise manner in which the good results are produced, there is no doubt of the fact, and I would earnestly suggest to surgeons, especially to my colleagues, the assistant-surgeons of the hospitals, who treat the majority of cases of orchitis, to adopt the plan proposed, rather than be submitting their patients in routine way to all the horrors of the middle passage, from tartar emetic to strapping of the testis.

Since the above was written I have seen one of my old pupils who has been spending the last six months in the Paris hospitals, and he informs me that the ordinary practice at the Hôpital de Midi in cases of acute orchitis is to make a puncture in several places with a lancet; the instrument is not carried into the body of the testicle, but simply through the tunica albuginea. He describes the plan of treatment as most successful.—*Lancet, Aug.* 6, 1864, *p.* 149.

DISEASES OF THE SKIN.

58.—ON ALOPECIA, OR FALLING OFF OF THE HAIR.

By Dr. J. Mill Frodsham, Physician to St. John's Hospital for Diseases of the Skin, and Senior Physician to the Farringdon General Dispensary.

In considering the treatment of alopecia we must remember that with the exception of the senile, which is incurable, the organs that secrete the hair are merely in a state of atony, and not destroyed; and, therefore, the main feature of the treatment must be to excite capillary circulation of the scalp, and thereby alter the vitality of the hair follicles. When the hair falls prematurely, as is especially seen in women, great care should be taken to find out the causes, among which may be enumerated: doing the hair too tightly, constantly in the same way, seldom undoing it, allowing the skin to become obstructed by the constant use of resinous pomades, &c.; also from want of æration, as constantly keeping the head covered. That resulting from accouchement and fevers has the additional complication of the rancid perspiration and local irritation of the pillow. These causes will themselves naturally suggest the treatment, which must be conducted on general principles, not neglecting local treatment. Much benefit will result from cold bathing the head, altering the mode of dressing the hair, allowing it to hang loose about the body. When the skin is unable to have free power of secretion from the too abundant use of pomades, &c., the head should be well washed with the yolk of an egg, or borax in almond emulsion, afterwards carefully rinsing with warm water, and to apply the following lotion, viz., spirit ammon. aromat., tinct. canth., glycerin, aa. ℥jss., aquæ rosæ, ℥vi. M.

A useful form of stimulating application is made by adding equal parts of the tinct. canth. of the P.D., and scented lard of the lin. canth. of the P.D.

In the form I denominated alopecia circumscripta, we require a purely stimulating treatment locally. The parts should be first well cleansed with benzole (Startin), and after the liq. vesicatorius of Messrs. Bullin and Birt applied with a hard, short brush, or the tinct. iodinii, twice the strength of the P. L. When the effects of these have passed away, one of the following ointments may be used: sulphur, 3 ss; hyd. nit.-oxyd, gr. v.; hyd. ammon.-chlor., gr. v.; combined with a few drops of creasote, and one ounce of lard. The internal remedies most to be relied upon are the mineral acids. A favourite form of mine is ℞. Acidi hydrochl. dil. 3iss; acidi nitrici dil. 3i.; æth. chloric. 3i.; inf. gentian.-comp. ℥viij. M. Fiat mist. cujus sumantur cochl. magn. ij. ter die.

I may here state the curative effects of these agents greatly depend upon their mode of application, which should be as follows: the hair, previously well washed, should be parted in straight lines, and the ointments or lotions applied by means of a hard brush to the skin, so freed until the whole of the scalp has been thoroughly gone over. In syphilitic alopecia the best internal treatment is the biniodide of mercury in small doses (potass. iodidi q. ij., liq. hyd. bichloridi 3j. aquæ ℥i.), and the local application at first should be some of the mercurial ointments before mentioned, and afterwards, if necessary, the blistering fluid of Bullen and Birt. In the alopecia resulting from chronic skin eruption—as eczema, impetigo, and pityriasis—our first object must be to cure the local disease. For this purpose an ointment, composed of the pyrolignous oil of juniper (huile de cade), sulphur ointments, zinc, and creasote lotions, should be used. Arsenious acid, with or without iron in the dose of $\frac{1}{10}$ of a grain, is the best internal remedy for these forms. It requires to be taken for at least a month, and it is, therefore, well to prescribe it in 3j. doses. If the eczema or pityriasis is confined to one spot, and resists treatment, recourse should be had to local blisterings.

In the case of tinea favosa, tinea tonsurans, the bulb of the hair becomes *modified from a parasite*. It is most important to cure the local disease as

quickly as possible, as in this the alopecia is permanent and extensive in proportion to its severity. In favus all internal remedies are useless and inconsistent, epilation, lotions of bichloride of mercury, and sulphur and tar ointments are to be used. This disease will seldom come under your notice, but tinea tonsurans very frequently. To cure it we must frequently have recourse to epilation and afterwards to the blistering fluid, or the tinct. of iodine, or bichloride of mercury lotion, or ointment composed of sulphur and creasote. By these means I have often cured the most inveterate forms. In pityriasis the creasote ointment and zinc lotions are the best treatment.

In epilation, care should be taken to have a proper pair of pincers, to hold the hand low down, and epilate in the natural direction of the hair, a proceeding which in this way causes little or no pain.

In all these forms the head should never be shaved. If required, the hair may be cut short around the diseased spots to admit of the more efficient application of the remedies prescribed.—*Medical Mirror, July*, 1864, *p.* 411.

59.—A CASE OF ALOPECIA AREATA TREATED BY CARBOLIC ACID.

By Dr. James Watson, Edinburgh.

[The patient was of feeble constitution, and Dr. Watson ordered her citrate of iron and quinine, cod-liver oil, wine, and good nourishing diet.]

As the scalp was nearly covered with a crust, the result of the free use of blistering fluid, I got the head poulticed for two or three days. When the crust was removed, the head had a somewhat œdematous look, otherwise it had all the appearance of a head bald from age. There were several patches of the scalp slightly reddened. I now began the application of carbolic acid dissolved in glycerine, in the proportion of one drachm of the acid to three ounces of glycerine. The lotion was applied night and morning; and, to prevent it evaporating, a cap made of oil-silk was constantly worn. The head was washed morning and evening, before the lotion was applied, with black soap and water.

Result of Treatment.—Under the constitutional treatment patient soon began to improve in appearance, and her general aspect assumed a healthier and happier character. The cheeks, which were formerly sunk, became full and plump, and the lips, once white and blanched, red and life-like. This change, however, was not immediate, but the result of eight or ten weeks' treatment.

The local treatment by carbolic acid was commenced about the 12th day of January. The first result observed was the disappearance of the œdema of the scalp, and a freedom from the uneasiness in the head, which succeeded the tic douloureux, and continued to annoy the patient till the lotion had been used for about two weeks. It was next noticed that the slightly crimson patches of scalp became pale, and the whole scalp white. The few hairs scattered over the scalp had now fallen out; and from ear to ear, and from the brow to the posterior aspect of the neck, there was not a single hair on the head. For a month after the lotion was used there was a slight furfuraceous desquamation of the scalp. At the end of six weeks from the commencement of the local treatment, the head was still quite bare, and it was only by the aid of a magnifying glass that it was possible to recognise openings in the skin which communicated with the fine canals leading to the hair bulbs, and distinguished this head from the baldness due to age. At the end of February, a crop of very fine, silken, white hair made its appearance over the whole scalp. It was perfectly free from pigment, resembling, yet differing from the hair of new-born children. At the end of March, the hair was about an inch long over many parts of the head, the white character of which gave to the otherwise young appearance of the patient a most peculiar aspect. The lotion was continued morning and evening till the end of June, since which date the *local treatment* has consisted solely in washing the

head night and morning with black soap and water, and the free use of a brush to the surface of the head three or four times in the course of the day.

Patient left the hospital on the 3rd day of August, 1864, with the scalp in perfect health, and with a covering of fine, glossy, silken hair, now very slightly tinged yellow, and measuring from one and a half to two inches long.

Remarks.—This case is interesting, inasmuch as it is, I believe, the first treated by carbolic acid. It is worthy of note that while the hair, which was plentiful, fell out, and in the course of fourteen days left the scalp entirely bare, the patient had not suffered, as generally occurs in these cases, at or about the time from any acute fever or inflammatory attack. The alopecia areata, although so complete as far as the head was concerned, was confined to the scalp; the eyebrows, eyelashes, hair over the pubes and axillæ remain intact. Although in this case I dissolved the carbolic acid in glycerine, it may be sometimes preferable to dissolve it in acetic acid; and when glycerine is used as the solvent, it may occasionally be wise to make the lotion considerably stronger.—*Edinburgh Medical Journal, Sept.* 1864, *p.* 236.

60.—ON THE TREATMENT OF IMPETIGO CONTAGIOSA, OR PORRIGO.

By Dr. W. Tilbury Fox, Physician to St. John's Hospital for Diseases of the Skin, and to the Farringdon General Dispensary.

[Impetigo contagiosa is another name for the more familiar porrigo or tinea, due to the presence of the achorion fungus.]

Clinically speaking, we find, in treating the disease—

1. That many cases get well if left alone; but this is a tedious affair.

2. That a more efficacious plan of treatment is that in which the internal administration of cod-liver oil and iron is combined with the local use of soothing applications—*ex.*, zinc ointment.

3. That a still more successful plan is to give cod-liver oil and iodide of potassium internally, and to apply externally sulphur ointment—a plan of treatment recommended and sanctioned by my colleagues.

I think we may safely conclude that the latter is the "basis" of successful treatment. It must be remembered that the disease occurs among the lower orders, and especially in delicate and strumous subjects; that it also follows vaccination, and is eminently contagious. Nature is able sometimes to carry on the suppurative and desiccative processes quietly, and to overcome further mischief by the establishment of convalescence; but there evidently is a low state of vital power, which antagonises this good effect, and leads to ulceration; hence we must always enjoin a tonic plan of treatment. At the outset, we may have a good deal of febrile action; this must be kept within due bounds by the ordinary means.

The local treatment is most important. The chief point is to remove all scabs (pediculi, according to my own observations, are rare). The scabs must be removed, and the medicaments applied to the subjacent sore surfaces. Sulphur ointment is the best. Solution of nitrate of silver, the solid salt itself, zinc ointment, iodide of potassium ointment, acetate of lead lotion, or benzoin may also be used. Sulphur has probably no very special charmed action in this disease, any more than in other cases—*ex.*, lichen, psoriasis, pityriasis, &c.

We must be careful to prevent all methods and modes of contagion; but, with the exercise of the greatest care, the disease will in all probability spread from one to several members of the same family.—*British Med. Journal, June* 4, 1864. *p.* 608.

61.—ON THE TREATMENT OF PRURIGO.

By Dr. Duchesne-Duparc.

Prurigo is not a disease which directly compromises the existence, but its obstinacy, its tendency to relapse, and the insupportable annoyance which it occasions, render it one which often causes much trouble and anxiety to the practitioner. Prurigo especially affects the nervous and the suffering. It has been supposed to be contagious, but this is certainly not the case; it may be complicated with itch, but when simple, no acarus or any other animalcule is present. What distinguishes the two diseases farther is, that the anatomical character of prurigo is the papule; of itch, the vesicle. Sometimes, no doubt, the diagnosis may be different. Thus, in chronic prurigo, some papules may present at their summit a small vesicle, and on the other hand, the vesicles of the itch may rest upon a papuliform induration; but even here the doubt should not be frequent, and should not last long, both because such cases are rare, and because the papules of prurigo occupy the back of the limbs and the posterior surface of the trunk, just the contrary of what is noticed in itch. The pruritus again affords light to the practitioner who is in doubt. This is the essential symptom of prurigo, and is much more marked and persistent than that which is met with in itch. Besides, the small lamellated crusts which follow the vesicles of scabies, could not be confounded with the black concretions formed in prurigo by drops of dried-up blood. The violent itching of prurigo may be simulated around the anus by ascarides in the rectum, by hemorrhoids, by slight inflammation of the rectum; and, in the case of the genital organs, by pediculi pubis, or eczema of the scrotum or vulva.

In a practical point of view there are only three forms of prurigo—the recent, the chronic, and that which was called by Alibert the latent, where papules cannot be seen by the naked eye. At present we only speak of the treatment of this disease.

If prurigo is recent, the antiphlogistic treatment affords the best results. If the subject is young, sanguine, and plethoric, and if the eruption occupies large surfaces, Dr. Duchesne-Duparc commences by bleeding at the arm, or applying leeches to the anus; he then prescribes a milk and vegetable diet, cooling drinks, especially lemonade, prepared with nitric or sulphuric acid, and sweetened with syrup. He lays much stress upon bran baths, prepared by boiling four pounds of bran for half an hour in a sufficiency of water, and mixing it with the water of the bath. Frequent lotions are made from time to time upon the affected parts with decoction of lettuce, dulcamara, or poppy; or the lotions are replaced by prolonged inunctions made with oil of hyoscyamus, opiate cerate, or the following preparation:—chloroform 75 grains, glycerine 300 grains. This treatment, combined with mild purgatives, often leads to a sudden and complete cure.

But if prurigo be, as is usually the case, of old standing, we must trust chiefly to external agents, though still insisting upon a suitable regimen and the use of acid drinks. The alkalies here form excellent topical applications; the salts of potash and soda, in sufficiently diluted solutions, both favor the resolution of the papules and calm the itching. With the same object we may have recourse to simple or aromatic fumigations, vapour baths, sometimes even to corrosive sublimate. But the topical application to which Dr. Duchesne-Duparc gives the preference is tar, in suspension or incorporated with lard and united with opium. Lotions and baths of tar-water should be frequently renewed. The following formula gives excellent results:—tar 60 grains, gummy extract of opium 15 grains, lard 1 ounce. In the case of baths and lotions it is well to add a gelatinous substance (such as 2 pounds of glue to 200 quarts of water), because the papillæ parts are the most sensitive of the skin. Dr. Duchesne-Duparc has found that sulphurous preparations are only moderately useful in prurigo. They only do good when it is complicated with pityriasis or psoriasis. The most useful of the mineral waters

are those which are alkaline, and contain a thick vegetable principle, such as the waters of Neris.

In prurigo pudendi, and in prurigo podicis, we must often have recourse to the application of prussic acid, sulphate or muriate of morphia, chloroform, &c. In some patients benefit is derived by the occasional use of astringents and antispasmodics; in others, slight and altogether superficial cauterisations do good. Nevertheless, though topical applications are very important, the necessity for internal remedies must not be overlooked, especially when the disease is of old standing. It is here that aconite is very useful. Many years ago, M. Cazenave derived great benefit from the use of this substance in papular cutaneous affections, and especially in prurigo. Considering prurigo as a neuralgia of the papillæ of the skin, having as an accessory character the appearance of papules, M. Cazenave had recourse to the simultaneous employment of alkaline baths, and the use of the following pills:—alcoholic extract of aconite, extract taraxacum, of each 15 grains. This is to be divided into 40 pills, of which the patient took one or two night and morning. Dr. Duchesne-Duparc has made use of similar pills, but generally prefers the tincture to the extract of aconite; where the case is very obstinate the use of the arseniate of iron and emollient baths are often useful. The prolonged use of the bath is especially important. Dr. Duchesne-Duparc often directs them to be of two or three hours' duration, and to be repeated daily. If, while the patients are undergoing them, friction of the affected parts of the skin, or, still better, an intelligent shampooing, be practised, a very beneficial modification of the skin will be brought about.—*Journal de Médecine et de Chirurgie Pratiques.—Edinburgh Medical Journal, Aug.* 1864, *p.* 165.

62.—TREATMENT OF ITCH AT ST. LOUIS.

By Dr. Hardy.

The treatment employed by Dr. Hardy consists, first, in friction for half-an-hour of the whole body, except the head, by means of black soap, and which is continued by the patient himself during an hour while in a tepid bath. When he leaves the bath he is submitted to a rapid and general friction with the following ointment:—Lard 64, sulphur 20, subcarbonate of potass and water of each 8 parts. The patient then dresses himself without wiping off the ointment, as the contact of this is necessary for the destruction of any remaining acari, and of any that might remain in the garments. Of 37,429 persons so treated in 1852–1862, only 535 required a second application.—*Gaz. des. Hop.*, No. 67.—*Medical Times and Gazette, June* 18, 1864, *p.* 665.

63.—TREATMENT OF PSORIASIS.

The Union Médicale publishes a treatment for psora, or to use a less scientific word, itch, which has been adopted by Dr. Vingtrinier in the prisons of Rouen, and which brings about a cure in the space of two hours. You are aware that M. Jadelot, of the Hôpital des Enfantes Malades, some years past, proposed a new method of treating the itch, which was by employing a bath prepared by adding to twenty gallons of water heated to about ninety-eight of Fahrenheit's thermometer, four or five ounces of the sulphuret of potash. This bath was to be used every day for five or ten days successively, according to the severity of the disease, the patient remaining in it nearly an hour each time. This plan has been much adopted in the French hospitals. It has the advantage of being less offensive than the common method, because the linen is not soiled with ointment, no sulphureous exhalation takes place from the patient, and the general habit is not impaired. *Fumigation has also* been employed in Paris with success, in the cure of

psoriasis, and this mode many physicians prefer to all others, particularly in very bad cases. The fumigation is produced by throwing half an ounce of sulphur mixed with two drachms of nitre into a warming-pan of hot coals, which is to be employed in the usual manner of warming a bed. The patient is then to strip naked and get under the clothes, which are to be closely tucked round his neck and shoulders so as to prevent as much as possible the escape of gas. The process is continued for about seven nights, and according to its inventor, Dr. Gale, even the worst cases may be cured in this way without any inconvenience to the patient, who will usually sleep sound. To arrive at a more rapid cure Dr. Vingtrinier proposes uniting the bath to general friction. He recommends: 1. Friction over the whole body with black soap during half an hour. 2. Simple tepid bath for the same lapse of time. 3. General friction with liquid sulphate of lime, leaving it to dry on the skin during a quarter of hour. 4. Immersion for half an hour in a bath prepared by adding to every one thousand grammes of water one hundred of flowers of sulphur, and two hundred of quick lime. On evaporating, the liquid sulphate of lime leaves on the skin a thick deposit which penetrates into the furrows and destroys the acari and their eggs. This method of treatment has been followed in Belgium for two years past, with great success, and it is recommended to all on account of its simplicity, economy, and the certain cure it will produce.—*Dublin Medical Press, June 22, 1864, p. 667.*

33.—ON THE USE OF ARMORACIA IN ACNE ROSACEA.

By Thomas Foster Ker, Esq., Manchester.

Acne rosacea is a disease which comes under the cognizance of the medical man, that very frequently baffles his skill, and is less under the control of medicine than many other skin affections. Any suggestion, therefore, in regard to its treatment will be acceptable to the profession, inasmuch as the number of sufferers in society from this complaint is far greater than might be at first imagined. So much unhappiness is it productive of that I conceive it to be the duty of any practitioner who has been so fortunate as to rescue "one victim," whose case was deemed hopeless, to bring such case in some way before the notice of his professional brethren.

Having seen the fresh root of horseradish, Cochlearia armoracia (nat. ord., Brassicaceæ), used externally as a rubefacient in catarrhal deafness and enlarged tonsils in the case of a patient suffering from a very obstinate form of acne rosacea, and to quickly disappear of itself with the advancing recovery of the patient's deafness and tonsillitis, my mind was at once impressed with the probable idea that the root would be found useful in other cases of the same character. I was thereby induced to make trial of it in several cases, and I have much pleasure in bearing witness to its remarkable success. Two ounces of the compound infusion of armoracia, with ten drops of compound spirit of ammonia, and a little syrup of ginger, made into a draught, to be taken night and morning; and a milk infusion of the fresh root to be used frequently as an application for the parts affected, and the avoidance of all stimulating liquors, with the occasional use of gentle aperients, will often prove the remedies appropriate to the treatment of this particular and obstinate disorder.

The following example, amongst other similar cases, illustrates the correctness of this account most strikingly:—

A clerical gentleman, a patient of mine, suffered particular annoyance from this disease for a period of five years. He had derived temporary advantage from the use of the solution of arsenic, four minims to be taken in porter three times a day. This plan always succeeded for a week or two; but the pustules reappeared invariably at the end of that time. They at length presented so marked an appearance as almost to induce a suspicion of a syphilitic origin—*clustered, very* acuminated, and bearing a rosy inflamed

base, occupying the apex of the nose, also a great portion of the right and left alæ. Both cheeks were patched with the acne as well. He is a tall and rather muscular person, with a sanguineo-nervous temperament. He had been very careful always of his diet, exercise, and abstained from stimulating liquors; but for the last few years he had had much mental excitement, more than his wonted habit, and had been also exposed to heated apartments and hot fires, from the effects of which he began to look anxious and pallid. His tongue was furred; his eyes were bloodshot; there was general debility; he became extremely impatient and irritable, though when quite well he was mild and enduring. His physical and mental prostration resulted in an attack of acne rosacea.

The case here stated was one of the worst I have seen, and was cured by armoracia only. This gentleman is now perfectly well, and progressed so favourably that I judged it unnecessary to order him change of air. This case alone affords a very striking and incontestable instance of the great efficacy of this simple remedy.—*Lancet, August* 13, 1864, *p.* 199.

65.—ON THE TREATMENT OF ECZEMA.

By Dr. T. McCall Anderson, Physician to the Dispensary for Skin Diseases, and to the Deaf and Dumb Institution, Glasgow.

[The following is a continuation of Dr. Anderson's lectures on this subject (see *Retrospect*, vol. 48, p. 110.)]

The preparations of *sulphur*, though not so generally useful as those of arsenic, are highly serviceable in the treatment of some cases of eczema, especially when the eruption occurs in persons of the lymphatic temperament, and when it is on the decline. When speaking of the administration of purgatives, I told you that the advantage which a sulphur purge possesses over a calomel one is, that it is eliminated in part by the skin, and exercises an alterative effect upon that structure. If, however, we wish to avail ourselves to the full extent of its alterative action, it is advisable to prescribe a course of one of the natural mineral waters which contain sulphur. Those of Harrogate and Moffat in this country, and of Enghien, Barèges, and Luchon on the continent, have the greatest reputation in this respect; and, while some of these waters may be had from the Apothecary, it is always more judicious, when it can be effected, to send your patient to the spring itself; for he is thus certain to get the waters fresh and pure, and, away from home and the fatigues and anxieties of business, his body is at the same time invigorated and his mind refreshed.

Alkalies are not nearly so generally employed as the preparations of arsenic and sulphur in the treatment of eczema. They are most beneficial when the patient is much addicted to the use of stimulants, and when there is a tendency to acidity of the stomach and to the deposit of lithates in the urine, or to gout or rheumatism. The preparation most in vogue is aqua potassæ, which may be given, largely diluted with water, in doses of twenty minims thrice daily to an adult. The alkali which I am most in the habit of using, however, and which has not, I think, been tried hitherto in this country for such a purpose, is the sesquicarbonate of ammonia in doses gradually increasing from ten up to thirty or even forty grains thrice daily, care being taken that the preparation is fresh and of full strength. A dose of forty grains is often borne well by a patient whose stomach has been gradually accustomed to its reception, while a smaller dose often occasions vomiting in the case of those who have not been in the habit of taking it. Sometimes it is well to continue the ammonia with Fowler's solution or one of the other arsenical preparations. If there is a decidedly gouty tendency, small doses of wine of colchicum (say ten drops), and in rheumatic habits, the acetate or bicarbonate of potash (in half drachm doses) may be added to each dose. The alkalies must be given *largely diluted* with water, and the dose must be *gradually increased till* the medicine disagrees or the eruption begins to fade.

Hydrocotyle Asiatica has been greatly extolled of late, especially by the French, in the treatment of eczema. It has been very little used in this country, however, although it seems well worthy of a trial, if we may judge from the high encomiums which have been passed upon it by our continental brethren. I have not yet tried it in Dispensary practice, owing to the narrow-minded policy of the French manufacturer, who sells each little bottle of granules at five francs; and, as there are about eighty granules in a bottle, it follows that every ten or fourteen days five francs must be expended, which is too much for the ordinary run of Dispensary patients. In private practice I have tried it several times, and apparently with benefit; but my experience of it is not yet sufficient to enable me to decide upon its merits. I would advise you, however, to try it in cases which resist the ordinary means of cure. The directions for the use of the granules are given in a paper which is enclosed along with each bottle.

Before leaving the internal treatment, let me lay before you four rules which you must carefully attend to in the employment of alterative medicines:—

1st. Let the dose, at first small, be gradually increased till the medicine disagrees, or till the disease begins to yield, and then let it be gradually diminished.

2nd. If the medicine disagrees, do not omit it altogether without very good reason, but try it in smaller doses, or in another form, or omit it for a few days till the bad effects have passed off.

3rd. To give it a fair trial, it must be continued for a considerable period of time, because in some cases the eruption does not disappear till after it has been administered for many weeks.

4th. Do not permit the patient to give up taking the medicine till some weeks have elapsed since the complete disappearance of the eruption.

If, as I hope, I have convinced you of the great benefit which accrues from the judicious selection of internal remedies in the treatment of eczema, and of their power, in many instances, of removing the eruption when administered alone, you will, perhaps, be hardly prepared for the statement which I make, as the result of some *experience*, that the *local treatment* is even more effectual than the constitutional, although it must be confessed that the applications made use of by many Practitioners in this country are unfortunately too often ineffectual, and not unfrequently injurious.

I shall not attempt a description of all the preparations in general use in the local treatment of eczema—some of them good, some useless, many hurtful—but shall endeavour to lay before you a short account of those which I have found most valuable, and, what is of the greatest importance, to point out, as far as possible, the indications for their use.

The first point in the local treatment of every eczematous eruption, without exception, is to remove all the crusts which have formed upon it. Till this is done, we can only guess at the condition of the parts beneath; our applications must, in consequence, be selected at random, and these cannot reach the diseased surface whose condition they are intended to modify. You will often meet with opposition on the part of the patient or friends in carrying this injunction into effect, either owing to their laziness, to their preconceived opinions, or to the pain which is sometimes experienced in the removal of the crusts. Patients come here, day after day, informing me that they have done what they could, but have only partially succeeded. You should, in such instances, repeat your instructions, and send your patient home again, for my invariable rule is to refuse to prescribe any local applications till the diseased surface is fully exposed to view, by which means much less time is lost in the end, and the subsequent treatment is much more satisfactory.

The removal of the crusts is a very easy matter, and each practitioner has his own favourite method of procedure. I usually recommend a poultice composed of crumbs of bread and hot almond oil to be applied to the eruption at night, and if the crusts do not come away with the poultice in the morning, the part is lubricated with fresh almond oil, and the crusts removed with the

finger nail about half an hour afterwards, when they have become thoroughly softened. When the crusts reappear, as frequently happens, especially at the commencement of the treatment, they must invariably be removed before the reapplication of the curative agent.

Let us suppose, now, that all the crusts have been removed, and the diseased surface fully exposed to view, what local applications are we to make use of?

If the eruption has just made its appearance, if the surface is acutely inflamed, if it is studded with numerous vesicles or pustules, but particularly if *burning heat* is complained of in place of itching, local sedatives must be employed. A very good application is a cold potatoe-starch poultice, a small quantity of a powder containing camphor being sprinkled over its surface before being applied, which relieves at once the burning heat.

℞. Camphoræ, ℥ss.; alcoholis q. s.; pulv. oxydi zinci; pulv. amyli, āā ℥iij. M.

Siq. Sprinkle a little over the part, or upon the poultice occasionally, to allay the burning heat. Let a small quantity be made at a time, and let the powder be kept in a stoppered bottle, as it loses its strength by exposure to the air.

Or, instead of poultices, emollient ointments may be employed, such as the simple or benzoated oxide of zinc ointment, cucumber ointment (Neligan), or cold cream. A mixture of powdered oxide of zinc and glycerine, in the proportion of half an ounce of the one to two ounces of the other, forms likewise a very soothing application, and to these may be added a little camphor if necessary.

℞. Camphoræ, ʒij.; pulv. oxydi zinci, ℥ss.; glycerinæ, ℥ij., cochinillini, gr. ij.; spt. rosarum, ℥j. M.

Siq. Stir the mixture before using it. Rub a thin layer over the inflamed part twice or thrice daily. A most elegant formula.

When the disease becomes chronic, as is indicated more particularly by the disappearance of the burning heat, and the supervention of itching, the local applications which are appropriate are very different, but even they vary according to the stage of the eruption.

If there is *infiltration* of the skin to any extent, the local treatment which I am in the habit of prescribing is that recommended by some continental dermatologists—in connexion with which the name of Hebra must always be honourably associated—and which has only of late began to peep forth in fragments in English Medical journals. This is the treatment by means of *potash* applications, which has been uniformly adopted at the Dispensary, and with great success. Having had the privilege some years ago of witnessing the carrying out of this means of cure in Hebra's wards at Vienna, some of the prescriptions may resemble very much, or even be identical with, those of that distinguished dermatologist, though I am unable to state at this moment which are due to him and which are mere modifications of my own. I trust, however, I have sufficiently done justice to his merits, and that you will acquit me of the desire of taking any credit, except in so far as the success of this treatment has been first thoroughly established in this country by my colleague, Dr. Buchanan, and myself.

The strength of the local application varies with the amount of the infiltration, and likewise with the extent of the eruption; for of course when the disease is extensive, it would be injudicious to make use of those very strong applications which may be applied with safety in the more circumscribed cases.

If the infiltration is slight, or the rash extensive, common *potash soap* (soft soap, black soap, sapo mollis, sapo viridis), or a solution of one part of it in two of boiling water, a little oil of rosemary or citronella being added to conceal in part the odour, may be used.

℞. Saponis mollis, ℥i.; aquæ bullientis, ℥ij.; olei citronellæ, ℥ss. M.

A piece of flannel dipped in this should be rubbed as firmly as possible over the affected parts night and morning, and the solution allowed to dry upon them, though it should be washed off before each re-application; or a

piece of flannel wrung out of the solution may be applied to the part, and left in contact with it all night if the patient can bear it.

A more elegant preparation is the *aqua potassæ* (Ed. Ph.), which may be painted over the eruption night and morning with a large brush, its irritant action being neutralised by means of cold water when the smarting becomes excessive.

Instead of soft soap or aqua potassæ, solutions of *potassa fusa* may be employed. In the mildest cases, with only slight infiltration, two grains of potassa fusa in an ounce of water; in the more severe, five, ten, twenty, thirty grains or even more. An ounce of water may be used, but I rarely resort to a stronger solution where the eruption is extensive. Even the solution containing thirty grains to the ounce, which may be applied in the same way as aqua potassæ, must be washed off with water very speedily, and the application should not be repeated oftener than once daily at the most. When such a strong solution is prescribed, and, especially if the eruption is extensive, it is advisable for the Physician to apply it himself, at first at all events. The solution has been used too strong, or been allowed to remain on too long, if it produces any manifest destruction of the skin. When the eruption is very limited and very obstinate, a much stronger solution may be applied, and Hebra sometimes uses a solution of one drachm of potassa fusa in two drachms of water, or even applies the solid caustic itself. This must be done, however, with the greatest circumspection, and the caustic washed off almost immediately, else it is certain to produce great destruction of the skin.

When these strong applications are used, and there is a tendency to the formation of fissures, it will be well to apply cod-liver oil or glycerine to the parts every night, by which means that brittle condition of the skin, which is so much favoured by the use of potash locally, and which leads to the formation of fissures, is in part avoided.

Instead of potassa fusa, some recommend solutions of chloride of zinc in similar proportions, but I have very little experience of it, being so well satisfied with the performances of the former. The following case, however, proves that it is a useful agent:—

Hugh D., aged about forty, saddler, came to the Dispensary for Skin Diseases, March 17, 1862. Small patches of eczema were noticed on the backs of his hands, sides of his fingers, and about his wrists. These were very itchy, with a good deal of infiltration; some of them studded with vesicles, and exuding a serous fluid, others dry and scaly. Although some of the patches were situated over the joints of the fingers, there were no fissures. A solution of chloride of zinc (℈j. to the ℥j. of water) was ordered to be painted over the affected parts morning and evening, and, if the action was too severe, it was to be moderated by the use of cold water. In the intervals between the applications he was to bathe the hands repeatedly in cold water.

March 24.—Greatly improved; itching nearly gone; infiltration of skin much diminished; serous exudation very slight, and only after the application of the zinc lotion.

The patient noticed a slight tendency to the formation of new vesicles on and around the patches, which was at once checked, however, by the lotion.

March 31.—Eruption gone.

When any of these irritants are made use of, they cause smarting, and, when stronger mixtures are applied, often considerable pain; but patients have informed me that, although the smarting and pain are severe, they prefer it to their old enemy, the itching. On the other hand, some patients, although this is rarely the case, will not submit to a repetition of the remedy. I was particularly struck with this in the case of a Medical man in this city, who consulted me some time ago about an extensive eczematous eruption of old standing, and for whom I prescribed the mildest of the applications above referred to. He told a friend shortly after that he had applied it once, and that it had nearly killed him, the fact being that he had been affected with eczema so long, and had tried *so many useless drugs*, that his faith in the effect of remedies *was shaken, and he would not give* a fair trial to a system of treatment which,

though a little unpleasant at first, would certainly have cured him. But Medical men are notoriously the worst and most refractory patients to deal with.

Having pointed out to you that the strength of the potash or zinc solutions which are employed varies with the amount of infiltration of the skin, it will probably have occurred to you that, when the eruption is extensive, and some of the patches much more infiltrated than others, a weak solution may be applied to the latter, a stronger one to the former; and it is equally obvious that, as the infiltration subsides, the solution may be gradually diluted.

Often, by continuing the use of a weak potash solution for some time after the infiltration is gone, all trace of the complaint disappears, but in most instances it is better to substitute for it one of the preparations about to be mentioned as the disease verges upon a cure. But if, on changing the local application, the infiltration of the skin reappears to any extent, you must at once have recourse to the potash solutions again. I have just one caution to give you before leaving this subject, namely, that you must be careful in the use of these solutions, and especially the stronger of them, in the case of infants, of delicate females, or of old and infirm persons, as the shock produced by their application may possibly be followed by serious results.

While these applications are being employed, cold water forms a very agreeable and useful adjunct. The affected parts may be bathed repeatedly with it during the day, and it is advisable that it should be allowed to fall upon them from a height. Sometime cloths wrung out of cold water may be placed upon the eruption with advantage in the intervals between the applications, and, if the rash is very extensive, much relief is experienced by plunging into a cold bath, or making use of the shower-bath or cold douche.

I have already pointed out that in mild cases the eruption is often kept up by the scratching alone, and that in these instances local sedatives have sometimes the effect of curing the disease by allaying the itching, and the desire to scratch the part. Hence you will understand how, in more severe cases, while the scratching does not of itself keep up the disease, it certainly tends to aggravate it and to make it more rebellious.

We must therefore exhort the patient to refrain from scratching as much as possible, and at the same time we must employ means to allay the itching. The potash and zinc preparations have certainly this effect in a marked degree, and so has the application of cold water (for the time); but sedatives and narcotics taken internally are not, in my opinion, of the slightest service except in so far as a large dose may produce sleep, and, when the patient has long been deprived of it, owing to the itching, this is much to be desired. Lotions of dilute *hydrocyanic acid*, in proportions varying from ℥v. to ℥j. (Ed. Ph.) in an ounce of water or glycerine, may be applied with advantage whenever the part is itching, instead of *giving way to the desire to scratch*.

When such a strong solution as ℥j. of prussic acid to the ℥j. of water is used, it must not be applied over a very extensive surface, and the patient must be warned that it is a very powerful poison. You will often find it of advantage to combine the prussic acid with one of the potash applications formerly referred to, in the proportion say of ℥x. to an ℥j. of the mixture.

℞. Potassæ fusæ gr. x.; acid hydrocyan. dil. (Ed. Ph.), ♁ j.; aquæ rosarum, ℥j. M.

Siq. Rub a little freely over the eruption night and morning, and when the itching sensation is severe.

Some prefer the use of cyanide of potassium in the form of ointment. For this purpose from five to ten grains may be mixed with cold cream, or the benzoated oxide of zinc ointment,

℞. Cyanidi potassi, gr. vj.; cerati galeni (Paris codex), ℥j.; cochinillini, gr. j. M.

Siq. Rub a little firmly over the parts which are itching, but let none of the ointment remain undissolved on the skin.

Tarry preparations are of the greatest value as local applications in the *treatment of eczema*, though they are of no use whatever when administered

internally. They have long been in vogue in this country, but have too frequently been used in a routine way, and without discrimination. You must therefore bear in mind the fact that they are chiefly of use in the declining stages of eczema, when the infiltration and itching are moderated.

Common tar (*Pix liquida*) is the application most frequently used at the Dispensary, on account of its cheapness, but in private practice you may employ more elegant preparations, such as the oleum rusci or oleum cadini (oil of cade). The latter, which is the product of the dry distillation of the wood of the *Juniperus Oxycedrus*, is manufactured at Aix-la-Chapelle, and you must be sure that it is got there, else you may have sent to you a liquid prepared from common tar.

Whichever of these preparations you select should be rubbed firmly over the eruption by means of a piece of flannel, and allowed to dry upon it. It should be applied thrice daily, and washed off as well as possible with soft soap, or, amongst the higher classes, with petroleum soap.

Several kinds of soap containing tar and oil of cade, under the name of tar and cade soaps (the latter should be obtained from Aix-la-Chapelle, but it is very expensive—3s. 6d. a cake), are used by some instead of these preparations in their pure state. If you employ them, you should use them like common soap—only rub them more firmly over the parts—and in many cases you will find it of advantage to allow the soap to dry upon the eruption.

I rarely employ tar alone, however, in the treatment of eczema, but usually combine it with one of the potash solutions, in which case you may use it before the infiltration has subsided; for while I told you that tar was most useful in the declining stages of eczema, I merely meant you to understand that it should not entirely take the place of the potash or zinc solutions while the infiltration was considerable. A most admirable preparation, one of Hebra's, and which is used to a great extent at the Dispensary under the name of "tinctura saponis viridis cum pice," and with the most charming effect, is a mixture of equal parts of common tar, methylated spirit, and soft soap, which should be used exactly in the same way, and as frequently as the simple solution of soft soap.

The following case, reported by my assistant, Mr. Arthur Jamieson, many similar to which you have an opportunity of seeing almost every day at the Dispensary, illustrates the beneficial effects of this mixture:—

Wm. S., aged forty-five, labourer, came to the Dispensary for Skin Diseases on Nov. 10, 1862. He was affected with "eczema impetiginodes" of both ears and the whole of the face, with the exception of the nose. He complained of intense itching, with considerable heat of the parts. The exudation, of a purulent nature, was abundant, and in many parts had concreted, forming large crusts which almost entirely covered the whole of his face. The infiltration of the skin was great; his general health pretty good.

He was ordered a calomel and scammony purge, and a mixture of oxide of zinc and camphor, in the proportion of gr. xx. of the latter to ℥j. of the former, was applied night and morning to the eruption after the removal of the scabs.

13th.—No improvement; ordered the tinctura saponis viridis cum pice.

24th.—Eruption completely gone.

In private practice, where expense is less an object than the elegance of the preparation, you may substitute oil of cade for common tar, and rectified spirit for methylated spirit, while a little oil of lavender may be added to conceal in part the disagreeable odour, or, instead of using soft soap at all, a solution of potassa fusa may be added to the mixture, the amount of the caustic potash depending upon the amount of infiltration of the skin.

℞. Saponis mollis; spt. rectificati; olei cadini, āā ℥j.; olei lavandulæ, ℥jss. M.

Siq. Rub a little firmly over the eruption night and morning, and wash it off before each reapplication.

The preparations of mercury and sulphur, justly esteemed in the treatment of eczema, are most beneficial when the eruption is verging upon a cure, when the infiltration and exudation are gone, and the itching moderated.

Of the mercurials, citrine ointment is my favourite application, though the red and white precipitate ointments are, perhaps, equally useful, or the "Unguentum hydrargyri iodidi" of the London Pharmacopœia. These may be used of full strength or diluted with lard, and, if it is indicated, a few grains of cyanide of potassium may be added to allay the itching. If a lotion is preferred, from one to four grains of the bichloride of mercury may be dissolved with the aid of a little alcohol and mixed with an ounce of rose water, while a little dilute hydrocyanic acid may be added if necessary, the solution being rubbed into the part two or three times daily. In using mercurial preparations locally, you must always bear in mind the possibility of their being absorbed in sufficient quantity to produce salivation; hence you must be careful in anointing an extensive surface, and should warn the patient to discontinue the application if the gums become tender.

It was only the other day that I ordered a lotion of bichloride of mercury (gr. ij. to the ℥j. of water) to be applied to the nose of a lady, and in three days, to my astonishment, salivation had occurred. On the other hand, I have repeatedly ordered stronger lotions to be applied to extensive surfaces for weeks without the occurrence of the slightest tendency to salivation, thus showing the peculiarities of different constitutions.

Of the preparations of sulphur, the common sulphur ointment, of full strength or diluted, and with or without the addition of cyanide of potassium, forms a very useful application. In some cases you may add a little bicarbonate of potash with advantage.

℞ Cyanidi potassi, gr. v.; sulphuris; bicarbonatis potassæ, āā ℥ss.; cochinillini, gr. i.; axungiæ, ad ℥j. M.

A drachm of sulphur mixed with an ounce of alcohol forms a capital lotion, but you must tell the patient to shake the bottle before pouring out the liquid, as the sulphur falls to the bottom. This should be rubbed firmly over the part night and morning, and allowed to dry upon it. If the patient is drinking sulphur waters at Moffat, Harrogate, or elsewhere, and especially if the rash is on the decline, he may combine their external use with their internal administration in the shape of warm baths.

When an ointment is employed in the treatment of eczema, you must give full directions as to the manner of applying it. A very small quantity should be melted on the point of the finger, and rubbed firmly into the affected part, and none of it should be allowed to lie undissolved upon the skin, nor, in most instances, should its colour be perceptible after its application; the surface should merely have the appearance of having been recently moistened with pure water. The part should always be cleaned with soap and water before reapplying the ointment; for if you smear layer after layer of it upon the skin, it becomes rancid, acts as an irritant, and is calculated rather to be prejudicial than otherwise.

Astringents are of use in some cases of eczema, such as the sulphate of zinc or copper in proportions varying from three to twenty grains in an ounce of rose-water or solution of the diacetate of lead, diluted with distilled water, but I rarely have occasion to use them, and I think you will find them inferior to the remedies previously described.

For the purpose of curing a very mild, or preventing a threatened attack of eczema, or obviating the occurrence of an immediate relapse, the skin may be washed occasionally with soft soap and water. In private practice, you may recommend the use of Hendrie's "Dispensary Petroleum Soap," which is sold at sixpence per cake, and which is one of the most delightfully perfumed soaps with which I am acquainted.

The following cases of eczema erythematodes is of value, as illustrative of many of the points of treatment to which I have adverted when the eruption covers an extensive surface:—

A gentleman from the West of Scotland, aged about forty, consulted me on November 9, 1861, about an eczematous eruption of great severity and of many weeks' duration. (He had had one previous attack, which lasted three years.) The parts affected were the neck, lower part of the abdomen, inner aspects of the thighs, and the arms and legs, especially on the flexor surfaces

of the elbows and knees. The eruption was bright red, and presented an erythematous surface, neither vesicles, pustules, nor papules being visible. There was no exudation from the abdomen or extremities. The skin of the neck, on the other hand, was much infiltrated, and from it serum exuded in abundance. The itching was severe. He was robust, without being corpulent, and with the exception of the eruption, was in perfectly good health. He was ordered to rub the inflamed parts firmly morning and evening with a piece of flannel dipped in the following mixture:—

℞. Acidi hydrocyanici, dil., ℥xl.; saponis viridis, ℥jss.; aquæ, ℥iij.; olei rosmarini, ℥j.

Cold water was to be frequently dashed over the parts, five drops of Fowler's solution taken thrice daily after food, and a farinaceous diet recommended.

November 12.—No change. Local application omitted, being too weak. The whole eruption was painted with a solution of potassa fusa (℥ss. to the ℥j. of water), which was washed off with cold water whenever the smarting became very severe. This was followed by the exudation of a considerable quantity of serum, especially from the neck. The patient was ordered to repeat this every two or three days, oftener or seldomer according to the severity of the application and the effect produced. The cold shower-bath was to be used twice daily, and the Fowler's solution to be continued.

In a letter, dated November 21, I was informed that the infiltration had quite disappeared from the arms, legs, and abdomen, and only some redness and itching remained. The infiltration, exudation, and itching of the neck were much moderated. He was ordered to continue the potassa fusa solution to the neck, and the following mixture was to be rubbed firmly over the other parts night and morning:

℞. Acidi hydrocyanici, dil., ℥xl.; olei cadini, ℥j.; saponis viridis, ℥ij.; olei rosmarini, ℥iss.; aquæ, ad ℥v. M.

The Fowler's solution, which agreed, was to be increased to seven and a half drops thrice daily. The bowels and kidneys being torpid, a teaspoonful of a powder containing sulphur, magnesia, and bitartrate of potash, was to be taken at bedtime.

On December 6th, patient stated:—"Since I last wrote, the complaint spread down the legs to the ankles. I have thus been affected from the ear to the foot, first and last. The strong application (potassa fusa, ℥ss., aquæ, ℥j.) checked the inflammation, and no exudation took place." The previous eruption he stated to be rapidly disappearing under the influence of the local applications, although the itching was considerable at times.

On December 30, 1861, only a little roughness and very slight occasional itching of the skin remained. The following ointment was to be applied night and morning:—

℞. Cyanidi potassii, gr. xii.; unguenti oxydi zinci benzoati.; unguenti citrini, āā. ℥j. M.

On January 9, 1862, the patient came to see me. The eruption was gone, and there was only a feeling as if the skin was not so elastic as natural. The local treatment was omitted, the dose of the Fowler's solution diminished to five drops thrice daily, and the purgative powder was only to be taken to relieve constipation.

January 1, 1863.—No return of the eruption. Treatment omitted ten months ago.

There can be no doubt that the local treatment was the most effectual in this case.

When the eczematous eruption occupies a limited extent of surface, it usually requires to be attacked by strong local applications, while it is not, as a rule, so much under the influence of internal medicine as when it covers a large area. In such cases, strong solutions of potassa fusa or chloride of zinc, or even these caustics in the solid form, may be employed locally in the manner and with the precautions previously described, and often with benefit; but you must remember to omit them whenever you have removed the infiltration of the skin.

Cauterisation with solid nitrate of silver may sometimes be resorted to

instead of the above, or the tincture of iodine painted over the part night and morning, and a poultice of bread and hot water applied about once a week to remove the red skin which forms a covering to the eruption, and prevents the new layers of iodine from coming in contact with the disease itself. But of all the local means for the removal of limited eczematous eruptions, none are equal to blistering them. This may be done by means of a solution of bichloride of mercury (℈j. to the ℥j. of alcohol), the fluid being painted over the eruption, and allowed to dry upon it. The action of the mercurial is, in this case, almost entirely local, and I have never witnessed any effect upon the system at large from its application.

The best blistering agent, however, is the glacial acetum cantharidis—that is, acetum cantharidis prepared with glacial acetic acid—the ordinary solution of the Pharmacopœia being too weak. It should be made in small quantities at a time, and kept in a good stoppered bottle, the stopper being removed for as short a time as possible, and, when not in use, covered with leather, otherwise its strength soon diminishes, and much annoyance is thereby occasioned. A little of this solution should be taken up by means of a paint brush, and painted firmly over the part till it becomes perfectly white. If the fluid is of full strength, and the skin thin, as on the face, it usually blisters it at once; but if the opposite holds, and especially if the head or palms of the hands are to be blistered, it may require to be painted over them for several minutes. After the skin is *thoroughly whitened*, a hot poultice may be applied to make the blister rise. One application is often sufficient to remove the eruption; but, if necessary, it may be repeated weekly, the crust produced by the previous eruption being softened with oil and removed before each reapplication.

A couple of months ago, a gentleman, aged about 35, and otherwise in perfect health, consulted me with regard to an eczematous eruption on the head, of twelve years duration, for which he had been repeatedly shaved, and had consulted many Physicians of eminence. Tar had been applied to the scalp systematically for some time, and every conceivable ointment had been used but without avail. After his hair was removed, I found that the disease corresponded to the form which I described to you under the name of eczema squamosum: it covered the whole head, and, as usually happens in these obstinate cases, was accurately limited to the hairy parts. The scales on the surface were numerous, the itching severe, and, on the crown, front, and sides of the head, the infiltration and redness of the skin were great. I blistered these parts with glacial acetum cantharidis, the fluid requiring to be painted on for some minutes, owing to the thickness of the skin, and ordered the rest of the scalp, which was less severely affected, to be painted with tincture of iodine morning and evening. In a fortnight the iodine was omitted, and when the crusts and scales produced by the iodine and the blistering fluid were removed, the scalp appeared perfectly healthy and without a vestige of the previous eruption. To consolidate the cure, however, tincture of iodine was painted over the whole head night and morning for a fortnight, and when the red skin was removed, the scalp looked remarkably well, there being not even the vestige of a scale, which can rarely be said even of the head of a healthy person. No other treatment was resorted to, and the gentleman has since been in America. In the interval his hair grew in greater force than ever, and he is delighted to be rid of his old and indefatigable enemy.

Many cases such as these might be mentioned, but I shall just refer to one more, which many of you had an opportunity of seeing:—A woman, pretty well advanced in years, came to the Dispensary a few months ago, to get advice about an eczematous eruption of old standing, which covered the whole of the palmar surface of each hand. She had likewise a tendency to eczema of the leg, which was removed by means of the "tinctura sapoins viridis cum pice," a preparation previously referred to. It is of the hands, however, I wish to speak. The eruption here assumed that form which I described to you as eczema rimosum, the fissures being very numerous and deep, and the infiltration of the skin great. Itching was mingled with the pain, but the

latter, on account of the fissures, predominated. Owing to the pain and stiffness, the hands were kept constantly in a semi-closed position, and she was unable to use them. I blistered each hand with the glacial acetum cantharidis, which had a marvellous effect. The eruption disappeared completely, and the patient returned with joy depicted in her countenance, and opened and closed her hands with perfect facility, not unmingled with pride. —*Medical Times and Gazette, July* 11, 1863, *p.* 31.

66.—CASES OF HOSPITAL GANGRENE.

By C. F. Maunder, Esq., Assistant-Surgeon to London Hospital.

[The disease known as "hospital gangrene" prevailed in the London Hospital during the latter part of 1863. It took its origin in the case of a young woman with an unhealthy sore on the dorsum of the foot. From such a case as this the disease may spread to a patient only separated by the distance of a bed or two, simply through the air. Besides this, the disease may spread from actual contagion, owing to the careless and indiscriminate use of sponges; or a disregard for manual ablutions on the part of the attendants. And even the conveyance of the poison from an infected to a healthy wound by the common house fly must be regarded as possible.]

This disease was no respecter of persons, attacking the old, middle-aged, and young of both sexes; destroying in its course the various tissues of the body, whether skin, muscle, blood-vessel, &c., &c., although it would appear to have a greater affinity for the integument, tending sometimes to burrow and to undermine the skin, and thus causing it to fall in and to be destroyed, than for other structures. Its local effects were suddenly and unexpectedly made evident, as in the case of the patient, the subject of anthrax, whose wound on one occasion was granulating and contracting, and eight-and-forty hours later was totally altered in character. Provided that the wound were small there was no constitutional sympathy; and even when a large surface was attacked, its effects upon the constitution did not appear to be marked by any peculiarity, prostration of the vascular system existing only in proportion to the size of the wound, and to its consequent debilitating influence, rather than as a result of blood-poisoning The functions of the important viscera were performed with tolerable regularity—the skin being normal and the tongue clean; pain (described as of a pricking, shooting character) was not severe in the majority of the examples which came under my observation, neither did the local inflammation run high; on the contrary, the type of the disease was decidedly asthenic, and attended in some instances by great vascular debility, without a corresponding degree of mental depression; indeed, in the minor cases, the aspect and general condition of the patient did not in any way lead to a suspicion of the local condition, an observation of the wound itself being absolutely necessary in order to establish the fact. Although no period of life appears to be exempt from this disease, it must be borne in mind that not one of the individuals attacked was in robust health; one was ill-fed; another, the subject of carbuncle; a third, young, but the operation to which she had been subjected had been followed by an attack of erysipelas; a fourth, healthy, but obese, and the operation had also been followed by erysipelas; while all had been in the Hospital at least a fortnight, and were consequently exposed to similar hygienic conditions. Of the regions of the body, no one part more than another appeared to be either specially obnoxious to or free from the ravages of this disease, for even the cephalic region, the trunk comprising in this instance the mammary region and the anterior-abdominal region, and both the upper and lower extremities, were alike attacked.

Whatever may be the source of this malady, whether in the atmosphere of a crowded ward, containing a large number of patients, the subjects of wounds, whose blood becomes vitiated through the respiratory organs, and so influences *the wound indirectly*; whether in the direct action of this

atmosphere upon the sore itself, or in both of these means conjointly; whether the cause always exist in some material form, or be generated by an accumulation of certain necessary and co-operating conditions; whether it be extraneous, and the result of an appreciable inoculation, or be autogenic, I am unable positively to determine. I incline, however, to the belief that it may take origin in two ways; either in a vitiated atmosphere, independently of the constant presence of any specific *materies morbi*, and that a wound affected by this former assumes certain characters, which we designate hospital gangrene, and becomes at once a fruitful source for the propagation of the complaint either by direct inoculation (by careless attendants, for example) or by the atmosphere taking up and depositing upon a previously healthy wound the germ of the disease now produced. Secondly, the disease may be introduced from without, as exemplified in the person of a female supposed to have been admitted with the disease upon her, and who thus gave origin to a scourge from which scarcely any surgical ward in the Hospital has been exempt. The belief that this scourge had an extraneous origin is supported by the opinion of Mr. J. G. King, who suspected the nature of the sore on the foot of the young woman already noticed, and promptly treated it, and to whose condition he referred me, on being asked if there were any wounds in the Hospital that had assumed similar characters to Cases 1 and 2.

At this period, too, Mary-ward, especially the west-end portion, was comparatively devoid of patients, and it may therefore be presumed that its atmosphere was comparatively wholesome. But, though it be possible that direct contamination will account for the spread of the disease in Mary-ward, yet such mode will scarcely explain its appearance in other and distant parts of the building, holding no direct communication with the above ward, and from this point at least its mode of propagation is at best conjectural.

On comparing the experience recorded by Guthrie (Commentaries on Surgery, p. 163, &c.), both of himself and others, of this disease, I find that some difference of opinion exists concerning its nature—whether local or constitutional—the majority being in favour of a local origin; and certain apparently sound arguments are used to support this theory, and instances are given as proofs. "Its character as a thoroughly contagious disease is indisputable. Its capability of being conveyed, through the medium of the atmosphere, to an ulcerated surface is also admitted, although some have thought that the infection was not always applied to the sore, but affected it, secondarily, through the medium of the constitution. Brugmans says that hospital gangrene prevailed in one of the low wards at Leyden, in 1798, whilst the ward or garret above it was free. The surgeon made an opening in the ceiling between the two, in order to ventilate the lower or affected ward, and in thirty hours three patients who lay next the opening were attacked by the disease, which soon spread through the whole ward."

"A wound," says Guthrie, "attacked by hospital gangrene, in its most concentrated and active form, presents a horrible aspect after the first forty-eight hours." Having detailed its character, he continues—"This gangrenous disease does not always prevail in this, its most concentrated form. The destroying process assume more of a sloughing than of a gangrenous character, whence Delpech has denominated it *pulpous*." This latter, rather than the more acute, is the form of the disease which characterised the cases under my observation, and this distinction is the more necessary because upon it depended the peculiar local treatment adopted, and to be advocated. South says, "My friend Arnott (South's Chelius, vol. i. p. 69) informs me that in January, 1835, in one of the female wards of the Middlesex Hospital three cases occurred which might be classed under the head of hospital gangrene." The character of the disease in these examples accords very closely with that of the recent epidemic, being of the *pulpous* form.

While discussing this subject it will not be irrelevant to notice briefly other forms of gangrene which might possibly be mistaken for the above *variety*.

1st. Sloughing phagedæna, which is very similar in its character to the above, but usually confines its attacks to the venereal or *soft* chancre, more especially when the subject of it has been previously contaminated by syphilis, or has been debilitated either by other diseases, by climate, or by mode of life. A simple wound, too, say on the lower extremity of an ill-fed, cachectic individual, will sometimes take on this action. The rapid sloughing of a cutaneous syphilitic node is another illustration.

2nd. Phagedæna, which is a milder form of the above, being less rapidly destructive, and also usually associated with venereal sores, whether hereditary, acquired, or tertiary.

Between these latter varieties and hospital gangrene an important distinction exists; gangrene being, I believe, both contagious and infectious, while sloughing phagedæna and phagedæna are possibly contagious, but certainly not infectious.

I think these last complications of wounds originate either in the sore itself, which is their seat, or in the then peculiar condition of the system reacting upon the sore, or possibly in both thus associated.

The *soft* or *non-infecting* chancre is prone to take on either of these latter complications.

Treatment.—The treatment adopted in the cases referred to was of two kinds—constitutional and local: the former being conducted in accordance with the indications present, and without reference to the peculiar condition of the wound; the latter had a direct and either a sudden and powerful or constant influence upon the sore. The remedies employed were *nitric acid* and *copious use* of *water* locally either singly or *seriatim*, and these I regard as the best means at our command for checking the ravages of hospital gangrene.

The use of these remedies has for its object either the sudden destruction of the whole secreting surface of a wound by means of a strong chemical agent, so as to form a slough, not only of the dying, but also, to some extent, of the subjacent living tissue, with the hope that on the separation of the eschar a healthy wound will result; or, the quick removal or dilution of the secretion from the poison-giving wound by the constant or frequent contact and change of water.

As to the comparative value of these remedies I am inclined to give the preference to nitric acid; it can be readily employed, its action is sudden and decided, and although the application of this agent causes temporary suffering, still the pain, inherent to the disease, subsides with that induced by the action of the acid. To be effectual, it must be applied to the whole affected surface and edges of a wound, and if seated on the exterior of the body a quarter of an inch of skin at the margin of the sore should be included in the eschar. Should the integument be undermined at any point, the instrument conveying the acid must be there inserted, that no particle of infected material may escape destruction, and remain ready to reinoculate the wound on the separation of the slough. When employed, this agent should not be used sparingly, and care should be taken that it penetrate the already existing sloughs, and involve some portion of tissue still living beneath them. If the patient be not under the influence of an anæsthetic, it will require some courage on the part of the surgeon to use this powerful agent thoroughly, especially if the wound be extensive, but he will be only doing his duty and be actually conferring a kindness upon his patient by acting energetically at once, rather than by an ill-timed sympathy with suffering, to run the risk of the necessity for a second application of the acid.

That a favourable result may follow the use of the acid, it must come into contact with *every* portion of the diseased tissues, and therefore, in some instances is manifestly inapplicable; while, on the contrary, in small, or even in large, superficial wounds, it is eminently appropriate. But a sore on the forearm, in which the deep fascia and inter-muscular septa had been destroyed, and the phagedænic action had travelled along the interstices of the muscles, had opened main arteries, and threatened to destroy the whole thickness of the limb,—may be regarded as an example of the disease in which the

use of nitric acid was inadmissible; this remedy, had it been possible, which it was not, to apply it to all the ramifications of the disease, would have been as destructive to the limb as the malady itself.

Constant ablution, the second remedy referred to, may be effected either by allowing water to run or drip continually upon the wound (irrigation); or by placing the member in a warm-bath; or by interrupted but frequent pourings of warm water over the affected part. The first and second methods are applicable to wounds on the trunk, arm, and lower extremity, while the bath may be employed for the hand and forearm. The water of the bath should be often changed, and however employed, may be medicated with some antiseptic agent—some preparation of chlorine or with Condy's fluid. This remedy should have a temperature agreeable to the patient's sensations, and is especially applicable to compound wounds in which various structures are involved by the disease in question—water will harmlessly follow the ramifications of the malady among important parts, to which nitric acid would be wantonly destructive. The length of time during which the water remedy was employed was determined by the condition of the wound. As soon as the sloughs had separated, and the diseased action had ceased, the bath was discontinued, and a preparation of chlorine usually substituted. In one instance, the bath was used thirty-one days. In another case, that of a boy, aged fourteen, under the care of Mr. Curling, with compound fracture of the leg, irrigation was employed three weeks.

In all cases, where practicable, a change of air is the grand *desideratum*—removal from one room to another; from one building to another; or from a town to the country, and especially the latter, will exert a salutary influence.

N.B.—Since going to press, a gangrenous stump has been most successfully treated by Mr. Curling with the *actual cautery*; the remedy having been thoroughly applied over the face of the stump, and in the interstices of the different tissues with the view of reaching and destroying every particle of the diseased structure.

I have also had the opportunity of verifying an important practical point—a distinctive character of *hospital* and *traumatic* gangrene. At a time when hospital gangrene exists in an hospital, every variety of sloughing is liable to be regarded with suspicion and anxiety, and I have watched with great interest three cases of wounds after operation which have been subjected to traumatic gangrene. Two of these were consequent on amputation, the third a flap turned up to favour the removal of a mammary tumour. In either case the portion of integument which was ultimately cast off was subjected to *desquamation of its cuticle*, a pathological condition which did *not* exist in any case of hospital gangrene which came under my observation.—*Clinical Lectures and Reports of London Hospital*, 1864, *p.* 114.

AFFECTIONS OF THE EYE.

67.—ON TREATMENT OF SQUINT BY OPERATION.

By J. W. Hulke, Esq., Surgeon to the Royal London Ophthalmic Hospital, Moorfields.

[This article is a review of 106 cases of squint treated by operation, with remarks upon the operation. In 50 cases of convergent squint Mr. Hulke adopted the operation in general use by his colleagues, and which he distinguishes as "the Moorfields' operation." In the remaining 49 he adopted the operation of Von Graefe; which operations he then proceeds to describe.]

In the Moorfields' operation, the eye-lids having been separated with a spring speculum, the assistant turns the cornea outwards, seizing, for this purpose, the conjunctiva boldly, close to the outer border of the cornea, with a rat-toothed forceps. The operator seizes, with a similar forceps, a small fold of conjunctiva, nearly midway between the inner edge of the cornea and *the caruncle, just* over the lower border of the tendon of the Rectus internus,

and snips it with slender blunt-pointed scissors, so as to make a small horizontal incision. The fascia, which this incision exposes, is in turn snipt through. Next a blunt hook, with rather a large curve, is passed through the wound beneath the tendon, which is put on the stretch by slightly raising the hook, and divided with small snips of the scissors between the hook and the sclerotic.

The advantages claimed for this operation are those which it possesses in virtue of its subcutaneous character, and the absence of that recession of the caruncle and undue prominence of the eyeball which are not infrequently observed where the conjunctiva has been freely divided.

I believe that these advantages are gained where care is taken to keep the conjunctival wound small, where unnecessary disturbance of the fascia is avoided by using a hook with not too large a curve, and by not sweeping the hook too widely in search of the tendon, where the tendon is divided strictly at its insertion, and by small snips without a wide separation of the scissor-blades, and where the fascia is only cut to the requisite extent for the perfect division of the tendon. Where there precautions are neglected, the Moorfields' operation loses somewhat its subconjunctival character, and should an unnecessary division of the fascia oculi have been made, retraction of the caruncle will hardly be absent when cicatrisation is complete.

The objections that the operation is not so easily performed, and that the complete division of the tendon is not so certainly known as where a free division of the conjunctiva and fascia over the tendon is practised, deserves little consideration; for the greater difficulty, if indeed it exists, is more than outweighed by the superior results of the subconjunctival operation, and because the surgeon may rest assured that the tendon has been thoroughly divided if he can bring his hook up to the edge of the cornea. The thrombus and ecchymosis, which are not very infrequent after this operation, are more real objections, though they may be partly avoided by making a counter-wound over the upper edge of the tendon, under cover of the upper eyelid.

In v. Graefe's operation the eyelids are separated, and the eye is turned outward by an assistant in the same manner as in the Moorfields' method. The surgeon makes a small opening in the conjunctiva, with sharp-pointed scissors (which are curved on the flat, and have an eccentric hinge), between the insertion of the tendon and the inner edge of the cornea, but rather nearer the latter, and not quite opposite the middle, but nearer the upper or lower edge of the tendon. He then slides the scissor-points beneath the conjunctiva, and snips a small hole in the fascia, through which he passes a hook (with a slightly swollen end and shorter curve than that used in the Moorfields' operation) under the tendon, which he stretches by pushing it on the convexity of the hook towards the cornea, and cuts at its insertion. The external wound need scarcely be larger than in the Moorfields' method, and when the operation has been well performed it as rarely inflames and suppurates. The button of granulations, which is not infrequently very troublesome after operations where all the tissues have been freely divided, occurred in only one of my 49 cases. I think there is less infiltration than after the Moorfields' method, the directness of the wound allowing the blood to escape readily. In both methods I think that the tendon is cut with equal certainty, and at the same point.

[After giving a tabular view of the cases the author describes "Critchett's operation" as follows:]

In this operation the conjunctiva is divided near the inner border of the cornea, and dissected in a flap, together with the fascia off the inner side of the globe. This liberates the tendon of the Rectus internus from its acquired faulty place of insertion. The Rectus externus is next divided, in order to allow a sufficient inversion of the cornea. Lastly, the wound at the inner side of the eye is closed with fine silk stitches, after removing so much of the free edge of the conjunctival flap, that the closure of the wound draws the sunken caruncle forward, and gives the cornea a slight convergence. The tendon of the Rectus internus which has been brought forward with the fascia

and conjunctiva, unites itself to the globe at an anterior point, nearer the situation of its normal insertion. The fine stitches give little trouble; they fall out in a few days, or they may be removed at the end of a week. Slight convergence is always desirable at first, because the cicatrix at the inner side of the eye always yields to some extent, and if at the time of the operation the vertical meridian of the cornea has only been brought to the middle of the lid, the final correction of the divergence will be imperfect.—*Ophthalmic Hospital Reports, Vol.* iv., 1864, *p.* 160.

68.—CASE OF EXTREME SQUINT CURED WITHOUT OPERATION BY THE USE OF PRISMS.

By Ernest Hart, Esq., Ophthalmic Surgeon to St. Mary's Hospital.

[Mr. Hart considers, from a careful study of the origin and nature of various forms of squint, that the treatment ought to be almost as much optinal as surgical; and that there is a large proportion of cases in which merely optical means succeed perfectly, either in averting the formation of a permanent squint, in curing it when the proper glass is applied early, or in preventing relapse after operation.]

J. C., aged twenty-seven, a sailor, of good general health, and had always possessed excellent sight. In May, 1863, he had a severe attack of rheumatic fever; various joints were attacked in succession, and he lay for nearly two months helpless in bed. Towards the end of the attack the eyes became affected: the light became painful to them; the eyeballs he describes as having become blood red, and acutely painful, the pain being severe over the brow, and darting through the head from the frontal region. The face was blistered by the abundant and acrid lachrymation. Thus there seems to have been an acute rheumatic ophthalmitis. He recovered well, and is not aware that there were any immediate traces left of the disease. He went a voyage to the Cape and came home in satisfactory health; but in the subsequent voyage in December, 1863, he became aware of a certain dimness and confusion in looking at objects, a difficulty in measuring distances, and in defining the outlines of things and persons before him. This came on, he thinks, quite suddenly; and in the course of the day he found that he had a decided convergent squint of the right eye, and that he had lost control over the movements of that eye. He consulted Mr. Hart in the middle of January last.

Mr. Hart found then a convergent squint, monolateral, of the right eye, permanent, and due to paralysis of the external rectus. The visual powers of the two eyes, tested separately, were very nearly equal. There was no dilatation or semi-dilatation of the right pupil; no drooping of the lid. The latitude of accommodation was equal to that of the left eye. The patient suffered greatly by the constant confusion arising from the double set of images which the incongruous eyes received. He was frequently unable to distinguish between the true image and its ghost, and, besides the giddiness and confusion thus occasioned, felt himself in danger in walking the streets.

Mr. Hart called the attention of the class especially to the case as one in which, from the positive and negative data above mentioned, the cause of the squint might be accurately determined. He referred it to insufficiency (paresis) of the right external rectus. The normal action of the other muscles of the eye and eyelid and of the iris excluded cerebral causes; while the absence of far-sightedness or short-sightedness in any marked degree, and the healthy appearance of the internal fundus of the eye revealed by the ophthalmoscope, excluded dioptric or retinal disorder from the etiology of the case. The cause being thus determined, it remained to decide on the remedy. Mr. Hart pointed out that in this case the cause of the double images which occasioned so much distress was that incongruous portions of the two retinæ were, by virtue of the displacement of the axis of the strabismic eye, impressed with the respective images of each object seen. This might be remedied, *then, by the use of* a prismatically ground spectacle-glass with the base

turned in the direction opposite to that of the squint; for such a glass has the power of causing a deviation of rays of light incident on its base, which may thus be used to deflect the rays proceeding from objects looked at, so that they may, in the case of the squinting eye, be made to fall upon a part of the retina congruous to that which receives them in the normal eye, and thus binocular vision be restored. In fact, a prism may be used thus to fuse and destroy double images, just as, conversely, with healthy eyes, it is sometimes used in sport to produce them. Taking a series of prismatic glasses ground to scale, Mr. Hart then essayed them with this patient before the class. A glass ground to an angle of twelve degrees placed before the deviating eye so altered the direction of the images which it received as to fuse the double image into one. This, however, would only have removed the visual inconvenience without curing the squint. By now selecting a glass of ten degrees the images could be nearly fused, but not quite; and then the horror of double images, which is instinctive, caused an involuntary effort of the semi-paralytic and enfeebled rectus externus, which just succeeded in drawing the eye so far further outward as to compensate for the diminished angle of the prism and fuse the double image. This, then, was the glass selected for the patient to wear; for by the aid of this glass the enfeebled muscle was, as it were, gymnastically exercised and strengthened. The fusion of images was not effected without an effort somewhat painful, and which could not long be sustained. Mr. Hart therefore directed the patient to employ the glass at intervals only during the day, gradually lengthening the period of exercise. In the course of seven days he had made considerable progress, could wear the glasses much longer at a time, and fuse the images more thoroughly and with less effort. He was then very anxious to rejoin his ship. Mr. Hart therefore furnished him with glasses of eight, six, and four degrees respectively, so that as the muscle became stronger a correspondingly greater effort might be required of it; the glasses fulfilling a true gymnastic as well as optical function, and acting just as dumb-bells of graduated weight, with appropriate exercise, may be made to act, in dealing with enfeebled muscles in other parts of the body. The patient was desired to communicate the result, or to show himself on his return. Recently the man presented himself quite cured. This cure was effected in the course of eight weeks. And in June he wrote to say that his eyes are now as good as ever, that the movements of the two are synchronous, and vision normal.

In another patient, now under treatment, in whom the squint was also monolateral and permanent, and on whom this method of treatment will be employed, the paresis of the abducens has followed on extreme debility after menorrhagia and lactation. Mr. Hart observed that it was essential to the good repute of this method of treatment that it should be employed with discrimination, and as the result of a careful diagnosis. It would be useless to attempt to cure by this means an alternating concomitant squint with hypermetropia, or a strabismus from active organic cerebral disease. But in every case of squint, the divergence of the eye must be regarded as a symptom, to be treated differently according to the nature of the various causes from which it arises. Sometimes a squint was the first indication of insidious meningitis in a child, of which he cited a remarkable case that he had lately seen in conjunction with Mr. Paul Jackson: then the opthalmoscopic indications are of the highest value for the purposes of general treatment, and the squint must be disregarded. Very frequently a slight squint or confused image, due to insufficiency of the synergic action of the ocular muscles, was the first symptom of an impending acute or chronic cerebral disorder, and the ophthalmic surgeon had to yield place to the physician. In either of such cases to operate would be useless cruelty. Another set of cases were those of which the above-quoted is a fair type. Then came the large class, so admirably investigated by Donders and Von Graefe, in which squint is due to hypermetropia of the eye leading to excessive contraction of the internal rectus: in such cases, the degree of squint being measured, the division of the tendon, carefully adjusted, was followed by the best results. But to make strabotomy yield the almost uniformly successful results which might now

be obtained from it, it was necessary to exclude all the cases which might be cured by other means, and also those which were incurable by any means. The application of a prismatic spectacle glass in the class of cases above defined was most successful, if carefully carried out and used for fit cases.—*Lancet, July* 30, 1864, *p.* 119.

69.—ON THE TREATMENT OF GRANULAR CONJUNCTIVITIS BY INOCULATION WITH PUS.

By C. Bader, Esq., Ophthalmic Assistant-Surgeon to Guy's Hospital.

[The treatment of granular conjunctivitis and of pannus by inoculation of gonorrhœal matter, was first practised in the Austrian army about 1812. The results obtained were very various, many were cured, some, however, were made worse by the treatment. Mr. Bader proceeds to give a short account of the different groups of cases treated by him by means of inoculation, and of the mode of operating.]

Mode of inoculation.—A small quantity (a drop) of the pus to be used for inoculation is placed with the tip of the little finger upon the conjunctiva of the lower lid of the eye which is to be inoculated, and is left there. The pus should be transferred from the person (child, &c.) from which it is taken while fresh. Pus, dried, will no more produce an effect. Experiments are made at the hospital at present by Mr. Henry Brietzcke, one of the assistants at the eye department, as to the possibility of preserving different kinds of pus in tubes. The kind of pus which is supposed to produce a mild kind of suppuration is easily enough obtained at a large hospital, but not so, perhaps, in military hospitals, where granulations are most prevalent. The object of the above experiments is to supply others with any pus desired. The fluid which surrounds the pus-cells seems essential for producing suppuration of the inoculated conjunctiva.

If only one eye be inoculated, the fellow-eye must carefully be kept closed; it may be untied once a week to wash it, taking care to bind it up again; the least particle of discharge from the inoculated eye would be sufficient to set up purulent ophthalmia. This can always be arrested if treated at the commencement.

The following seemed the most effectual mode for keeping the not inoculated eye closed and secure from risk of accidental inoculation. A thick layer of Canada balsam or gum mastic was spread over the skin of the eyelids, then wadding was laid on to the level of the bridge of the nose and margin of the orbit, and retained in position with a light bandage or strapping.

Treatment of the inoculated eye.—It consisted in cleanliness, by washing away the discharge from the eyelids with tepid water every hour, if the pain was very severe; twice a day (morning and evening), if there was moderate or no pain. Washing the eyes twice a day suffices as soon as the acute stage of suppuration has subsided. In severe cases of pannus it seemed desirable to obtain abundant suppuration; the purulent discharge was, therefore, not washed away for one or two days after suppuration had commenced. The lids becoming glued together, the pus accumulates upon the conjunctiva, and seems to occasion a more abundant discharge.

The unsatisfactory results obtained by some persons from inoculation seem in most cases to be due to a desire to check the purulent inflammation. A treatment which tends to check the inflammation (suppuration) arrests the destruction of the pannus, and of the substance which helps to form the granulations, and leaves the case in as bad, or in a worse, condition than it was before inoculation. Perforations of the cornea and changes in the eyelids were not specially attended to while the purulent discharge continued. A case was considered cured when the granulations were destroyed, the palpebral conjunctiva having assumed its smoothness. Operations for artificial pupil, changes in the conveyance of tears, distortions of the lids, should never be attended to as long as the granulations are not destroyed; the latter *frustrate* all attempts to improve vision, &c., &c.

All cases of granular conjunctivitis can be cured by inoculation; but if the entire or part of the cornea be transparent, the difficulty arises of producing such a suppuration as will destroy the granulations without leading to destruction or perforation of the cornea. In such cases one has to select the pus, taking it from purulent ophthalmia of children of a mild form. All cases of granular lids might be cured by pus from a gonorrhœa if some means of protecting the cornea were found. Those who adopt the treatment by inoculation ought to be familiar with what are called granular lids. Minute red granulations, giving the palpebral conjunctiva a red velvety appearance, must not be treated by inoculation. The large red, or gray red, or gray or yellow and opaque granulations, are those which will disappear when inoculated.

The term vascular cornea or pannus is applied to a cornea the surface of which is more or less overrun by blood-vessels passing upon it from the ocular conjunctiva. It is one of the results of the morbid action set up upon the cornea, by the contact of the granulations of the conjunctiva and of their secretions. The cicatrices in the conjunctiva caused by scarifications become much smoother and less conspicuous.

In cases where one cornea is vascular throughout, the other transparent in part, both eyes ought to be inoculated, since it has happened that the cornea of the worst eye became transparent and the granulations disappeared, but returned, the secretion from the uninoculated fellow-eye coming again in contact with the inoculated conjunctiva. It is better to cure the granulations in both eyes, to risk one eye and to have one good eye, than to render one cornea transparent, finally to have to inoculate the fellow-eye, and possibly have the cured eye reinoculated and lost. If both cornea are opaque and vascular, both eyes may be inoculated, and the patient may be treated as outpatient, informing him of the contagious property of the pus. If one eye is bound up, or if a case is inoculated where part of the cornea or the whole of it is transparent, it ought to be kept in the hospital until the chronic stage of suppuration has set in.

The duration of the acute suppuration lasted from three days to six weeks; the chronic suppuration from two to eighteen months. Some cases got well in two, others after eighteen, months, cleanliness being the *sole* treatment.

The symptoms of the acute stage of suppuration commence from three hours to two days after inoculation; they are itching, increased watering, photophobia, then redness and swelling of the eyelids, with chemosis and purulent discharge, reaching sometimes a very high degree, the pus streaming over the patient's cheeks for days; the cornea becomes more vascular, its surface becomes yellow; opaque portions of the vascular web on its surface slough away, its curve remaining generally unaltered. The pain is sometimes severe, preventing sleep for several nights. If it increases three to four days after the acute stage has set in, a perforation of the cornea is threatening; the acute symptoms gradually subside, the pain generally first. The acute stage is supposed at an end as soon as the patient opens the eyelids spontaneously.

Among different kinds of pus the following were used:—1. Pus taken from the urethra of a man suffering from gonorrhœa. 1*a*. Pus taken from an eye which had been inoculated with pus 1. 2. Pus from the eye of a child suffering from purulent ophthalmia, its mother having purulent discharge from the vagina at the time the pus was taken from the child's eye. 2*a*. Pus from an eye which had been inoculated with pus 2. 3. Pus from the eye of a child suffering from purulent ophthalmia, its mother having no yellow or suspicious-looking discharge from the vagina at the time the pus was taken from the child's eye. 3*a*. Pus from an eye which had been inoculated with pus 3. 4. Other kinds of pus—*i. e.* from catarrhal ophthalmia, from ophthalmia following operations, &c.—*Guy's Hospital Reports*, *Vol.* x., *p.* 62.

70.—ON REMITTENT OPHTHALMIA—STRUMOUS OPHTHALMIA OF CHILDREN.

By Henry Hancock, Esq., Surgeon to the Charing-Cross and Westminster Ophthalmic Hospitals, &c.

[Many years ago Mr. Hancock published some papers to show that what is commonly known as strumous ophthalmia does not depend upon that particular condition of constitution.]

The disease described under the titles "strumous," "scrofulous" or phlyctenular ophthalmia, is an affection of simple character, depending upon disorder of the digestive organs, influenced by the age of the patient, and presenting a type peculiar to diseases of the earlier period of life. True strumous or scrofulous ophthalmia is, as pointed out by Jacob, an affection of an entirely different character. Phlyctenular ophthalmia attacks children of all conditions, whether scrofulous or otherwise; it does not appear to be influenced by station in life, nor is it especially restricted to any particular colour of the hair or complexion; it attacks the fat, stout child, equally with the thin, and the highly-fed equally with the child of penury and want. In all cases the principal diagnostic signs of remittent fever are present; more urgent and well marked, it is true, in some instances than in others, but in all they may more or less be traced, if ordinary attention be paid to the subject. The remissions of severity; increase of fever and heat towards night; the subsidence thereof, and accession of perspiration; the eruption about the nose and corners of the mouth; picking the nose and lips; rubbing the eyes; fœtid, sour, and disagreeable breath; the strawberry tongue, covered with moist fur; the enlarged liver; the tumid, hard, and swollen belly; irregularity of the bowels, sometimes constipated, at others relaxed; the evacuations clay-coloured, or dark, or slimy, and mostly offensive. The tendency to eruptions over the face, head, and body, so invariably present in this form of ophthalmia, bears such strong affinity to the symptoms of the remittent fever of children, that I have long regarded the ophthalmia of children as a disease of the same character, and have consequently discarded the term "strumous," and "scrofulous," as applied to it, designating it as *remittent* ophthalmia.

The term "strumous ophthalmia," has heretofore embraced a large class of cases. Mere congestion of the palpebral, or of the ocular conjunctiva, with or without phlyctenular or pustular eruption; inflammation of the sclerotica or of the cornea; ulceration; suppuration, or sloughing; interstitial deposit between the layers of the cornea; certain affections of the iris; and in some rare instances, of the retina, if accompanied by intolerance of light, having been classed under this designation. I propose, however, in this and subsequent papers, to consider first, the more simple form, the remittent ophthalmia; secondly, the "phlyctenular, or pustular ophthalmia," and lastly, the "strumous."

Remittent ophthalmia is the most frequent and simple, depending upon derangement of the digestive organs; it may attack any child in whom such derangement obtains. It is for the most part confined to the superficial tunics of the eye, and is very frequently accompanied by cutaneous eruptions. It may exist for a great length of time, without producing any serious permanent results. The complaint will often be confined to the conjunctiva of the eyelids, the edges of which are loaded with a viscid secretion. In its more severe form, the disease extends to the ocular conjunctiva, the redness being restricted to one or more fasciculi of superficial vessels, extending from the back of the eyeball to the margin or front of the cornea, where they commonly terminate in phlyctenulæ, which are either absorbed, leaving superficial white specks when seated in front of the cornea; or, suppurating and bursting, result in superficial or deep-seated ulceration, occasionally penetrating the entire thickness of the cornea, allowing the iris to protrude; or, in other cases, destroying a larger portion of the cornea, and changing its brightness altogether into a yellowish slough. These two latter forms are *always succeeded* by permanent adhesion of the iris to the cornea, and the

formation of a cicatrix, which rarely disappears. The local symptoms in the milder forms, are pain; intolerance of light; redness; and the formation of simple pustules, with increased flow of tears. In the severer forms, ulceration, or even sloughing of the cornea will sometimes occur. In the majority of cases there is more of discomfort than of actual pain, although in some instances the pain is very severe. It is usually remittent, being absent during the day, recurring at night, and subsiding towards morning. In the early stages the pain is always sharp, darting, and intermitting; but when phlyctenulæ, pustules, or ulceration of the cornea exist, a continuous sensation of grit or sand in the eye obtains. The intolerance of light is a symptom so prominent and distressing, that many authors have been led to regard it as the cause, rather than the result of the disease. Experience, however, proves the error of this opinion; it is very true that with the subsidence of the intolerance of light a very marked improvement in the disease takes place; this, however, is not due to the subsidence of the intolerance of light, but to the removal of the cause which produced both it and the disease, the intolerance of light being merely a symptom of the disease, a sympathetic or functional affection, a morbid sensibility, depending upon disorder of the alimentary canal, and various secretions.

Although permanent and persistent in some, the photophobia is generally remittent; it appears to alternate with the pain, for whereas the latter is less urgent in the morning, and during the day, the intolerance of light is most severe at those times, subsiding towards evening, when the pain becomes more aggravated. The intensity of the photophobia, however, must not be regarded as any criterion of the severity of the disease; the less urgent and serious the malady, the greater, in most instances, will be the intolerance of light. A child may open its eyes when considerable disorganization has taken place, but when the attack is comparatively slight, it will make a great effort to exclude the minutest ray of light.

Treatment.—In the *British Medical Journal*, June 4, 1864, Mr. Cheshire, Senior Surgeon to the Birmingham and Midland Eye Hospital, in advocating the employment of tartarised antimony as a remedy in strumous ophthalmia, observes, "I am aware it has long been the practice of ophthalmologists to administer a single dose of tartarised antimony, as a beginning to the treatment of strumous ophthalmia, &c., &c., but it does not appear to have been resorted to as a remedy, *per se*, for the cure of strumous affections of the eye."

In the same Journal, for the 25th June, 1864, Mr. Price, of Margate, in eulogising Mr. Cheshire's notice, adds, "That the plan of treatment therein recommended, is both little known and little practised, I am fully convinced; and my object in again bringing the subject forward is to endorse the statements of Mr. Cheshire, in the hope of inducing practitioners to follow this mode of treatment on a more extended scale."

It is very evident that Mr. Cheshire and Mr. Price had either forgotten, or had not read my lectures published in the *Lancet*, in the year 1852, in which I gave the result of my treatment during several previous years of strumous ophthalmia (so called), by tartarised antimony, in opposition to the then existing treatment of that disease by tonics, and the local application of arg. nitratis, &c. Since the year 1849, I have not only steadily pursued this treatment, but strenuously recommended its adoption by the numerous surgeons who, from time to time visit the Royal Westminster Ophthalmic Hospital. Under these circumstances, I may be excused for again bringing forward the substance of the treatment I have so many years advocated.

The main objects to be steadily borne in mind in the treatment of remittent (strumous) ophthalmia, are, relief of the organs of digestion, and the correction of the secretions. If these are attained, the other symptoms will improve, and the eyes share in the general change for the better; but unless a systematic plan, embracing diet, clothing, good nursing, as well as medicine, be adopted, little benefit will result. As I have before observed, remittent ophthalmia may, as a general rule, always be traced to a disordered condition of the digestive organs, a want of digestive power, an incapability of properly assimilating the food. The liver is sluggish, irregular in its

action, and frequently enlarged; the mesenteric glands are commonly increased in size; the secretions are vitiated, and the blood consequently becoming impoverished and impure, the whole system is deranged. The child is rendered pale, weak, and fractious; its appetite is lost, or very capricious, and it is said to be strumous, weak, and delicate; but this condition need not become permanent, if the case be judiciously treated; if it is not at the outset erroneously supposed to depend upon cachexia or debility. The debility is for the most part more apparent than real, and proceeds from irritation, or of oppression rather than of actual weakness. Remove the cause, and the debility will disappear *pari passu*.

I will not here enter upon the local treatment usually resorted to, as I have long ceased to employ it in remittent ophthalmia, excepting where the cornea has given way and the iris protrudes; in such cases, touching the protruding iris with the pointed nitrate of silver and applying the extract of belladonna over the eyebrow are very useful. From repeated trials of the various local remedies recommended, I am convinced that they do more harm than good, causing great suffering without compensating benefit; indeed, children get well much more quickly when they are not used at all. The treatment should commence with an emetic of tartarised antimony, given as advised by Dr. Mackenzie, viz., in minute doses at frequent intervals, until free vomiting is excited; and this in all cases, however attenuated or however stout the child may appear, the object being to relieve the stomach of its offensive contents and to render its secretions more natural; to correct and restore the secretions of the kidneys, liver, and other glandular structures, and to influence the capillary system generally, so as to improve the condition of the skin and mucous membrane, and allay the morbid excitement of the nervous system.

This is best done by the tartarised antimony given as above, and it is very important that attention should be paid to the mode of its administration. The mere inducement of vomiting, the simple evacuation of the contents of the stomach is not sufficient, unless the medicine by being absorbed into the blood is enabled to produce other results; hence I have not observed anything like the same amount of good to result from sulphate of zinc, nor the ipecacuanha powder, or even from the tartarised antimony when the latter is administered in a single large dose to cause immediate vomiting. The following is the form which I usually employ:—

℞. Antim. tart. gr. iv.; syrup. ℥iv.; aq. cinnamoni ℥iij.; aq. distill. ad. ℥viii.

For a child under three years of age, two teaspoonfuls should be given every ten minutes until vomiting ensues; above this age, a table-spoonful should be given at similar intervals. It commonly requires four or five doses to produce the full effect, in some instances more, in a few less. This treatment should be repeated daily, until the intolerance of light begins to diminish, which it will mostly do after the first or second day, although in some of the more obstinate cases, four, or even six days will elapse before the desired result is obtained. When, however, the intolerance of light begins to subside, the emetic should be discontinued, and a powder of calomel and rhubarb, or mercury with chalk, and compound scammony powder every night, or every alternate night, until the tongue becomes clean, the abdomen softer and flatter, and the alvine secretions healthy; as these changes are brought about, the eyes will improve if attention be paid to the diet. When the attack is complicated with skin eruptions, aperients will be required for a somewhat longer period. Should the attack of ophthalmia be preceded by measles, small pox, scarlet fever, or any other complaint of a highly depressing character, or if the child has been exposed to excessive want, is much attenuated, and covered with a cold clammy perspiration, calomel, even in small doses, should not be given, except with great caution; in no instance should more than one dose be given without the patient being seen, as should ptyalism take place, such extensive sloughing of the fauces, gums, and even of the lips and cheeks may ensue as to destroy the child in a very few days. *I have known this* occur more than once. With such children it is safer to *abstain from* the use of calomel altogether and to substitute the compound

scammony powder, with taraxacum and henbane. When mercury, however, is especially indicated, I always combine it with quinine, taking care not to push the medicine to such an extent as to produce ptyalism.—*Journal of British Ophthalmology, Oct.* 1864, *p.* 35.

71.—TARTARISED ANTIMONY AS A REMEDY IN STRUMOUS OPHTHALMIA.

By Dr. William Price, Surgeon to the Metropolitan Infirmaries for Scrofulous Children and Adults, Margate.

A short but valuable notice, from the pen of Mr. Edwin Cheshire, appeared in a recent number of the British Medical Journal, on Tartarised Antimony as a remedy in Strumous Ophthalmia. That the plan of treatment therein recommended is both little known and little practised, I am fully convinced: and my object in again bringing the subject forward in these pages is to endorse the statements of Mr. Cheshire, in the hope of inducing practitioners to follow this mode of treatment on a more extended scale.

During the past six years, 109 cases of strumous ophthalmia, occurring in children under fifteen years of age, have been admitted into the Metropolitan Infirmary, at Margate. Out of this number, thirty-eight suffered with photophobia; and twenty-five had either vascular corneæ or phlyctenular ophthalmia. They had been all, or nearly all, treated by the administration of tartarised antimony in small but repeated doses; and, save in a few instances, with the most marked and decided benefit.

My attention was first directed to this plan of treatment in the Paris schools; and the success I there witnessed attending its employment, induced me early to forsake the more routine practice of giving cod-liver oil and tonics. It must not be supposed that I have overlooked the beneficial effects of sea-air and a liberal diet upon the London poor when removed to this salubrious climate; for the cases mentioned have been generally those in which the children had resided some short time by the sea-side. Amongst a large number of private cases annually coming under my care, I cannot call to mind a single instance in which tartarised antimony has been prescribed. From this circumstance I gather, too, that surgeons rarely resort to, or do not sufficiently appreciate its singular efficacy in certain cases of ophthalmia occurring in scrofulous subjects, in the adult as well as in the child.—*British Medical Journal, June* 25, 1864, *p.* 685.

72.—INOCULATION AND SYNDECTOMY.

By George Lawson, Esq., Assistant Surgeon to the Royal London Ophthalmic and Middlesex Hospitals.

The number of cases of severe granulation of the lids, accompanied with vascular corneæ, which have been treated by the inoculation of purulent matter at the Hospital, a short account of which, by Dr. Bader, appeared in the last number of the Ophthalmic Hospital Reports, prove indubitably the success of this line of treatment. It is, however, necessary that the disease should be sufficiently advanced before such a mode of dealing with it can with safety be resorted to. The greatest success has resulted in those cases, where the lids were not only severely granular, but where the whole cornea was completely vascular, semi-opaque, thickened, and the pupil scarcely visible; where, in fact, there was little if anything to lose, and all to gain. Such eyes will bear strong purulent matter, as the yellow pus from an infant with purulent ophthalmia, whose mother may have suffered from gonorrhœa, and after a rather long course of suppuration, will almost invariably recover, and good useful sight will be regained.

In the treatment of *granulations* by inoculation, much discrimination is

required in the selection of the cases, and in the quality of the pus which should be used.

It is best that the pus should be always in the first instance taken from the eye of an infant suffering from purulent ophthalmia, as it seems to me, that although gonorrhœal matter may in extreme cases answer very well, yet there is always a certain amount of risk as to its purity, whether some syphilitic virus may not accompany it, for since a chancre in the urethra is occasionally associated with gonorrhœa, it is difficult, if not impossible to say, that the patient from whom the matter is derived, is free from it. I prefer for this reason always to take it from the child.

The strength of the pus with which you wish to inoculate may be judged of: 1st, by the colour, and 2nd, by the severity and duration of the inflammation which it has excited in the eye from which it is taken. The yellow pus is always more active than the whitish discharge so commonly seen. Again, the period of the disease at which the pus is taken influences materially the amount of inflammation and suppuration it is capable of setting up. Pus from the eye of an infant in the early and most acute stage of purulent ophthalmia, will produce much more serious effects than that taken from the same eye at a later period of the disease, after it has undergone some treatment, and is on the decline.

Another point of practical importance is settled by the experience of the hospital.

Pus fresh from the eye of an infant gives rise to a more moderate suppuration with less œdema of the lids than when taken from an eye which is suffering from inoculation. The strength or virulence of the pus seems to increase in its travel through one or two eyes, so that if A is inoculated with mild purulent matter, the pus from A's eye will produce more intense ophthalmia in the eye of B than the original pus which inoculated A.

The most certain and effectual cure of severe or otherwise intractable granular lids undoubtedly is inoculation.

Many other remedies give great but temporary relief, and many cases, in course of time get, to a certain extent, well; but the only remedial agent which will for a certainty destroy the granulations and leave the lining membrane of the lids smooth within a definite period of time, is inoculation.

Now from experience we know that in the very severe forms of granulation, where the cornea has become a pannus and but little more than perception of light remains, this line of treatment is specially applicable, and that the results are most satisfactory, but in the large majority of patients who apply for hospital relief, the disease is not so advanced; and it is a question of serious moment whether it is not possible so to prepare the eye by previous treatment, that the milder cases may with safety be fitted for inoculation. This, I think, has been partially accomplished, and the cases I have now to record show that if a large portion of conjunctiva has been removed from around the cornea (syndectom), and the eye allowed thoroughly to recover, that the operation of pus in such an eye is considerably limited. This no doubt is owing to the purulent inflammation being principally confined to the conjunctiva lining the lids, for by the operation of syndectomy, one eighth of an inch of conjunctiva and sub-conjunctival tissue has been removed from around the cornea, so that not only is there a great diminution in the quantity of the mucous membrance on the globe which can become inflamed, but the sub-mucous tissue has also been destroyed, and a firm broad cicatrix spreads around the cornea, and serves as a barrier to arrest the extension of the suppurative action to it from the conjunctiva.

Case 1.—*Granular lids, with the upper half of the Cornea vascular, treated twelve months ago, by Syndectomy, but without benefit—Inoculation—satisfactory Result.*—E. R., æt. twenty-four, has suffered from granulations of the right lid for the last four years. Twelve months ago, on account of vascularity of the upper part of the cornea, the operation of syndectomy was performed, and a broad band of conjunctiva, with the sub-conjunctival tissue *removed*. She made a good recovery, and for a few weeks after it appeared *as if she was going* to derive benefit from the operation; the vascularity of

the cornea certainly diminished, but the granular state of the lid remained unchanged. She continued occasionally to attend the hospital as an out-patient, and used a mild astringent lotion. The vascularity of the cornea, in four or five months after the operation, was as great as before, and there was considerable photophobia. Steadily the eye became worse, and on September 17, 1863, she was again admitted into the hospital.

Her condition was then as follows :—The right lid, granular. The upper two-thirds of the cornea rough and vascular, but the lower one-third almost clear.

Sight.—Able to count fingers, but not to read any type. There is great photophobia and lachrymation.

September 29th.—The eye was inoculated with mild pus from the eye of an infant who had been under treatment for purulent ophthalmia.

30th.—Twenty-four hours after the inoculation the eye began to water and the lid to swell.

October 1st.—Eyelid much swollen ; free purulent discharge. Has had a great deal of pain in the eye during the night.

4th.—Profuse discharge from between the lids ; complains of a good deal of pain.

The purulent ophthalmia thus started, was allowed to proceed uninterruptedly, no local astringents were applied to arrest the discharge. The patient was only allowed to wipe the eye with a piece of linen dipped in tepid water, and keep the eye clean.

On October 20th.—The purulent discharge had sufficiently abated to allow her to leave the hospital, and attend as an out-patient. She had great photophobia, but very little pain. The cornea is entire.

After leaving the hospital, the discharge gradually ceased, and the sight steadily improved.

January 29th, 1864.—The patient to-day came to the hospital, and the following is her state :—

No vascularity of the cornea. It presents a general smooth surface, but is very slightly clouded. The lid is still granular, but very little when compared to its condition previous to inoculation.

She is able to read No. 12 of Jaeger's Test Types.

The eye will continue to improve, and probably the remaining granulations will diminish until they ultimately disappear. The cause of their not having been entirely destroyed is probably that the pus was not quite strong enough, and the period of suppuration rather too short.

Case 2.—Granulations of right Eyelid, Cornea Vascular, but with clear Interspaces—Syndectomy, followed two months afterwards by Inoculation—satisfactory Result.—S. W., æt. twenty-four, had been under treatment from time to time for eight years, on account of a granular condition of the eyelids, accompanied with entropion. For the relief of the entropion, the eyelashes had at last been excised, but the granulations of the right eyelid resisted all treatment, and the cornea had become rough, uneven in its curvature, and vascular, but with tolerably clear interspaces between the larger vessels.

In November, 1862, she was admitted into the hospital, and as the eye did not seem a favourable one for inoculation, Mr. Bowman performed syndectomy, removing a broad band of conjunctiva from around the cornea, and all the subjacent cellular tissue. At this time she was able to count fingers and see large objects, although she could not read any type.

After the first effects of the operation had passed away and the wound had healed, she certainly saw better, and could make out letters of No. 20. But this improvement was of very short duration, for the eye soon became very irritable, and the cornea more dull, and after a lapse of between three and four months, her sight was decidedly worse than before the operation.

She continued much in the same state until August, 1863, the amount of irritability of the eye would vary, at one time better, at another worse, but the vision remained unaltered. She could see an object moving in front of her right eye, but she could not with any certainty even count fingers at three or four inches distance.

August 21, 1863.—Was readmitted for the purpose of inoculation. The eye was inoculated on a Friday at 12 o'clock, with strong pus from a patient in the house undergoing inoculation.

At 11.30 on the following day the lid began to swell. Great œdema followed, but the purulent discharge was not at all excessive. After eight days, the swelling of the lid and the purulent discharge began to diminish, and at the end of the fourth week she was made an out-patient. At the time she left the hospital, there was still a good deal of discharge from between the lids. No treatment was adopted to arrest the purulent ophthalmia, it was allowed to run its course, but the patient, after the second or third day, was permitted to wipe away, with a piece of linen dipped in tepid water, the discharge from the lids, and so keep them clean.

February 11th, 1864.—I saw the patient this morning. The membrane lining the lid of the inoculated eye is perfectly smooth, and the sight has so much improved that she is able to read No. 12. The cornea is slightly nebulous but no longer vascular.

From long-continued irritation, its curvature is manifestly altered, and hence the benefit which her sight should derive from having regained so much of the transparency of the cornea is greatly curtailed.

Case 3.—*Granulations of both Eyelids with Vascular Corneæ—the Left Eye treated by Inoculation—the Right Eye first by Syndectomy, and then by Inoculation—good Result.*—The following case is specially interesting, as both eyes were almost in a similar condition when she first applied to the hospital, and two different modes of treatment were adopted. The right eye was inoculated, and the result was most satisfactory.

On the left eye, after an interval, syndectomy was performed, but with only temporary relief. The granulations continued as before, the cornea became a perfect pannus, and inoculation was then resorted to. Strong pus was used, but the activity of the purulent ophthalmia was much less than in the right eye, sufficient, however, to destroy the granulations, and to restore to the patient a useful eye.

Ellen R., aged twenty-one, was admitted into the hospital in July, 1862, in the following condition:—

The lids of both eyes very granular, the granulations large and fleshy. Both corneæ completely vascular. No photophobia. She could guide herself about, but was unable to discern features or read any type. She could merely distinguish an object in front of her.

The left eye was inoculated on 5th July, 1862, with pus from a child with purulent ophthalmia.

Severe inflammatory action followed with great œdema of the lids and purulent discharge. It was allowed to run its course unchecked, and when the activity of the inflammation had subsided, she was made an out-patient.

In October of the same year, the report states that the eye is perfectly quiet, the conjunctiva of the lids smooth, and she is able to read words of No. 6 Jaeger.

On October 10th, 1862, she was re-admitted for the treatment of the right eye.

Since the left eye had been inoculated, this eye had continued to get worse, and she had now little more than mere perception of light.

Syndectomy was performed, and one-eighth of an inch of conjunctiva and sub-conjunctival tissue removed from around the cornea, and close up its margin.

Free suppurative action followed this operation, and a semi-purulent discharge continued for many weeks after she left the hospital.

On December 4th, the report states that the discharge still continues. The sight is decidedly improved, and she is just able to count fingers.

So far the progress of this case was reported in the last number of "The Ophthalmic Hospital Reports," p. 64. For about two months afterwards the eye slightly but slowly improved. The discharge entirely ceased, and for a time she ceased her attendance at the hospital.

In July, 1863, she again became a patient. The eye had relapsed into its

former state; the granulations were large and abundant, and the cornea a perfect pannus. She was unable with that eye to read any type or to distinguish any object.

On August 8th, she was readmitted as an in-patient, and the eye inoculated with strong purulent matter from the eye of a man then undergoing the same treatment.

Twenty-four hours afterwards, free purulent ophthalmia was established. The discharge was much less in quantity than from the other eye when inoculated, but the lid was much swollen.

On the 29th, she left the Hospital. The lids were then in their normal state, the eye free from all pain. A moderate discharge still continuing, and able to read words of No. 20.

She has steadily improved, and, February 15, 1863, the report is, she can read No. 12, the cornea is cloudy, but no vessels to be seen on its surface. The lining membrane of the lid smooth, and the eye quite free from irritation. This eye will continue to improve.

She is able to do needle-work, and with the left eye to read a book, with No. 6, or Bourgeios type.

Case 4.—*Granular Lid of the Right Eye treated with Syndectomy, and afterwards by Inoculation.*—J. M., aged twenty-eight, admitted into the Hospital under Mr. Bowman, November 24, 1863. He was an old soldier, and went through the Crimean campaign. His eyes were first inflamed when in the Crimea, and since then they have never been well. The left eye has partially recovered, but in the *right* the conjunctiva of the lid is granular, the cornea opaque and vascular, the ocular conjunctiva congested, and he has great photophobia. He has perception of light, but he cannot count fingers.

November 24.—Mr. Bowman performed syndectomy, and removed a portion of conjunctiva from around the cornea, with all the corresponding subjacent cellular tissue.

25th.—He had but little pain, and slept well.

27th.—Complains of pain over the brow. There is no discharge.

December 1st.—The wound having healed, the eye to-day was inoculated with pus from the eye of a child four weeks old, suffering from purulent ophthalmia with thick purulent discharge, but the whole cornea quite clear.

4th.—Lids much swollen; profuse discharge.

8th.—Swelling of lids diminished; discharge less.

15th.—Discharge very slight; cornea becoming more clear; can count fingers. Left the hospital to attend as an out-patient.

January 15th, 1864.—Called to-day to show himself at the hospital. Slight haziness of the cornea; no vascularity; reads No. 8 Jaeger without glasses. —*Ophthalmic Hospital Reports, Vol.* iv., 1864, *p.* 182.

73.—CLINICAL REMARKS ON THE VALUE OF BELLADONNA IN THE TREATMENT OF IRITIS.

By Jabez Hogg, Esq., Assistant Surgeon Royal Westminster Ophthalmic Hospital; Editor of the Journal of British Ophthalmology.

After a careful examination into the relative value of the various agents for the cure of iritis, and opium in particular, I believe I am perfectly justified in saying that the chief value of the latter consists in its sedative or narcotic principle, when employed in sufficient doses, for the relief of the pain attendant upon the attack. If we take the trouble to examine a little closely into the published cases of alleged cures without mercury, or with opium, we shall find, remarkably enough, that in nearly all, at some period of the treatment another remedy, belladonna, has been employed, perhaps applied externally, in combination with some other potent drug. For example—on looking over the cases published by Dr. Williams, of Boston, we at once notice the secret of his success lies in this practice—he invariably commences the treatment "with an *instillation of a solution of atropine*;" this is

ordered to be repeated until complete dilation of the pupil is produced: at the same time iodide of potassium or some other equally efficacious remedy is prescribed in full doses. What better treatment in the majority of cases could be adopted? There are other practitioners, we fear, who, while professing to cure by the agency of morphia alone, do not possess the honesty of Dr. Williams, but are most careful to conceal what they employ in conjunction with their boasted remedy.

It is generally conceded that in all cases of iritis, the greatest possible danger to be apprehended is in the formation of adhesions between the iris and the capsule of the lens, posterior synechia, or between it and the cornea, anterior synechia; the latter is not of very frequent occurrence. In the acute stage of the attack lymph is effused, and the pupillary space wholly or partially obliterated in a comparatively short time, and thus vision will be quickly lost even should the choroid and retina escape the destructive process set up by the iritis. In milder attacks the danger of synechia and a fixed pupil cannot be always avoided, more especially if the attack is unheeded by the patient, or if not seen by a medical man during the first thirty or forty hours. Even if promptly attended to, the patient may not escape with a perfect iris; and therefore the case can only be said to be cured if no adhesions or irregularity of the pupil remain after the attack has been entirely subdued.

It is quite obvious, indeed, that our chief care in the treatment of this malady must be directed to preserve the iris intact, free from the smallest deposit, and quite circular. To fulfil this indication we must, from the very first, keep the pupil well dilated, and thus place it as much as possible out of harm's way and in a state of rest. To expect to effect this by any so-called constitutional remedies, without at the same time employing belladonna or atropine, is to neglect a most important point of treatment and jeopardize your patient's vision. Whereas by a liberal use of belladonna or a solution of atropine (2 to 4 grs. to the oz.), or a fiftieth part of a grain of a very finely levigated powder of the sulphate of atropine, applied to the conjunctiva of the lower eyelid, and repeated at intervals of three or four hours, until the pupil becomes fully dilated, and keeping it in this condition, we may, I believe, almost adopt any kind of treatment, or even leave the patient to careful nursing, particularly if the case be one of a non-specific uncomplicated nature. After the pupil has been fully dilated, the further application of atropine once daily is quite sufficient to maintain the full effect and keep the iris free from danger; but if lymph or interstitial deposits can already be detected, it will be found to be much more difficult to dilate the pupil, and it is under these circumstances that our good old remedies, mercury, iodide of potassium, &c., come into play with the happiest results, and will not only preserve sight, but save the iris from complete disorganization.

In England the value of mydriatics in the treatment of iritis has been very generally recognised, and always highly esteemed by our ophthalmic surgeons from a very early period. This is not the case in Germany, and some other countries. Dr. Mackenzie in his early writings says: "Belladonna ought to be employed in every case, and in all stages of the disease. The usual mode of employing it is in extract, moistened to the consistence of cream,* and liberally painted on the brow and eyelids morning and evening. As it is during the night that the disease appears to make most progress, and as during sleep there is a natural closure of the pupil, which must favour the permanent contraction which iritis tends to produce, the evening is evidently the most important time to apply belladonna. After blood-letting it will be found to act much more powerfully in dilating the pupil.

"As soon, in general, as the inflammation has subsided in any considerable degree, and the fibres of the iris have become somewhat freed from effused lymph, the pupil will begin to expand, and even in neglected cases, where it has been allowed to become almost obliterated, the continued use of bella-

* *Glycerine is the best vehicle for dissolving or thinning the extract, and possesses great advantage over water: it keeps moist for a long time when applied to the brow.*

donna for months is sometimes attended by a gradual dilation of the pupil, an elongation of the threads that bind it to the capsule, and a corresponding improvement in vision. After the acute inflammation is gone (subdued by extracting blood, &c.), a filtered aqueous solution of the extract of belladonna, or a solution of the sulphate of atropia, may be dropped into the eye morning and evening; applied thus to the conjunctiva, belladonna has more effect than when painted on the skin, and sometimes breaks through adhesions when smearing the outside of the eyelids has failed."

So little appears to have been known in Germany of the employment of mydriatics, that up to a very recent period we find it stated of a Prussian quack oculist, that he derived a large annual income from patients who literally flocked to him for the cure of cataract, &c. His treatment chiefly consisted in the application of belladonna drops to the eye, belladonna plaisters to temples, and mercurials. He kept his secret by supplying his patients with drugs from his own store. It was, therefore, high time some one better qualified for the task should fully satisfy himself of the value of belladonna; and accordingly Dr. Graefe, in 1856, published a paper in which he communicates the results of his experiments with mydriatics in the treatment of iritis, "the dangers of which," he says, "in general have materially diminished since the introduction of a bold use of mydriatic remedies in the acute form."

[There are some people in whom great irritation follows the use of this drug. No matter what preparation is made use of, the distress to the patient is equally great.]

In such cases (of iritis), as well as where there is any intolerance of the drug, I am in the habit of taking away a small quantity of blood from the temple by cupping, or by leeches applied either to the nose or lower eyelid: and should this fail, to paracentesis or division of the ciliary structures; especially the latter where there is diffuse cloudiness of the aqueous humour. Many of these to-be-apprehended evils are in part, if not wholly due to some impurity of the drug; at other times to the careless preparation of its solution, such as, dissolving the atropia in the strongest spirit of wine, or adding more spirit than is actually wanted to facilitate its perfect solution in the distilled water. Again sometimes very strong solutions are employed, which in my opinion are highly objectionable. In no case have I found it necessary to exceed 4 grs. to the oz., and more frequently I employ a 2 gr. solution. Nothing, indeed, answers so well as the latter: and if this cannot be borne, we shall find it advantageous to apply over the brow an ointment of the sulphate of atropine, or the belladonna liniment of the British Pharmacopœia, both of which are efficient preparations. Their action is of course much slower, but in peculiar cases this is decidedly an advantage over the effects produced by the more rapidly acting solutions, which may certainly be classed among the abuses of this valuable agent in those peculiar forms of eye disease requiring the application of mydriatics to affect the movements of the iris.—*Journal of British Ophthalmology, October*, 1864, *p.* 54.

74.—ON EXTRACTION OF SOFT CATARACT BY SUCTION.

By T. Pridgin Teale, Junr., Esq., M. A., Oxon., Surgeon to the Leeds General Infirmary.

During the last three or four years the treatment of soft cataract by "linear extraction" has to a great extent displaced the older and more tedious method of "solution."

In the latter method the surgeon, having broken up the lens with a needle, trusts to the natural process of absorption for removal of the opaque matter. In the former, either finding the lens completely opaque and soft, or in the case of partial cataract, having by a previous operation induced a cataractous condition of the whole lens, he proceeds at once to extract it through a linear wound in the cornea,—in fact, he coaxes out the softened lens through a small opening, by means of the curette.

When the lens is *nearly diffluent*, it runs readily along the groove of the

curette, and the operation is easily completed. Sometimes, however, although opaque, the lens is of firmer consistency, and does not readily escape along the groove. In this case it requires slight pressure to be exerted on the globe to aid its expulsion, and sometimes even the repeated introduction of the curette. Finding this difficulty in one or two of my own cases, in the autumn of last year I was led to inquire whether the principle of suction might not be made use of in withdrawing from the eye through a small wound all such cataracts, whether traumatic or spontaneous, as have neither a hard nucleus from old age nor have undergone calcareous degeneration. I believed, moreover, that if the posterior capsule could be thoroughly cleansed from all opaque matter, without undue violence to the eye, there would be much less risk of inflammatory mischief after linear extraction, and that one of the chief sources of opacity of the posterior capsule would be removed. Having therefore satisfied myself by experiment that the softer portions of even a healthy lens could be sucked through a fine tube, I requested Messrs. Weiss to make for me a suction instrument, which, being modelled upon the ordinary curette, I named a "suction-curette."

The suction-curette consists of three parts; a curette, a handle, and a suction-tube.

The *curette* is of the size of the ordinary curette, but differs from it in being roofed in to within one line of its extremity, thus forming a tube *flattened* on its upper surface, and terminating, as it were, in a small cup. The curette is screwed into the "handle."

The *handle* receives the curette and is hollow for a short distance, thus being a continuation of the tube of the curette. Passing out at right angles from this portion of the handle is a further continuation of the tube, to which the "suction-tube" can be fixed.

The *suction-tube* is a piece of india-rubber tubing, 10 or 12 inches long, having an ivory or metal mouthpiece at one end, and fitting on to the projecting part of the handle by the other.

Details of the operation.—The operation by suction which I have adopted is founded upon, and is essentially a modification of, "linear extraction," the principles of which have been worked out by Mr. Bowman, and have been clearly and fully described by Mr. Critchett and Mr. George Lawson.

First stage: *The efficient rupture of the anterior capsule.*—The pupil having been dilated by atropine, and the eyelids fixed by the stop-wire speculum, the anterior capsule of the lens should be very freely torn open by two needles passed through the cornea from opposite sides. In carrying out this step the surgeon should bear in mind that its object is not merely to liberate the cataract, but also to ensure such a tearing up of the anterior capsule that it may curl back from the anterior of the pupil and be lodged behind the iris. At the same time he ought by all means to avoid injuring the *posterior capsule:* a caution to be especially remembered in cases where the cataract is dwarfed and the anterior capsule tough, or where, in traumatic cataract, the lens has been much reduced in bulk by absorption. If the operator wishes to avoid the use of the two needles, he may rupture the capsule at a later stage by introducing through the corneal opening made for the curette the hook used in extraction of the hard cataract. The two needles, however, give more perfect command over this important step in the operation.

Second stage: *The opening in the cornea.*—Having withdrawn one needle, and steadying the eye by means of the other, the operator should next make an opening in the cornea for the admission of the tubular curette of the suction instrument. For this purpose a broad needle has been made for me, by Messrs. Weiss, of such a breadth as to make an opening of the exact size required for the curette. The needle should enter the cornea opposite the margin of the pupil when fully dilated, and, passing somewhat obliquely through the laminæ of the cornea, should make a valvular opening, in order, firstly, that it may not be too central and leave a scar in front of the pupil; secondly, that it may not be too near the attached margin of the iris, and thus favour its prolapse and adhesion to the wound; and thirdly, that the curette, when *introduced, may not* rest upon nor bruise the iris.

Third stage: *The removal of the cataract by suction.*—Having carefully introduced the curette (if it hitches in traversing the corneal wound, it may easily be disengaged by being turned edgeways), the surgeon should hold the open end of the tube steadily within the area of the pupil, gently burying it in the opaque material. The suction power may then be applied, and regulated in degree as the opaque matter runs off into the tube. As soon as the

Mr. Teale's Suction Curette.

pupil is clear, the curette may be *carefully* depressed towards the posterior capsule in order to ascertain whether any opaque matter remains, but it should not on any account be swept before or behind the iris. If the suction be continued after the opaque matter has been removed, the cornea is drawn down over the open end of the curette, and blocks it up, thus preventing the iris from being sucked into the instrument and injured.

If the operation has been efficiently performed, it will be found that the cataract has been completely withdrawn from the eye, through an opening

in the cornea no larger than would admit the common curette, without any injury to the iris, without rupture of the posterior capsule, and with such complete division of the anterior capsule that it has disappeared completely behind the iris. It will be also found, I think, in the majority of such cases, that recovery is most speedy, that the operation is followed by little or no irritation of the eye, that the patient on the eighth or tenth day can read No. 1 (Jäger), and that the conditions which usually produce opacity of the capsule have been provided against.

The foregoing rules apply to a simple case of complete soft cataract. They are also applicable, with slight modification, to cases of traumatic cataract of recent occurrence. In these cases, however, it is necessary, in the first place, to be very careful to tear open completely the anterior capsule which may have been previously ruptured in the accident producing the cataract; and, in the second place, to bear in mind that the posterior capsule may also have been torn through. Should this have occurred, the suction operation will be complicated by the admission into the anterior parts of the eye of the vitreous humour, which would tend to pass through the tube more readily than the denser material of the cataract. When such a defect occurs, it is sometimes possible, by careful management of the curette, to withdraw the opaque lens without at the same time drawing off a serious amount of vitreous humour.

Another complication may arise—namely, partial cataract, in which the nuclear portions of the cataract are opaque, and the cortical portions are healthy, tenacious, and adherent to the capsule. This difficulty must be met in the same way as in "linear extraction"—by the preliminary operation of puncturing the anterior capsule, so as to admit the aqueous humour into the structure of the lens, and so to cause its disintegration. It may be possible to withdraw by suction even a partially sound lens without the preliminary disintegration; but I have not yet attempted to do so, not from any difficulty in drawing such healthy lens through the tubular curette, but because, when the peripheral parts of the lens are transparent and adhere to the capsule, it is hardly possible to ascertain when the capsule has been completely cleansed from lenticular matter.

Another class of cases presenting difficulties is that in which a soft cataract has become wasted and calcareous, or partly so. In such cases the solid portions will not pass along the tube of the curette.

The following points seem worthy of notice:

The extreme point of the curette should be well rounded, and not sharp.

The *cup* (open extremity) and the *tubular portion* of the curette should be of nearly equal calibre, so as to pass readily through the small opening in the cornea.

The curette should be made with a *broad-milled rim* in front of the screw, to facilitate the screwing into the handle.

On former attempts to extract Cataract by Suction, and their failure.—When I first devised the suction-curette, I was under the impression that I had hit upon a new idea, and that the proposal was original. By the kindness of several friends, however, I have been directed to accounts of various previous attempts to apply the same principle to the extraction of cataract.

The Persians, ages ago, are said by Avicenna to have sucked out cataracts through a hollow needle. How far they succeeded I am not able to state.

In 1847, M. Laugier invented his "aiguille à pompe," a hollow needle fixed in a syringe, apparently like that now in use for subcutaneous injection. Its use is discussed by M. Desmarres. The needle having been thrust through the sclerotica, vitreous humour, and posterior capsule, and lodged in the centre of the lens, the suction was applied by means of the syringe in the handle of the instrument. If the cataract were fluid, it was drawn into the instrument; the pupil became clear, and sight was immediately restored. If the cataract were not fluid ("et la cataracte liquide est fort rare"), the vitreous humour was drawn out, the cataract was left *in situ*, and the eye collapsed. This misadventure was followed by internal inflammation of the eye, and in consequence the operation was condemned. "En résumé, l'opération de la

cataracte par succion est abandonnée." Failure in this operation was to be expected from using the needle of the syringe as the piercing instrument, and from traversing the sclerotica and rupturing the posterior capsule, which ought to have been preserved as the barrier between the cataract and vitreous humour.

Again, M. Blanchet brought forward another method of extracting cataract by suction. Having dilated the pupil, he made an opening in the cornea with a broad needle, through which he introduced a small tube with a flageolet-like mouth attached to an Anell's syringe. With this blunt tube he pierced the capsule of the lens; and if the cataract proved soft, he pumped it out through the tube by working the piston of the syringe. The main defects of this plan of M. Blanchet were—first, the attempt to puncture the anterior capsule with a blunt instrument, thereby using unnecessary force in reaching the cataract; and secondly, the imperfect opening of the anterior capsule, whereby the capsule remained in the area of the pupil, and, becoming opaque, rendered a secondary operation necessary.

On Suction Instruments.—The original suction instrument which Messrs. Weiss made for me consisted simply of a tubular curette fixed in a handle, to which a small india-rubber tube* with a mouthpiece is attached. The flexible tube is of such a length as to reach from the mouth of the operator to the curette when held in the eye.

Shortly afterwards Messrs. Weiss made, at the suggestion of Mr. Bowman, a modification of this instrument, in which the suction power is applied by an ingenious mechanism in the handle, so that the hand which holds the curette controls the suction. Mr. Bowman also inserted a piece of glass tube between the curette and the handle, to enable the operator to watch the result of the suction.

A third instrument has been suggested and made for me by Messrs Weiss, which is simply a light glass tube with the tubular curette fixed at one end, and the flexible tube with a mouthpiece at the other end.

A fourth instrument has been made for Dr. Bader, of Guy's Hospital, by Krohne, of Whitechapel, and is thus described by Mr. Lawson :—"The suction-power is a small, hollow india-rubber ball, placed at the extremity of a tube which terminates in a glass tubular curette. Pressure is made on the ball with the hand to expel the air from the tube, and its readmission is regulated by a well-contrived stop apparatus placed close to the curette. After the air from the ball has been expelled and its readmission prevented by closing the stop, the curette is introduced into the eye, and the amount of suction is regulated by a little trigger connected with the stop apparatus within."

Having used the first three forms of instrument I have found them to do their work perfectly; and I have no doubt that Dr. Bader's is at least equal to them. On the whole, perhaps from having used it more frequently, I prefer the original curette (with the addition of the glass tube), as the suction is more immediately at command when applied by the mouth, and the instrument can be guided with greater delicacy when the hand is not fettered by applying the suction power.—*Lancet, Sept.* 24, 1864, *p.* 348, *and Ophthalmic Hospital Reports, Vol.* iv., *p.* 149.

75.—DESCRIPTION OF A NEW INSTRUMENT FOR THE EXTRACTION OF CATARACT.

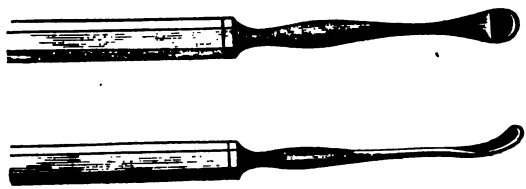
By G. Critchett, Esq., Surgeon to the Royal London Ophthalmic Hospital.

[The object of the present paper is to bring before the notice of the profession a little instrument lately contrived for the extraction of cataract. Mr. Critchett calls the little spoon recommended, the "Vectis Spoon."]

Some time ago an important modification in the usual method of extracting a cataract was proposed by Schûft of Berlin, which I must briefly describe :—

The idea of the flexible tube was probably suggested to me by reading an article by Mr. Greenway, of Plymouth, in which he describes a suction instrument for making artificial pupil.—Medical Times and Gazette, Dec. 15, 1860.

The eye having been exposed by an ordinary wire speculum and fixed, a keratome, or spear-shaped knife, is introduced near the margin of the cornea and made to traverse the anterior chamber so as to form a slightly curved slit or opening, about three and a half lines in extent; a portion of iris is then drawn through the corneal opening and cut off. The capsule of the lens is freely opened, and then the most important and critical part of the proceeding commences– viz., the removal of the lens. This he proposed to accomplish by a peculiar form of spoon with perpendicular dished edges. This spoon is carefully introduced behind the lens and then withdrawn so as to bring away a spoonful of lenticular matter; and this proceeding is repeated until, as far as possible, the entire cataract is removed. If this can be accomplished without the loss of vitreous humour, without bruising the iris, and without leaving fragments of the opaque lens in the eye, and probably concealed behind the iris, I am strongly of opinion that the chances of success are decidedly greater, and the anxieties connected with the after-treatment of the case far less, than when the old method of extraction by means of a large semicircular flap is employed. Some considerable experience of this operation, dating back from its first proposal by Schüft, whilst it has convinced me of its great value in many cases, has also impressed upon me several important objections and sources of failure, most of which are removed by the form of spoon that I wish to substitute. In the first place, Schüft's method is inapplicable to the hard amber senile cataract: it is impossible to pass the thick spoon behind the posterior surface of the hard cataract without rupturing the hyaloid membrane; and even if this were accomplished, it would be scarcely possible to remove a bulky hard lens, together with a thick spoon occupying considerable additional space, through a slit in the cornea. In the second place, even in cataracts of medium density, there is often considerable difficulty in passing this form of spoon behind the lens; it is apt to become early entangled in the substance of the cataract before it has passed behind its posterior surface, to break it into fragments, and to *push portions into the space behind the iris*, where they remain concealed until the aqueous humour gradually floats them into the pupil, where they excite irritation, and either retard the progress of the case or even seriously compromise the eye. Such being some of the more serious objections to this form of spoon, I was led to adopt what I consider to be an important and valuable modification. The indications were as follows:—An instrument that by its form would glide between the posterior surface of the cataract and the hyaloid membrane without resistance and without becoming entangled in the substance of the former, that would adapt itself accurately to the posterior surface of the lens, and that on its gradual withdrawal would bring the cataract, or at any rate the firm nuclear portion of it, away unbroken and without taking up more



space than a thin metallic layer. A glance at the peculiar form of the Vectis Spoon (see figure) will show that these three important indications are accomplished. The thin edge facilitates greatly the gliding of the instrument behind the cataract, as it offers no resistance whatever, but makes its way as a boat moves through the water; having passed behind the cataract, it forms a concave bed, in which it may rest, and to which it accurately moulds itself; and when the withdrawing movement begins, the edge, which offered no resistance as long as the instrument was passing behind the lens, now slightly imbeds itself in its substance and earns its name of "Vectis" by drawing the cataract easily and surely through the corneal opening. I

have now employed this instrument on several occasions for some months, and, as a practical man, I am strongly impressed with its value and with its great superiority over the Schûft spoon. It enables us to deal with all varieties of cataract, however dense (I do not of course include soft cataracts), and I venture to predict that when this mode of operating is fully understood, the safety and facility in its performance will be recognised as so much increased by the Vectis Spoon, that the old method of flap extraction will be nearly, if not entirely, superseded.—*Lancet, June* 11, 1864, *p.* 666.

76.—A CASE OF CONGENITAL CATARACT TREATED BY IRIDESIS, IN WHICH SOME MODIFICATIONS WERE INTRODUCED IN THE OPERATION.

By G. Critchett, Esq., Surgeon to the Royal London Ophthalmic Hospital.

[Cases illustrating the method of practice pursued in the case of cataract related in this paper have been published before, by Bowman, Hulke, and others; and iridectomy has been employed in similar cases, by Von Graefe.]

It becomes an interesting practical point to determine whether the formation of an artificial pupil in nuclear congenital cataract places the patient in a more advantageous position than if the cataract were removed, and if so, whether iridesis or iridectomy is the more useful operation. It is also necessary to ascertain, as precisely as possible, the limits and exact conditions under which the method of proceeding is indicated, rather than the somewhat more severe and radical method of removing the cataracts. I am induced to bring the following case before the notice of the profession, because it presents some points of peculiar interest, and because, in the course of the treatment adopted, it brought out some important practical results, that may be of use in determining the questions to which I have in the above remarks drawn attention. Early last year, 1863, a young gentleman, the son of a clergyman, æt. twenty-two, consulted me respecting his sight. The account that he gave of himself was as follows—I quote his own words:—"As far back as I can remember I have been conscious that my sight was defective, that of the right eye has always been the worst. Indeed, I cannot recollect that the right eye has ever been of any use to me. I knew my left to have been always defective from the fact of never having been able to distinguish the features of any person with their back against a strong light, the face of a person so placed having always appeared to me perfectly black. I noticed the same effect in looking at a powerful lamp, beyond which I could see very little without shading my eyes with my hand. In the dusk, while walking, I frequently ran against objects, and a street lamp only made the surrounding gloom to appear deeper. I was also unable to measure distances, or see objects approaching in the twilight. When I was about twelve years old a film was for the first time observed in both eyes, that in the right eye being more distinct than the left. This had doubtless existed since childhood, and had become gradually more evident, and was at last accidentally observed. For two years after this, by the advice of an oculist, all study was given up, and I was turned out on a farm. At the end of that time I again resumed my studies. Each year I found that I required a more powerful light. The invention of moderator lamps proved a great boon to me, but the effect of even these powerful means gradually wore off in time, and as years passed on, I was reduced to guessing a good deal at words, especially in an imperfect light, for I found that I frequently mistook a strange word, unless I spelt it over with great care. But the progress of the cataracts had been so gradual that my sight had become highly educated, and after residing three years in Oxford, I took my degree without difficulty, though at times I suffered great inconvenience, both in the lecture room and at examinations, from want of light." Such was the account that I received from this young gentleman when he first consulted me, and which I have copied from his own written statement. On examination I found that the right eye was useless to him; it could only perceive dim outlines of objects, and there *was divergent* strabismus of that eye to the ex-

tent of about three lines. On dilating the pupil fully with atropine, rather more than the nuclear half of the lens was found to be opaque, and of a dense grayish white colour, the margin being perfectly transparent. Through the transparent part the fundus of the eye could be readily seen with the ophthalmoscope, and appeared quite normal. When the pupil was thus dilated the vision was restored to a remarkable extent. Although the eye had been so long disused, and was so very imperfect during the contracted state of the pupil, yet as soon as it became fully dilated, he could read No. 4 of Jaeger, and he stated that his sight was brighter and clearer than he ever remembered even with the left eye, both for near and distant objects. With the left eye he could read slowly and with difficulty, and by the aid of a strong light, No. 8 of Jaeger, at a distance of five inches. His perception of large objects also was misty and indistinct beyond a range of two yards. Under the influence of atropine, a nuclear cataract could be seen of the same size as that in the right eye, but of a pale gray colour, and much less opaque. The margin, like that of the right eye, was perfectly transparent. Vision was not improved in the left eye, but on the contrary rather impaired and confused by the action of atropine. This young gentleman was anxious to enter the Church, which he felt to be impossible under existing circumstances; he had also a strong impression that his sight was becoming worse from month to month. He was anxious to avoid, if possible, the necessity of wearing strong convex glasses. Under these circumstances I determined to perform iridesis. Early in April, under the influence of chloroform, I made an artificial pupil inwards in both eyes by iridesis. The effect of this operation was in some respects curious and interesting. Sight was so far improved in the right eye, that he could read No. 4 of Jaeger fluently in a good light; he could also see distant objects well. It was curious also that in reading he obtained binocular vision. The right eye, which in repose remained divergent to the extent of three lines, might be seen to travel inwards, so as to converge accurately with the left as soon as he directed the eyes upon a minute object, as in reading, but when looking at distant objects there was double vision. This seems to prove that the divergent strabismus was caused by the defective vision; that under the stimulus of the focussing effort, the internal rectus was able to reassert its preponderating power, and overcame even an extreme degree of strabismus, but that in looking at distant objects, and when the focussing effort was but feebly exerted, the external rectus still possessed too much influence, and produced double vision; the only defect that remained in the right eye was a feeling as if more light were wanted in the eye, and considerable difficulty in seeing small objects when the light was rather subdued. The effect of the operation upon the left eye was less satisfactory, as might have been expected. There was some confusion and distortion of minute objects, the power of reading with it was not so good as before the operation, and even distant objects were indistinct. When, however, the two eyes were used together, the power of reading was good, far better than before the operation, and in looking through a telescope with the left eye, vision was quite distinct.

In July I again saw my patient, the sight of the left eye had improved, partly perhaps by practice, and partly because the nucleus of the lens had become denser. The sight of the right eye remained the same. There was still double vision for distant objects, and a strong desire for more light in the eye. As my patient was very accurate in all his observations, and intelligent in his suggestions, I determined to endeavour to remove as far as possible the inconveniences of which he now complained.

About the middle of July he was once more placed under the influence of chloroform. I then proceeded in the first place to divide the external rectus muscle of the right eye subconjunctively; I then performed a second iridesis in the same eye in such a way as to enlarge the pupil and alter its shape, giving to it a crescentic form, with the two cornua of the crescent cut off. The effect of this operation was very satisfactory; double vision and all tendency to divergent strabismus were removed, more light was admitted into the eye, the patient could see better in a subdued light, and retained quite as

accurate vision, both for near and distant objects, as before the second operation—in fact rather better. The right eye was now, in all respects, decidedly the best of the two.

I again saw my patient in October. The right eye had retained all the advantages gained by the last operation, and had even somewhat improved by use. The left eye remained about the same. There was some confusion, except when the light was very strong, and there seemed to be less light admitted into the left eye than into the right. Under these circumstances, encouraged by the success which had attended the enlargement of the pupil in the right eye, I proposed to my patient a similar proceeding in the left, partly with a view of admitting more light through the transparent margin of the lens, and partly to cover, if possible, more of the semi-opaque nucleus. I performed this my fourth and last operation under chloroform, and succeeded in both these objects. A fortnight after this operation he was able to see rather better with the left eye than the right, and as the opaque nucleus gradually increased in density the distinctness in the outline of objects was brought out more vividly, though of course with both eyes he sees better than with either one employed singly. He now can read No. 1 of Jaeger at 5 inches, and No. 16 at 20 inches. His sight for small print is slightly improved by a weak convex glass, and for distant objects by a weak concave, but he rarely employs either. He writes me word that he was ordained in December last, and that he is able to perform all his clerical duties with ease and comfort, though he still is somewhat at a loss when the light is feeble.

This case helps us in determining some important practical points. 1st. It shows that the nucleus of a congenital cataract is liable gradually to become more opaque, although the marginal portion may retain its transparency. 2ndly. That divergent strabismus results from defective vision, and from the non-exercise of the focussing power; that it will subside in a great measure on the restoration of sight, and that it may exist where the retina is quite healthy. 3rdly. It shows that the artificial pupil should be of a sufficient size, at least equal to the area of the natural pupil, though necessarily of a different shape. 4thly. It proves that a semi-transparent state of the nucleus causes some confusion of vision, by allowing rays to pass with a different degree of refraction, and that the more this can be covered, the better the result will be. 5thly. It suggests that there should be sufficient clear margin to ensure an artificial pupil, at least equal in its area to the average size of the natural pupil, and that where suitable cases are selected as good a result may be anticipated as where the cataract is removed. 6thly. It may be observed that four operations were performed without the slightest reaction. The eyes being on each occasion fit for use in a few days. This is the usual experience of this operation, it may therefore be regarded as a far safer proceeding than the removal of a congenital cataract in an adult.

I consider the performance of a second iridesis as a valuable modification of the original operation in certain cases, and a preferable proceeding to iridectomy in cases of congenital cataract; it enables us to limit and define the pupil more accurately, and to draw the iris more completely over the nuclear portion of the lens, so as chiefly to expose the transparent marginal part. The arguments against the performance of this operation, as contrasted with the removal of the lens, seem to be the possibility of the marginal part of the lens becoming at some time or other opaque; the altered and deformed shape of the pupil, and the limitation in the amount of light admitted into the eye; and the question whether vision is ever quite as good as after removal of the lens, in consequence of the imperfect refractory power of the margin of the lens and cornea, as compared with the centre. The chief arguments in its favour are its greater safety and the avoidance of the necessity for using powerful lenses. Each individual case must be judged by its own special merits. The surgeon must weigh the respective advantages and disadvantages, he may even leave the choice in some measure and in some cases to the patients, and it is possible that the case I have related may assist him in forming his judgment.

As I have introduced *some slight* modifications, both as regards the steps

of the operation of iridesis, and the instruments employed since I first described it in this Journal, I think it may be useful if I briefly detail my present method of proceeding in cases such as the one I am now describing. The patient having been placed under the influence of chloroform, the eye is exposed by means of a wire speculum, to which a stop is introduced, so as to regulate the degree to which the lids are separated, and thus to prevent a constant strain upon them. As the most advantageous position for the artificial pupil is inwards towards the nose, the eye must be held firmly outwards. I then introduce a broad needle curved at an angle, upon the flat, just at the point of junction of cornea with the sclerotic, slightly encroaching upon the latter. A small loop of fine black silk is then placed over the opening that has been made. A fine blunt hook, made of silver, so as to allow of being curved conveniently for entering at the nasal side of the eye, is then passed through the loop of silk and the opening into the anterior chamber to the margin of the pupil, which is thus drawn out through the opening by means of the hook. An assistant then seizes each end of the loop of silk with small broad pointed forceps, and tightens them carefully, so as to enclose the portion of iris that has been caught in the hook and pulled out: care should be taken to include the iris only in the tie, and not any of the conjunctiva, as it detaches more readily. Some little management is also necessary in bringing the hook out through the small wound, otherwise it is apt to entangle itself in the opening as it is drawn out. This is done by turning it on the flat as it emerges. In the course of two or three days, the little knot of silk, together with the portion of iris that has been included in the ligature, drops off. If this do not occur, it may be lifted off with a pair of forceps.

In performing a second iridesis, as I did in this case, at an interval of three months, a method of proceeding similar to the former must be adopted. The opening must be made through the junction of the cornea and sclerotic, about 2½ lines from the previous one, and the hook must be introduced round the edge of the previous artificial pupil, so as to draw it out in such a way as to alter its shape. The effect of this is to enlarge the pupil in one way, and expose more of the clear margin of the lens, and diminish it in another, so as to cover more of the opaque part of the lens. The chief points requiring care in the operation, are: 1st. The size and position of the opening, in front of the ciliary attachment of the iris, but not through the transparent cornea, and just large enough to admit the hook. 2ndly. The hook must be carefully made, round and smooth at the end, so as to prevent the possibility of injuring the capsule of the lens, and after it has entangled the margin of the iris some management is required, as already stated, in causing it to emerge readily through the small wound by which it has entered; this is done by some careful rotation. 3rdly. It is important to draw the hook, together with the iris that is held by it, completely out through the wound as far as practicable, without detaching one from the other. In drawing the ends of the silk tight, care is required to keep them on a level, and in an exact line with the portion of iris that is to be ligatured, otherwise it may be torn away, and thus the object of the operation may be frustrated. A little care is also required in detaching the hook after the prolapsed portion of iris is strangulated; it must be carefully unhooked, as it were, and forcibly dragged away.—*Ophthalmic Hospital Reports, Vol.* iv., 1864, *p.* 150.

77.—ON IRIDECTOMY AND SECTION OF THE CILIARY MUSCLE.

By the Editor of the Journal of British Ophthalmology.

We could name a dozen of our countrymen whose reputation in eye disease, well attested by important contributions to science in their special department, must be held to be second to no practitioners of Continental celebrity. Any one with competent powers of analysis, who carefully reads the numerous translations of foreign works upon the subject so constantly issuing from *the press, will, by* internal evidence, soon be convinced not only of this, but

that Foreign practitioners are a generation, at least, behind us in practical knowledge and operative skill. A widely entertained idea to the contrary, however, has been successfully rubbed into public opinion, both at home and abroad; nor need any surprise be expressed at the circumstance, which may be accounted for without going far below the surface of things. . . .

That our appreciation of the present position of ophthalmic surgery in England will be disputed, we have little fear. Before us are the medical journals of the past six months, containing all that has been said upon the Iridectomy question of the day. To this lamented exhibition of incautious and sometimes incompetent zeal, backed up occasionally by the solemn utterances of gifted individuals committed to a selfish policy, we should apply for abundant confirmation, if challenged to produce proofs of the servile degeneracy to which the school of English oculists has fallen, through influences such as we have referred to. . . .

It will be as well to make a few remarks here upon the inconvenience we have experienced throughout the discussion, from the extraordinary looseness of the term employed by British advocates, to designate that peculiar diseased condition of the eye, in which, with much positiveness and exactness, they allege, that to be of any use to the patient, iridectomy can only be properly performed. Of all the names that might have been selected, *glaucoma* certainly appears to us the most inappropriate and inopportune. Under the old system of nomenclature, before the invention of the ophthalmoscope, which now affords such abundant means of accurately defining the nature of many hitherto unperceived differences in eye disease, there was some excuse for the use of an expression, suggested by a prominent objective symptom, a *greenish* appearance, reflected from some part of the internal eye; and which afforded imperfect knowledge an opportunity, at times very convenient, of not committing itself to a more definite description. . . .

If the advocates of iridectomy, on the one hand, cut so sorry a figure as controversialists, we must say their opponents exhibit quite as poor an appearance on the other side. A great sin of omission convicts them of much ungenerous disregard for the merits of others, and can be as easily proved against them. To do this, we shall, in the first place, point out that after all, in the discussion, truth has sometimes managed to show her head above the confusion and strife of words, where she was in much danger of being completely submerged. Her reputed residence in a well is no myth, or otherwise the iridectomy deluge of ignorant opinion and positive assertion would have been too much for her. At all events, some few facts have been proved to our satisfaction. To take them in order, it seems agreed by common consent of all parties, *Firstly*—That some operation, cutting into the internal eye, is of great advantage in a particular class of inflammations connected with the choroidal system; including in this, the coat itself, the iris, and the ciliary structures. *Secondly*—That it would be well to avoid, as much as possible, terms used in an exploded system of ophthalmology when describing symptoms, or defining disease, as they appear with our present greatly improved means of making examinations. *Thirdly*—That it is highly necessary to consider inflammation seated in any one part of the choroidal system, as if it affected, by complication, the whole. *Fourthly*—That irido-choroiditis should be classed, according to circumstances, into *acute* and *chronic*. *Fifthly*—That *tension* of the eyeball is the chief characteristic of the former, or acute form, when an operation is required, whilst the opposite condition of *softness* sometimes indicates the same need in the latter or chronic stage. *Sixthly*—That the two conditions naturally imply different operations; one obviously to reduce tension; the other for the very opposite purpose is less patent, and could not have suggested itself, but might be the result of a happy accident combined with ingenious observation. *Seventhly*—That such occurred in Graefe's practice, and led to iridectomy. And *finally* (of course to our own satisfaction, and apart from any theory)—That Hancock's operation for the division of the ciliary muscle fully satisfies all the requirements of *acute irido-choroiditis*, and posterior *arthritic ophthalmia; whilst, on the* other hand, iridectomy is calculated to

give the best chance for improvement wherever some degree of atrophy of the eyeball exists with or without synechia in *chronic irido-choroiditis*.

Now, it is regarding these very important points, and especially the ultimate conclusion to which they lead, that we charge the Anti-Iridectomists with not having candidly recognised the whole truth in their treatment of the subject in dispute. If they could, they would gladly have burked Hancock and his operation too. They have always carefully avoided any mention of his name, or reference to section of the ciliary structures, as a proper substitute for Graefe's more serious proposal. By a most disingenuous policy, they have also sought to steer a comfortable middle course; "damning with faint praise" a certain amount of excellence they admitted in their opponent's propositions; and limiting hostilities to a demand for principles and definitions, which they knew very well could not be given under the circumstances, without giving up the whole question at issue. In this cleft stick, accordingly, they held their German friends, satisfied without going further into opposition, for fear they might be compelled to magnify a contemporary fellow-countryman's fame, by showing that, at least, in one half of the cases claimed for iridectomy, in *acute irido-choroiditis*, the early stages of *internal arthritic ophthalmia*, &c., Hancock's operation was all that was needed.

There is, in fact, nothing more unworthy in the conduct we have had to comment upon with regard to the leading opthalmologists of the day, and their Continental proclivities, than the like professional jealousy of the Anti-Iridectomists, always remembering to forget whatever was calculated to reflect credit upon their own country. Through published letters innumerable, we look in vain for any approving reference to what seems to us, the very best measure as yet proposed for at once evacuating the overcharged humours of the eye, unloading the gorged and tumid vessels of the ciliary zone around the cornea, and at the same time dividing the muscular fibres: abnormal spasm in which, seems to be constituted the constricting element in the mischief going on. There cannot reasonably, we think, be the least doubt that Mr. Hancock's operation will ultimately become the settled universal practice in *acute irido-choroiditis*, whenever an incision is necessary to relieve the tension and vascular excitement; and yet that these great friends of humanity, so frequently appealing to a sense of duty, shut their eyes, and quietly ignore, as if it never existed, the ample and perfectly reliable testimony as to the positive permanent benefit that has followed its use in many hundred cases.

It will have been perceived that throughout we have assumed recurring *acute irido-choroiditis* to be not only due to a constitutional dyscrasia, but moreover, that it is the same disease as Graefe's acute glaucoma, the *ophthalmia arthritica interna* of more experienced observers, without his bold, presuming genius. It is to this latter feature of his character he is in a great measure indebted for his notoriety, and, in our opinion, responsible besides, for the numbers of mutilated eyes that has since been sacrificed on his authority. Can anything less be said, we confidently ask, when we reflect that in his paper, "On Iridectomy in Glaucoma," (*p.* 362, *Vol. V. New Syden. Soc. Pub.*), he insists "in the acute period that the advice *to operate immediately and without hesitation* cannot be too urgent," and after further argument and several references to decisive cases, which he wishes us to accept as cases in point, he continues, on the next page, to say "only a glance at the fate of these different eyes is needed *to induce the abandonment of all other treatment in glaucoma than that of the immediate performance of iridectomy.*" The italics, it deserves notice, are not even ours, but apparently the author's own, to emphasise and add to the importance of the advice given, and yet, in the face of these positive injunctions, the effect of which upon his English imitators may be gathered from the fact, that the very same practice of early, immediate operation was insisted upon, over and over again, in the earlier letters published during the late discussion in the pages of the *British Medical Journal:* in the face of this we repeat, and after all the mischief *of indiscriminate* operation has been done, we find Graefe himself, five years *subsequently, entering* the field again, with another Paper to show that the

"*urgency in acute cases varies.*" (*p.* 41, *Part* 1, Vol. IV. *Oph. Hospital Reports.*) And that the "urgency does not depend on the intensity of redness (!) or chemosis, nor on the amount of tension (!!) or pain (!!!) but, above all, on *the degree of impairment of sight.*" Let it be remarked, the necessity of iridectomy is stated here not to depend upon redness, pain, or swelling, the ordinary indications of acute inflammation, but on a loss of vision, which, in fact, is the only measure we have of the progress of chronic glaucoma; the Professor thus endorsing the opinion expressed in the sixth conclusion of our summary. But what follows improves even upon this remarkable withdrawal from the positive position assumed by Graefe in his first papers—"As long as the patient is able to read large print, and the field of vision is not contracted, the operation may be deferred a few days, if need so require, especially if the inflammation tend to abate, and the sight to improve somewhat under the treatment employed." A few short years have produced a wonderful change of opinion, but we did not expect that he would so candidly have admitted the necessity of quite another sort of practice. In this, we trust his imitators will follow the good example. It does seem strange, after such distinct language as "*to operate immediately and without hesitation,*" &c., to read now from the same hand the passage just quoted, arresting the operation, "*especially if the inflammation tends to abate, and the sight to improve somewhat under the treatment employed.*" Just so, now it is, be in no hurry to operate; first try the usual means to subdue the inflammatory stage, and if it succeeds, all very well, if not, then iridectomy may be tried. His English friends must certainly thank him for refuting them so completely, as far as regards the importance and value they attach to an early resort to iridectomy, and at the same time suggesting what they have always objected to, first to try ordinary antiphlogistic remedies. He wishes, indeed, to impress this upon the minds of his disconcerted followers, for in this last paper referred to we find, further, "these facts need not prevent an intelligent practitioner from making use of iridectomy for cases of acute glaucoma, but only serve to recommend to him some degree of prognostic circumspection." This is merely a roundabout way of telling them to be cautious. At all events, they would have resented any such temporizing advice from anti-iridectomists, and stigmatized it as the result of imperfect observation or incapable weakness. We are glad, therefore, to find it on record from their instructor himself, and hope that in future, the eyes of humanity may be spared the too frequent application of the knife of the young or zealous iridectomist."

The object, however, of this paper has been, not one of analysing the scientific accuracy or practical value of a few propositions in eye disease, which, owing their origin to German ingenuity of thought, have been unduly magnified into authoritative doctrine, by British Ophthalmologists, but rather that of expressing an opinion as to the general unsoundness of Graefe's conclusions on iridectomy as a treatment for glaucoma; that is to say, wherever we could fix him to any data to guide us as to what he really meant; and which would stand as evidence between him and ourselves; at the same time we have not in any way sought to misrepresent his views. We have also made an effort to remove the stigma, which some of our contemporaries endeavoured to affix upon national character, by ostentatiously seeking from abroad that information they could much more readily and trustworthily have obtained in our ophthalmic institutions. Finally, we emphatically enter our protest against the outrageous estimate of the value of iridectomy, entertained by some of our leading ophthalmologists, who pretend to receive it as a great addition to modern surgery, and as a blessing and boon to suffering humanity; because had they been moved by these always excellent reasons, they would long before this have recognized in division of the ciliary structures, a safer and quite as effective an operation, the means of securing all the advantages iridectomy could offer, without the danger of mutilation which always must attend it. There are some eyes, however, that require no less a delicate instrument to open them than a crow-bar; and if after what we have said in vindication of national honour and reputation, the British

admirers of the German practice fail to perceive the superiority of the product of home sagacity and operative skill, we shall give up the task of further endeavouring to convince them, and class them as belonging to the cases of the most hopelessly blind—those who will not see.—*Journal of British Ophthalmology, Oct.* 1864, *p.* 2.

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78. ON THE OPERATION OF IRIDECTOMY.

By T. Wharton Jones, Esq., F. R. S., Professor of Ophthalmic Medicine and Surgery in University College, and Ophthalmic Surgeon to the Hospital.

Iridectomy is the cutting out of a piece of the iris, and is an operation which has been long in use for the formation of artificial pupil. There are two forms of the operation—the one named *central excision* of the iris, the other *lateral excision*.

The formation of artificial pupil by central excision, you see had recourse to here in certain cases of closed pupil. The formation of artificial pupil by lateral excision, which we have occasion more frequently to perform, is applicable to those cases especially in which the pupil is free either wholly or in part, but in which the centre of the cornea opposite is so opaque as to prevent the entrance of the light into it. By excision of the iris on one side, opposite a clear part of the cornea, the rays of light entering the eye by that clear part are afforded a free passage on their way to the retina.

The opacity of the centre of the cornea in those cases in which the pupil is still open wholly or partially, is an effect of various forms of ophthalmia. Though after the subsidence of the inflammation, the cornea tends to become clear again, opacity often lingers in the central part. I should observe that the clearing goes on from the circumference to the centre. If there has been no penetrating ulcer of the cornea with *prolapsus iridis*, the pupil remains wholly open and free behind the opacity. When, however, there has been ulceration eating through the whole thickness of the cornea, a portion of the iris will be found adherent to the cornea, in consequence of its having become prolapsed through the perforation caused by the ulceration. In the situation of the cicatrice left by the healing of the ulcer, and to a considerable extent all round, the cornea remains permanently opaque. In such a case the pupil is contracted and dragged behind the opaque part of the cornea and its margin is free on one side only.

The operation for artificial pupil by lateral excision we sometimes perform in cases in which the transparency of the cornea is unimpaired, but in which there is obstruction of the pupil by opaque deposit of lymph and extensive adhesions of the pupillary margin of the iris to the capsule of the lens, the lens being in other respects still transparent.

Having thus indicated the kind of cases in which it has been long customary to perform the operation for artificial pupil by lateral excision of a piece of the iris, let us now address ourselves to the mode of performing the operation.

The best position for the patient is to lie on his back on a table with his head sufficiently raised; in this position the patient's head is secured against any sudden backward movement which might be fatal to the eye at the moment the iris is being drawn out in order to be excised. If the patient is young, or not sufficiently firm, you will find it necessary to give chloroform.

Matters being thus so far arranged, the surgeon, having all his instruments ready at hand, takes charge of one eyelid and his assistant of the other; then, at the side where the state of the eye permits of the artificial pupil being formed—a point of course, previously determined on—he makes an incision through the cornea, close to and concentric with its margin into the anterior chamber to the extent of about a quarter of its circumference. This section of the cornea is, perhaps, best effected by puncturation and counter-puncturation with a cataract knife, as in making the section of the cornea *for extraction* of the cataract. This was what I did in the case of glaucom-

atous ophthalmia in which I performed iridectomy last Friday; that is, three days ago. In operating on that occasion, however, I made the section on the sclerotic side of the margin of the clear cornea, the reason for which proceeding I shall afterwards explain. In operating exclusively for the sake of an artificial pupil I would not, unless there were a very small extent of clear cornea, make the section beyond the extreme margin of the cornea, as I did in this instance, for my object would not be to cut away the whole breadth of the iris at the place.

The section of the cornea having been made, the necessary result was, as you saw, escape of the aqueous humour. Another result, which generally takes place, you saw was prolapse of the iris, that membrane being forced out through the opening in the cornea by the gush of aqueous humour which took place at the moment of completing the section.

In cases in which there is adhesion of the iris to the cornea (*synechia anterior*, as such a state is called), or of the iris to the capsule of the lens (*synechia posterior*, as this state is called,) *prolapsus iridis* may not occur.

If the iris protrudes, the second step of the operation is performed by simply seizing the protruding iris with a fine forceps, such as this which I used on the occasion referred to, and dragging it further out so that its pupillary margin may be included in the piece excised.

If, on the contrary, in consequence of adhesion to the cornea or capsule of the lens, the iris does not of itself protrude through the opening, it is necessary to enter the point of the forceps closed, a little way into the anterior chamber, and then opening the forceps, the Surgeon seizes the iris by pinching it up, and drags it gently out to the necessary extent. In performing this manœuvre, we must take care not to push the point of the forceps beyond the pupillary margin of the iris, for fear of wounding the capsule of the lens —a wound of which membrane, as you are aware, would induce opacity of the lens.

The excision of the protruded piece of iris is effected by means of this well-known form of scissors, curved on the flat. In performing this act, the scissors are to be held, of course, with the convexity of the curve next the cornea, and with their length at right angles to the line of the section of the cornea, in order not to run the slightest risk of including a bit of the flap of the cornea in the excision. In the operation last Friday, I was careful to snip the protruded iris first at one end of the wound in the cornea and then at the other, before completing the excision of the whole. This I did, as it was my object in that case to excise a large piece of the iris. It was for this reason, also, that I made the section on the sclerotic side of the clear margin of the cornea. Had I been operating merely for the formation of an artificial pupil it would have been sufficient to snip off as much of the iris as could be comprehended in one cut of the scissors.

When the eye and iris are otherwise healthy, as is usually the case when you perform iridectomy merely for the sake of obtaining an artificial pupil, there is little or no bleeding from the cut iris. Matters are different when the operation of iridectomy is performed under the circumstances under which I performed it in the case I have been referring to. The iris, being in common with other parts of the eyeball, the seat of great venous congestion, blood escaped in that case, as you saw, in very considerable quantity—so great as to fill the aqueous chambers.

The operation being completed, what is the state of the eye? By the evacuation of the aqueous humour, the vitreous body and lens are, by the elastic reaction of the coats of the eyeball, pressed forward against the remaining portion of the iris, and this, again, against the cornea.

The edges of the corneal incision very quickly adhere, so that when you examine the eye the second day after the operation, you, perhaps, find union has taken place; and, as a consequence of the closure of the opening in the cornea, you find the aqueous humour reaccumulated and the cornea again plump.

Whence is this regeneration of the aqueous humour derived? I think there can be no doubt that it is derived by exudation, chiefly from the blood

circulating in the vessels of the very vascular ciliary processes which project free into the posterior chamber all round its circumference.

A very common answer to this question used to be that the aqueous humour is secreted by the lining membrane of the aqueous chambers; but this, I must say, is a statement not exactly in accordance with our notions of the process of secretion, unless it be understood to refer merely to the detached cells seen suspended in the aqueous humour examined under the microscope, and which are, no doubt, thrown off from the surface of the walls of the aqueous chambers. Another opinion which has been enunciated of late years is, that the aqueous humour is derived principally from the surface of the iris by exudation or secretion. In disproof of this opinion, it may be enough to refer to the fact that in a case of irideremia or congenital total absence of iris, in which I had occasion to perform the operation of paracentesis corneæ for an internal inflammation, regeneration of the aqueous humour took place as rapidly and completely as is usual in cases in which the iris is normally existent; and to another fact—viz., that the whole iris has been torn away by accident, with, at the same time, loss of the aqueous humour, but in which case the aqueous humour, notwithstanding, has been duly regenerated.

If there was any blood effused into the aqueous chambers at the time of the operation, it disappears by absorption—usually in a very short time. The patient commonly recovers from the operation in a few days.

So much for iridectomy or lateral excision of the iris for the sake of forming an artificial pupil. I should now direct attention especially to the operation as performed in the case of glaucomatous inflammation of the eye at present under our care.

Before speaking of glaucoma, and iridectomy as a means of treating it, however, I would make some remarks on the evacuation of the aqueous humour as a means of treating certain anterior internal ophthalmiæ.

It is about fifty years ago since the value of this operation was brought under the notice of the Profession by Mr. Wardrop, in cases especially of anterior internal inflammation of the eye attended by dimness of the cornea. Considering that the dimness of the cornea was the result of intraocular pressure, Mr. Wardrop punctured the cornea and evacuated the aqueous humour in order to relieve this pressure. Mr. Wardrop was led to adopt the practice by reflecting on the fact, that if you take the eye of a sheep, for instance, and squeeze it in your hand, the cornea becomes dim, but resumes its transparency when the pressure is relaxed.

Whether the dimness in this experiment be of the same nature as the dimness of the cornea in the inflammations of the eye under notice is a question of no practical consequence. It is enough for us to know that evacuation of the aqueous humour is a most valuable remedy in the treatment of corneitis and such anterior internal inflammations as involve the iris and cornea.

Such inflammations give rise to more copious exudation of aqueous humour, and the irritation resulting from the intraocular pressure occasioned by the increased accumulation keeps up the disease and opposes the beneficial operation of the medicines employed. Relieve this source of irritation by evacuating the aqueous humour, and the inflammation will begin to yield to the action of the medicines it previously resisted.

Often a single performance of the operation is sufficient; sometimes the operation may require to be repeated two or three times. We have seen that the aqueous humour is quickly regenerated, and that the source whence it is derived is principally the blood circulating in the vessels of the very vascular ciliary processes.

I have over and over again proved the value of this operation as an auxiliary of the medicinal treatment, in cases in which the inflammation was very stubborn, and by means of it have often rescued the eye from imminent destruction.—*Medical Times and Gazette, July* 23, 1864, *p.* 83.

79.—ON THE PATHOLOGY OF GLAUCOMA.

By T. Wharton Jones, Esq., F.R.S., Ophthalmic Surgeon to University College Hospital.

The name glaucoma is applied to a morbid condition of the eye, in which there is great deterioration of sight—even total blindness—and in which, through the much dilated and, perhaps, ovally or otherwise distorted pupil, there is seen an appearance as if of a sea-green opacity; hence the name glaucoma, from γλαυκος, sea-green.

This sea-green opaque appearance behind the pupil was at one time supposed to have its seat in the vitreous humour. Glaucoma, therefore, as opacity of the vitreous body, used to be contrasted with cataract as opacity of the crystalline lens.

It was, however, demonstrated by Dr. Mackenzie many years ago, partly by dissections after death, and partly by catoptrical exploration of the eye during life, that the cause of the sea-green opaque appearance in glaucoma really has its seat in the lens, and that it is owing to an alteration in the condition of that body, whereby, though still in a great measure retaining its transparency, it reflects much of the light entering it, and that of a sea-green tint.

Though giving the name to the disease, the sea-green opaque appearance behind the pupil, it is to be remembered, is merely an incidental accompaniment of it. A very much more serious state of matters characterises its real pathology. Indeed, the appearance of a grayish or greenish opacity behind the pupil is not at all an uncommon appearance in the eyes of old persons whose sight, considering their age, is not particularly impaired.

Great venous congestion may be said to be the characteristic morbid condition of the eye in glaucoma; and the parts the congestion of which is more immediately fatal to the sight are the choroid and retina.

If we examine the interior of a glaucomatous eye by means of the ophthalmoscope, we not unfrequently find that the humours are sufficiently clear to allow of the state of the papilla optica, retina, and choroid being ascertained.

In consequence of the excavation of the papilla optica, the course of the retinal vessels, on entering the eye, appears peculiar. Having penetrated the lamina cribrosa, they run on the bottom of the excavation, by the overhanging margin of which their further course is concealed; and when they reappear on the retina they do not look like continuations of the same vessels.

The arteries of the retina, being constricted, appear slender and pale, whilst the veins are dark and enormously distended.

As to the alterations observed in the condition of the choroid:—The quantity of the pigment in the cells of the pigment membrane is diminished, whilst there may be an increase in the quantity of the pigment in the cells of the stroma of the choroid. In this case, the vessels of the choroid are seen in ophthalmoscopical examinations more distinctly, and with darker spaces between them.

The turgid tortuous ramifications of the veins extending from the place of insertion of the recti muscles on the white of the eye, manifests a state of external venous congestion likewise. Indeed, the glaucomatous condition of the eye very generally coexists with abdominal congestion and hemorrhoidal disease—so much so, that the turgid and tortuous veins, so common in the white of the eye in such cases, have been named "abdominal vessels."

In the glaucomatous eye, even at a very early stage of the disease, the iris is inclined towards the cornea, and the aqueous chambers are diminished in depth. This is owing to the increased accumulation of blood in the veins of the choroid and retina, and to an increase of the fluid in the cells of the vitreous body—itself the result of the congestion—whereby the posterior segment of the eyeball is distended, and the lens pressed forward against the iris.

It is in a great measure owing to this posterior intraocular distension that the papilla optica becomes excavated, and that the retina suffers.

It is in consequence of this posterior intraocular distension also that the eyeball is felt so hard to the touch. Owing, also, to the intraocular distension, pain in the eye, around the orbit and all over the side of the head, is a very common attendant of glaucoma. It is especially severe when inflammation of the eye supervenes, which it is very prone to do. In this case, also, the sight is rapidly destroyed.

The inflammation is of that character which has been named arthritic posterior internal ophthalmia. The affection is also named acute glaucoma.

I have said that the lens, though it looks opaque in glaucoma, may be still clear enough to permit the bottom of the eye being ophthalmoscopically examined. The lens, however, is liable eventually to become cataractous. In such cases, there is usually already disorganization of the retina, so that the cataract is merely superadded to hopeless amaurosis.

Great venous congestion appears to be the fundamental morbid condition of the eye in glaucoma, the retina and choroid being especially affected. We saw represented in Liebriech's ophthalmoscopical figures the arteries of the retina constricted and pale, whilst the veins are very turgid and dark. That there is a similar condition of vessels in the white of the eye, is evident; for while the rectal veins are turgid and tortuous, corresponding arteries are scarcely to be seen.

On subjecting the retina of a glaucomatous eye to microscopical examination, I have found the radicles of the veins with varicose dilatations, or pouches.

The capillaries have been said to be varicose, but I did not find them so, and I do not remember, in the course of all my researches into the state of the blood vessels in inflammation, to have found anything you could call a varicose condition of the capillaries. Constricted arteries appear with their walls much thickened from contraction of their circular muscular fibres. This thickening of the walls of the arteries must not, however, be called hypertrophy. In the web of the bat's wing or frog's foot, under the microscope, you see the thickening of the walls of the artery take place when the calibre of the vessel becomes constricted by contraction of the circular muscular fibres; and, on the other hand, you see it superseded by thinning, when the calibre of the artery becomes widened by relaxation of the circular muscular fibres.

The varicose state of the venous radicle, I saw take place in the mesentery of the living mouse, under the microscope, by the walls of the vessel here and there becoming much constricted, and here and there much dilated. This was towards the end of the observation. The corresponding arteries were constricted, except where their last ramifications opened into the capillaries. Here the flow of blood in them was retrograde and sluggish. In the varicose venous radicles there was congestion of red corpuscles.

The observation to which I refer illustrates the relation between constriction of the arteries and congestion of the veins. An artery becoming constricted, the blood is propelled through it with diminished *vis a tergo*. In consequence of this, the corpuscles are seen gradually to accumulate in the capillaries and venous radicles to which the artery leads, and thus congestion of blood-corpuscles is established. In a case of this kind I have seen a small incision at the place by emptying the distended vessels, relieve the congestion of the surrounding parts. To this observation, which I first made on the tail of the tadpole, under the microscope, many years ago, I shall have occasion to refer in my next Lecture.

Seeing that constriction of the small arteries of a part causes congestion of the capillaries and venous radicles to which they lead, and seeing that the fundamental morbid condition of the eye in glaucoma is great venous congestion, with constriction of the small arteries, we can understand how it is that belladonna or atropia applied to the eye in glaucoma aggravates the *symptoms.*

The action of belladonna on the small arteries of a part to which it is ap-

plied is, as I have over and over again explained, to excite the circular muscular fibres composing their middle coat to contraction, and, consequently, to produce constriction of their calibre. It is on the same principle, as I have also explained, I may here remark, that belladonna causes dilatation of the pupil. It excites the radiating muscular fibres of the iris to contraction, they being like the circular muscular fibres of the arteries acted on through the sympathetic nerves. The circular muscular fibres of the iris are under the control of the nerve of the third pair, and are not excited to contract by belladonna. They are, however, excited to contract by the Calabar bean, as you know. It has been fancied that belladonna acts by paralysing the circular muscular fibres of the iris, and so allowing the uncontrolled action of the radiating to come into play, but there is no good foundation for such an opinion.

To return to the injurious action of belladonna in glaucoma. Belladonna, by increasing the constriction of the arteries of the eye, determines an increase of the congestion in the veins.

When the glaucomatous state of the eye is not much developed, the eye slowly recovers from the injurious operation of the belladonna. This may not, however, prove to be the case in an advanced stage of the disease. In a stage in which the sight is already much deteriorated, a single application of belladonna to the eye is likely to deteriorate it still more, and that irrecoverably, to the great horror and distress of the patient and his friends and to the discredit of the doctor. Be cautious, therefore, of applying belladonna or atropia to the eye in glaucoma. A late venerable and esteemed friend of mine used to tell me, in disparagement of the application of belladonna to the eye, of the case of a distinguished author whose sight—it must have been but a glimmer of sight—was destroyed by a single application of belladonna for a supposed cataract. The case, I have no doubt, was one of glaucoma in an advanced stage, and the application of the belladonna acted merely like the last feather on the overladen camel's back.

To resume our theme from which these observations on the action of belladonna in glaucoma have been a digression. The venous congestion of the choroid and retina in glaucoma, as we have seen, causes some degree of intraocular distension. And as an effect of the continuance of this congestion there supervenes an increase of the fluid of the cells of the vitreous body, which occasions a corresponding increase of the intraocular distension.

Whilst in anterior internal inflammations of the eyeball in which evacuation of the aqueous humour is often so beneficial, the intraocular distension has its seat especially in the anterior segment, as may be seen by the prominence of the cornea and depth of the aqueous chambers; here the intraocular distension has its seat chiefly in the posterior segment.

One effect of the distension in the posterior segment of the eyeball is seen to be that the lens and iris are pressed forward, and the aqueous chambers thus diminished in depth. Another effect of the intraocular distension implicating the posterior segment appears to be the excavation of the *papilla optica* as seen with the ophthalmoscope—an effect the possibility of which may be conceived if we call to mind that the optical nerve enclosed in its neurilemma lies within its fibrous duro-matral sheath (which is continuous with the sclerotica) to a certain extent free, like a viscus in a serous cavity; for such a connexion could allow of the nerve being slid slightly back into the sheath by the long-continued intraocular pressure bearing on the lamina cribosa. Some degree of atrophy of the optic nerve appears also to have a share in the production of the excavation at its entrance. Perhaps, also, on the other hand, an elevation of the retina, in the manner of chemosis, by the congested and thickened choroid.

In the earlier stages of glaucoma the sight varies, being sometimes better and sometimes worse. The improvement of sight occurs in fine weather, after rest of the eyes and when the digestion is good and bowels free. These and other similarly favourable conditions, we can understand, promote diminution of the intraocular congestion, and thus determine a corresponding relief of the pressure on the retina. The deterioration of sight, on the con-

trary, occurs under the influence of opposite conditions, and especially mental depression, whereby the intraocular congestion is increased, and the pressure on the retina correspondingly aggravated.

In a more advanced stage, when the retina has suffered in its integrity, any occasional diminution of the intraocular congestion affords but small amelioration.

Photopsy, or the appearance of luminous spectra before the eyes, is a common symptom in glaucoma. It arises from pressure on the retina by the congested choroid. Such spectra may continue to present themselves even after the sight is lost. From seeing them the patient is apt to flatter himself with the delusive hope that sight is still in the eye, and may yet be restored.

The hardness of the eyeball in glaucoma has appeared to me to be owing sometimes not alone to intraocular distension, but also to extraocular pressure arising from a condensation and shrinking, with loss of elasticity, of the tissue of the sclerotica. This view, I find from the French translation of my work on "Ophthalmic Medicine and Surgery," has also suggested itself to M. Cusco, who designates this extraocular pressure *concentric*, whilst to the intraocular distension he gives the name of *excentric*.

I have said that arthritic posterior internal ophthalmia is identical with what is named acute glaucoma.

In its typical form glaucoma is a chronic affection, and presents itself in various degrees of development; from the case in which the patient's attention is drawn to his eyes only by experiencing an occasional dimness of sight, by seeing luminous spectra, on stooping perhaps, more readily than usual, and by a feeling of fulness in the globe;—from the case in which, if the patient be led by the subjective symptoms I have enumerated, to take advice on his case, the medical man, on examination, will find the eyeball unnaturally hard to the touch; the sclerotica dark-looking; the veins on the white of the eye I have before spoken of, turgid and tortuous, the iris inclined towards the cornea and behind the rather dilated and sluggish or immovable pupil, the characteristic greenish appearance as if of opacity. From such a case to the case in which the sight is very much impaired or altogether lost,—in which luminous spectra may, notwithstanding, still be seen—in which there is pain in the eyeball and around the orbit—in which, behind the much and irregularly dilated pupil the medical man sees, on examination, the glaucomatous opaque appearance strongly marked, if there be not actual glaucomatous cataract—in which the tissue of the sclerotica, besides being dark and dirty looking, presents here and there a condensed and whitish aspect—in which the turgid veins are unusually large and tortuous—in which the iris itself is pervaded with tortuous veins, though properly speaking not inflamed—in which the eyeball is of stony hardness—in which the cornea is dim and rough, perhaps, with its epithelium occasionally rising up in the form of large phlyctenulæ—in which, lastly, sclerotic staphyloma presents itself, perhaps, in some part of the front of the eyeball.

In the course of the disease, intraocular hemorrhage may occur. The extravasated blood may be in the substance or on the surface of the retina, and may extend into the vitreous body. In this ophthalmoscopic figure of Liebreich, which I show you, you see blood filling the excavation of the papilla optica and extending into the vitreous body. Sometimes blood shows itself in the aqueous chambers, or in the interstices of the cornea.

The various organic alterations in the eye, with the corresponding subjective symptoms, in chronic glaucoma, which I have now recapitulated, take place more or less gradually; and it may be, perhaps, only after many months that the extreme stage of development is attained. The acute form of glaucoma, on the contrary, is of sudden invasion and rapid development, though, be it remembered, that the affected eye may not have been for some time previously altogether sound, nor the patient's general condition satisfactory.

At the commencement of an attack of arthritic posterior internal ophthalmia or acute glaucoma there is often vomiting and diarrhœa; the vascular *injection* of the eye, both sclerotic and conjunctival, is great and of a pur-

plish tint; there is excruciating pain all over the side of the head; the sight rapidly fails, and may perhaps be lost in a single night; the cornea is dim and rough, and the other characters of glaucoma present themselves in an aggravated degree.—*Medical Times and Gazette, July* 23, 30, 1864, *pp.* 83, 111.

80.—ON THE TREATMENT OF GLAUCOMA BY IRIDECTOMY.

By T. Wharton Jones, Esq., F. R. S., Ophthalmic Surgeon to University College Hospital.

[In this article (Mr. Jones' third lecture) it is not the writer's intention to enter into the general treatment of glaucoma; but simply to speak of the operation of iridectomy as a means of treating the disease *apropos* of the case which forms the text of the paper. Glaucoma being a disease of advanced life, occurring especially in persons of broken health, it can be readily understood why it is an affection which should prove so rebellious to treatment.

Here was a case, then, in which the right eye was, some three months ago, operated on by iridectomy for chronic glaucoma without any improvement of the sight, though the man expressed his belief that his left eye had gained in strength since the operation on the right.

This gain for the left eye, however, did not prove of long continuance; for the man who presented himself the other week—that is about three months after the operation on the right eye—with an attack of arthritic posterior internal ophthalmia, or acute glaucoma of moderate severity of the left eye. On this eye, as you saw, I performed iridectomy a fortnight ago. On the night of the operation, and for two nights following, the patient took a pill containing two grains of calomel, one grain of extract of colchicum, and one grain of opium.

As a result of the operation and this treatment, the inflammation and pain began to abate and the sight appreciably to improve. In proportion as the blood, which was effused in considerable quantity into the aqueous chambers at the time of the operation, became absorbed, this improvement of sight advanced so far that on Tuesday last, about ten days after the operation, the man said he could see all round about him much better than he was able to do before the operation. Unfortunately, however, there supervened without any evident cause, since Tuesday, a new effusion of blood into the aqueous chambers, whereby the improvement of sight such as had been gained was to some extent lost.

On Wednesday, at my visit, I ordered the application of leeches to the temple, the effect of which has been absorption of the effused blood to a great extent, so that the sight has now regained the improvement it had lost.

This spontaneous effusion of blood, I may here remark, exemplifies the tendency to intraocular hemorrhage in glaucoma I spoke of in a former lecture.

How did excision of a piece of iris act in producing the result I have described?

Iridectomy is commonly said to operate beneficially by removing the intraocular distension, and thus relieving the retina and ciliary nerves from pressure. No doubt it appears to produce some such effect; but how does it operate in removing the intraocular distension? this is the question.

On the assumption that the surface of the iris is the principal source whence the aqueous humour is exuded, iridectomy has been said to remove intraocular distension by diminishing the extent of the exuding surface, and thus setting a limit to the accumulation of aqueous humour. We have, however, seen that whatever good iridectomy may do in the way of diminishing intraocular distension, it cannot be by removing a source of the aqueous humour, because we have shown that the iris has little if any share in secreting aqueous humour; and it is evident that if it had, the removal of so small a portion of that membrane would not be likely to produce any great impression on the quantity of aqueous humour exuded. We have, moreover,

seen that in glaucoma the intraocular distension is not owing to an increase in the quantity of aqueous humour, but that, on the contrary, it has its seat in the posterior segment of the eyeball.

An opinion has been expressed to the effect that iridectomy, by establishing a larger and freer communication between the two chambers of the aqueous humour promotes the diminution of intraocular distension. The force of this opinion, however, I cannot see, even supposing what has been just denied, that the distension has its seat in the aqueous chambers.

In reviewing our case in order to discover what can be ascertained from it in explanation of the *modus operandi* of iridectomy, we must not overlook the effect of the preliminary section of the cornea and the evacuation of the aqueous humour that takes place in relieving the tension of the eyeball at the time. We all know what relief to tension is elsewhere afforded by an incision through a fascia investing a congested or inflamed part; and although undue accumulation of aqueous humour is not, as we have seen, a cause of the distension of the eyeball in glaucoma, still the evacuation of the aqueous humour must contribute for so much to the relief of the distension.

Let us now direct attention first to the operation on the right eye. Though this eye quickly recovered from the effects of the operation, though the quantity of blood effused into the aqueous chambers was not very great, and though what there was became speedily absorbed, no restoration of sight took place. The left eye, however, as we have seen, felt stronger, and the man could exercise the sight of it better.

According to the view of the pathology of glaucoma which I have submitted to you, this result of the iridectomy on the right eye might be explained as follows:—

The intraocular venous congestion was relieved by the operation—in what manner I shall endeavour to explain by-and-bye; and the circulation of the blood within the eyeball being thus rendered freer, absorption of superabundant fluid in the cells of the vitreous body was favoured so that altogether the intraocular distension was diminished. But you will ask, if so, why was sight not restored? The answer to this, I suppose, must be that the retina had already sustained so much material organic injury from the combined congestion and pressure, that on the diminution or removal of the distension it was no longer fit to resume its function, though the pain was relieved.

The relief of the right eye, however, from congestion and distension, reacted by sympathy—or whatever other name you choose to employ to designate that influence which the condition of one eye undoubtedly exerts on the other—in a favourable manner on the left eye; so that what intraocular congestion and distension existed there, were for the time ameliorated. But as the patient is a person far advanced in life, and presents indications of a gouty constitution and broken health, we have no reason to be surprised that the amelioration in the condition of the left eye should have been for a time only; and that under the unfavourable circumstances of the late severe weather and its accompanying changes, the left eye should have become inflamed, and that the inflammation should have been what is called arthritic posterior internal ophthalmia or acute glaucoma.

From the iridectomy on this eye we have gained more for its sight than we did from the operation on the right eye for its sight.

We have seen that it is in acute glaucoma that most benefit to sight is derived from iridectomy. This effect, such as it is, must, in my opinion, be looked for in the circumstance that the disease not having been of long standing, the retina may not as yet have suffered material organic change, and, therefore, is still capable of resuming to some extent its function on being relieved from pressure by the subsidence of the inflammatory congestion, and the consequent absorption of superabundant fluid in the posterior segment of the eyeball.

Let us now endeavour to explain how subsidence of the inflammatory congestion of the choroid and retina might be promoted by iridectomy.

At my last lecture, I referred to a microscopical observation which I had

made on the tail of the tadpole when in a state of congestion—viz., that I had seen a small incision at the extremity, by emptying some of the congested vessels, relieve the congestion of the vessels higher up, so that the circulation, which was previously stagnant there, became re-established in its activity. Now, the excision of the piece of the iris, and the consequent evacuation of a small quantity of blood, appear to act by relieving in a similar manner the congestion of the choroid and retina, and thereby promoting in them a freer circulation. In fact, it is in a similar manner also, that scarification of the palpebral conjunctiva operates so beneficially when that membrane is in a state of great inflammatory congestion. It is not the mere loss of blood that does good, for that is insignificant, but the emptying of some of the vessels permitting of the re-establishment of the circulation in its activity in the part generally.

In our case, although the inflammation has been relieved and the sight somewhat improved, there is not any prospect of a restoration of what may be called *good sight*; and this judgment is, I believe, applicable to most other cases of iridectomy in acute glaucoma. In chronic glaucoma we have seen that the judgment is much less favourable. You have only to look through this table of cases in the Ophthalmic Hospital Reports (No. 10, January, 1860) to be satisfied of the inefficiency of the operation in chronic glaucoma.

You thus perceive how unfounded the claim is which has been set up for iridectomy as a *cure for glaucoma!*

The removal of the opaque lens by operation is entitled to be called a cure for cataract. So also, in the cases I have before spoken of, in which iridectomy is performed for the sake of an artificial pupil, the operation is entitled to be called so for a cure for the closed or obstructed pupil.

All this is intelligible, but as regards iridectomy in glaucoma the case is different.

Evacuation of the aqueous humour in certain cases of anterior internal ophthalmia cannot be called a *cure*, though it is often a very useful auxiliary. In like manner, *iridectomy is not a cure for glaucoma*, though it may sometimes prove a useful auxiliary in the treatment of arthritic posterior internal ophthalmia, or as it has been called acute glaucoma. Here it may be said, perhaps, *remedium anceps melius quam nullam.*

We have seen that there are certain symptoms premonitory of glaucomatous failure of sight. In such a case iridectomy has been especially recommended as a prophylactic of the threatened mischief.

If under such circumstances you should think it warrantable to propose the operation, you ought to do so only with great reserve, as you could not truly give your patient an assurance that the operation would have the desired effect of preserving his sight. The patient, until he actually finds himself rapidly becoming blind, moreover, would be slow to listen to your proposal, especially if you faithfully explained to him, as it would be your duty to do, the *cons* as well as the *pros* of the operation. For though iridectomy, as we have seen, is an operation which inflicts comparatively so little violence that the eye, in general, very quickly recovers from it, still it has not unfrequently happened that the patient has been rendered not only not better—has been left not even in *statu quo*, which is always some consolation—but has been made positively worse.

That iridectomy and extraction of the lens combined may not permanently secure the eye against an attack of acute glaucoma was shown by a case of cataract we had here some years ago. The patient was a woman about sixty years of age, and the eyes were at the time otherwise quite healthy. I performed extraction on one eye, and in making the section of the cornea the iris, in consequence of a premature escape of the aqueous humour, fell before the edge of the knife, and a very considerable piece of it was cut out. The lens was easily extracted, and the case did well. The eye, indeed, recovered so rapidly that the patient was able to leave the Hospital within ten or twelve days after the operation. Having been fitted with cataract glasses, the woman enjoyed capital sight, and continued to do so for some two or

three years. At the end of that time she one day presented herself at the Eye Infirmary, complaining that the sight of the eye was totally gone. She stated that the eye had been inflamed for some ten days past, and that she had been suffering severe pain in the head all that time.

On examination, I found the eye—the eye on which both iridectomy and extraction had been performed—hopelessly glaucomatous.

Suppose this patient had been seen earlier in the attack, would a second iridectomy have saved the eye? It is not likely, but if so, it is evident that the eye could not bear much further repetition of it, for the good reason that there would be at last little of the iris remaining to cut away.

This suggests the question—Are persons with congenital absence of the iris liable to glaucoma? Such persons are rare, and I have never seen one affected with glaucoma exactly, though I have seen one attacked with internal inflammation of the eye of somewhat arthritic character after an operation for cataract. This was the case in which, after paracentesis corneæ, the aqueous humour was as quickly and completely regenerated as usual. I do not, however, suppose that congenital absence of the iris offers any immunity against glaucoma. If so, what could an iridectomist do in case of an attack?

Iridectomy, we have seen, may be advantageously performed in certain cases of chronic anterior internal ophthalmia with extensive adhesion of the pupillary margin of the iris to the capsule of the lens. Here we can understand that one effect of the operation is really to establish a freer communication between the anterior and posterior chambers of the aqueous humour.

The operation has also been had recourse to in the hope of warding off the relapse of gonorrhœal rheumatic iritis, to which some persons are so painfully subject.

Iridectomy, however, may not secure the eyes against even a first attack of gonorrhœal rheumatic iritis, as was shown in the following case:—

Some years ago we had a man in this hospital with gonorrhœal ophthalmia affecting both eyes. When first I saw him there was considerable chemosis, and both corneæ had become opaque. Fortunately, they were saved from destruction; but after the subsidence of the inflammation, though clearing of the corneæ took place to a very great extent at the circumference, opacity still lingered in the centre, so as to obstruct the sight completely. I therefore performed the operation for artificial pupil by iridectomy on both eyes. The iris of both eyes was at this time quite sound, in proof of which I may mention, in passing, that the pieces cut out were the subject of a very successful microscopical investigation of the muscular structure of the iris, by Mr. Joseph Lister, now Professor of Surgery in Glasgow, who assisted at the operations.

The man gained pretty useful sight, and went on well for some two or three years, when he had an attack of general gonorrhœal rheumatism, in the course of which the eyes became affected with very severe and stubborn iritis. The condition of one eye in particular remained after this very much deteriorated.

If iridectomy does not secure against a first attack of iritis, it is not likely to secure against a relapse. It has, however, the effect of saving the eye from closed or obstructed pupil, and may, therefore, often be advantageously performed.

We ought not, however, to be forward in proposing iridectomy even in stubborn cases of anterior internal ophthalmia. Cases have come under my notice in which disastrous results, or, at the best, results inferior to what might have been otherwise obtained, have followed the unadvised performance of the operation. On the other hand, I have had repeated occasion to congratulate myself on the successful issue of Medical treatment, alone or aided only by the evacuation of the aqueous humour in cases of the most threatening character. In one case which lately came under my care, the opinion had been given that iridectomy was the only remaining resource. Notwithstanding this, a cure was effected without even the aid of *paracentesis corneæ*.

Too hasty a proposal of operations, let me observe, in conclusion, betrays imperfect command of the resources of Medical treatment. To operate is far easier than to conduct Medical treatment properly—it is to cut the string instead of untying the knot.

It is sometimes said that the Medical treatment injures the health of the patient. I deny this, if the treatment be properly conducted. No doubt the health of the patient is often injured by ill-conducted treatment; but ill-conducted treatment, at the same time, fails to cure the eye. You may be sure that the eye will never be cured at the expense of the general health. In my experience, when an affection of the eye has been cured, the general health has been at the same time improved by the treatment.

In illustration of what I say, I may mention a case which occurred to me a great many years ago. It was a relapse of very severe gonorrhœal rheumatic iritis, in which the general health was so much benefited by the Medical treatment employed for the cure of the eye, that the patient, some years afterwards, finding himself out of condition, wanted me to subject him again to similar treatment, although the eye itself continued well.—*Medical Times and Gazette, August* 6, 1864, *p.* 139.

81.—CLINICAL DATA RESPECTING CEREBRAL AMAUROSIS CONNECTED WITH THE USE OF TOBACCO.

By Jonathan Hutchinson, Esq., Surgeon to the London Hospital, and Assistant Surgeon to the Royal London Ophthalmic Hospital.

[The opinion has long been entertained that tobacco is a frequent cause of loss of sight. This effect of its constant use is noted by Mackenzie in his "Diseases of the Eye." At the end of a very long and very able paper, Mr. Hutchinson sums up the subject as follows:—]

1. That there is a special form of cerebral amaurosis usually unattended by other permanent disorder of the nervous system, in which white atrophy of the optic nerves supervenes.

2. That there is little doubt that this form of amaurosis commences centrally, the alterations in the supply of blood to the optic nerve and retina being secondary.

3. That in the early stages of this disease, symptoms of functional brain disturbance are usually present; somnolence, headaches, giddiness, slight stupor, and sometimes vomiting.

4. That the symptoms last-mentioned almost always pass off when the amaurosis is more advanced.

5. That the rapidity of the extinction of sight is usually in ratio with the severity of the other cerebral symptoms.

6. That this form of amaurosis may occur at any age, and in either sex.

7. That it now and then follows exanthems, congenital syphilis, disturbed menstrual functions, great anxiety, injuries to the head, &c.

8. That it occurs in large excess in adult males, typical and uncomplicated examples of it being indeed very infrequent in women.

9. That in men it is sometimes attended by disturbance or suspension of the generative function, but that in the great majority of cases no such connexion is proved.

10. That there is no reason to think that it is frequently consequent on masturbation, or that it occurs in any special connexion with acquired syphilis or intemperance.

11. That no adequate explanation can be afforded of the clinical fact that men suffer from it much more often than women.

12. That it does not occur in connexion with any special occupation.

13. That a large proportion of those who suffer from it have been smokers.

—*Clinical Lectures and Reports of London Hospital, vol.* 1, 1864, *p.* 81.

82.—A NOTE ON REFLEX DISEASE OF THE EYE, OF TRAUMATIC ORIGIN.

By J. Vose Solomon, Esq., Surgeon to the Birmingham and Midland Eye Hospital.

The Mechanical Irritation of an Orbit from which the Globe has been removed is capable of Producing Symptoms of Reflex Disorder in the Previously Healthy (?) Eye of the opposite side.—W. V., a healthy man, thirty-six years of age, by trade an iron moulder, received a severe burn of his *left* eye, from the spurting in of some molten iron, on the 1st of July, 1857. He obtained prompt and careful treatment, yet the vision was lost, and a connecting band formed between the globe and its lower lid. Early in the following September the right eye suffered sympathetically. It was painful, and there was a sensation of external compression. The vision, which was best in bright light, became, at the same time, disturbed by what appeared to be a cobweb, which constantly floated before the eye. The pain and dulness of sight increased so much that, on the 17th of March of the following year, 1858, the patient requested me to remove the disorganized globe, which was accordingly done by enucleation, in the usual way. The optic nerve was divided close to the globe. In three hours after the operation there was a marked mitigation of the severe cutting pains which had affected the right eye during the last three or four months, and the cobweb (*musca*) appeared to be much smaller. In the course of a short time, the band which connected the lower lid to the globe was regenerated, and presented an obstacle to the introduction of an artificial eye. It was therefore divided, and a glass mask inserted in the orbit. The mask excited a good deal of local pain and conjunctival inflammation, which, in its turn, reproduced sympathetic ocular disorder of the same degree and kind as had existed before the excision was performed. On the withdrawal of the mask, and subsidence of the conjunctival irritation, the left eye assumed its former condition of usefulness. At the end of a fortnight the mask was once more used, when the sympathetic symptoms reappeared—flashes in the dark (*photopsy*) being superadded. The patient has now for more than four years been engaged in his trade, as an iron moulder, taking the precaution of ceasing work whenever the eye becomes dim or affected with flashes. The sight is generally clear, and he is capable of reading small print without fatigue.

English authors on ophthalmology have hitherto attributed the occurrence of sympathetic traumatic ophthalmia to the commisural arrangement of the optic nerve fibres.

The phenomena presented by the preceding case do not appear to afford a *locus standi* for such theory, inasmuch as the optic nerve on the side of irritation had been severed; but they may be satisfactorily explained on the supposition that the irritation from the eyeless orbital cavity was conveyed by the ophthalmic division of the fifth pair of nerves to the centre, and thence reflected to the opposite side. This view derives support from examples of reflex traumatic disorder wherein the symptoms are purely those of hyperæsthesia of the fifth, and which yield, almost *instantaneously*, on removal of the original source of irritation. Of this class of cases some account is given in a paper which was read by me, before the Birmingham Branch of the British Medical Association, on April 9th, 1857, and published in the journal for that society for June 13th (p. 496) of the same year.

Careful observation of a large number of instances of reflex ophthalmia—a disease of great frequency in Birmingham, and the neighbouring mining districts—has led me to the following, among other conclusions, namely:—that the *primary* traumatic irritation is conveyed to the sound eye through branches of the fifth nerve; that soon afterwards, and sometimes simultaneously, the vasa-motor nerves take on disordered action, and, as a consequence, intraocular congestions and their results occur; that, if the disease be not arrested, the optic nerve fibres become inflamed, or affected with such other changes as induce atrophy, and which lead to similar degeneration of *the opposite* nerve, attended by an amaurosis, which is incurable. In this

stage of the complaint the commisural arrangement of the optic nerve fibres is probably the channel through which the disorder is conveyed.

When the state of the eye admits of its fundus being examined by the ophthalmoscope, in the early stage of reflex ophthalmia, the most constant appearance is congestion of the vessels of the retina, and more especially of the veins; but where there is an advanced amaurosis the usual signs of atrophy of the optic nerve entrance, which are sufficiently familiar to render description in this place needless, are revealed.—*Dublin Quarterly Journal, February*, 1863, *p.* 58.

SYPHILITIC DISEASES.

83.—REMARKS ON SYPHILIS AND ITS TREATMENT.

By Dr. Henry Veale, Assistant Surgeon, Royal Artillery.

In the therapeutics of Syphilis, the three following rules may be regarded as absolute. 1st, That every non-indurated chancre should be thoroughly cauterised if the ulceration be progressive or stationary; 2d, That the escharotic employed for the destruction of a venereal sore should never be of less strength than the fuming nitric acid; 3d, That every suspicious excoriation should be treated as a chancre. It is very seldom that any difficulty is experienced in obtaining the cicatrisation of venereal sores when they are treated in this way.

It has never occurred to me to meet with a true serpiginous chancre in military life, nor have I ever known a soldier in whom it had existed.

Sloughing sores—sloughing phagedæna—are more common, and are almost invariably associated with a bad habit of body of one kind or another, and, after cauterisation, are best treated by the tartrate of iron, both locally and internally, as Ricord long ago pointed out.

Buboes.—It is acknowledged to be useless to attempt the repression of a specific bubo, and as it is impossible to foretell whether any given bubo accompanying a venereal sore will prove to be specifically infected or not, it would seem to be prudent never to attempt repression at all. The result of such preventive measures as are usually recommended, viz., leeches, blisters, low diet, antimony, and other similar antiphlogistics, appears to me to be as unfavourable both to the part and to the system as any that follows suppuration itself, even when they happen to be successful; but when they fail, the strength of the system has been wasted to no purpose, and a bubo remains, slow in its progress towards recovery, and, by its exhausting discharge, often developing any lurking tubercular tendency. As soon as a patient with a primary sore begins to have pain and swelling in the groin with other symptoms indicative of incipient bubo, it will generally be found that the best practice is to apply hot fomentations and poultices, to enjoin absolute rest of the part, at the same time to diminish in no respect our care for his general health, and, if pus forms, to make a small but dependent opening the moment its presence is detected. I am opposed to waiting until more complete suppuration shall have occurred, as well as to the practice of "free" incision, and could easily adduce cases in support of what I deem to be the more suitable treatment.

It may be concluded, that the moment pus is detected in a bubo it ought to be evacuated, and by a small incision: the latter being advisable in all cases, first, for the sake of preserving appearances, and, secondly, for the limitation of the extent of surface to be infected if the pus should prove of a chancrous quality. This point may always be determined in the course of a few days by the appearance of the opening. If it show a tendency to heal, we may be sure that we have not to do with a chancrous bubo, but if it have a red, irritable, ulcerating aspect, or if its margins become ash-coloured and sloughy, the case is otherwise, and the part should be dressed with a strong solution of nitrate of silver, sulphate of copper, black wash, or some similar application. Injections of the same should likewise be made into the interior

of the bubo, and this treatment should be continued until the specific nature of the ulceration has been eradicated.

But, unfortunately, patients frequently avoid applying for assistance until the mischief has been done, and sometimes also we fail to detect the presence of pus early enough to enable us to follow this line of practice. In such cases, when we make the opening and introduce a probe, we find that it passes freely between the gland and the skin, showing that the subcutaneous tissue has already, so far, been destroyed, and the best practice then is to divide the skin by a single or a crucial incision, so as fully to expose the gland. The skin being then retracted and most carefully preserved, the gland should at once be destroyed by the chloride of zinc or some other equally powerful escharotic, whilst nitric acid should be applied to such of the surrounding parts as may have been infected; and when the sloughs separate, and the part again puts on a healing appearance, the skin should be drawn over it and union be promoted. It is really surprising, sometimes, to see with how little deformity a man may thus escape, even though the incisions at different times may have extended for several inches, and so many as three or four glands have been destroyed. It may, perhaps, be objected that sinuses are liable to follow upon small openings of the kind I have recommended, but, without entering into an examination of the process by which sinuses are formed, which, however, would probably suffice to refute the objection, I may perhaps be permitted to lay some stress on the fact, that I have never once observed a sinus in the groin to be so produced.

With reference to the general or constitutional treatment of venereal sores and buboes of the non-infecting kind, my belief is that measures of depletion are never required or even advisable. On the contrary, if the patient's health chance to be good, we should endeavour to keep it so; but if it be already in a bad state, it is almost needless to add that a tonic method of treatment is indispensable.

I shall now proceed to the consideration of the indurated chancre, and of constitutional syphilis.

Until an indurated chancre has been followed by enlargement of the lymphatics in the neighbourhood, I am of opinion that it is both justifiable and expedient to obtain the destruction of the part affected by it, for which purpose excision is especially applicable when the sore happens to be seated on any part which, like the prepuce, admits of ablation without detriment or disfigurement to the patient. I make this statement with the utmost possible deference for those observers who affirm that it is useless to extirpate an indurated chancre. I do not pretend to deny that most frequently, when the induration has become manifest, the entrance of the poison into the system is inevitable, and has perhaps already taken place; but I think it is an error to conclude that the induration is the result of the general contamination and a proof of it. For, if it were so, why should it occur only once? why should we be unable to reproduce it by a subsequent inoculation? why should not every superficial solution of continuity occurring at this period also undergo the same process? and why should it occur only at the site of inoculation? It would seem to be more probable that the specific induration is a strictly local process, dependent, it may be, upon the conjunction of several conditions; of which, however, the following may be regarded as the chief:—First, the constitution must have been previously free from syphilitic infection. It must not have undergone that kind of leucocytosis which the syphilitic virus produces. Indeed, every leukæmic state seems to be adverse to the process of induration; for we find that in women, in whom a chloro-anæmic or leukæmic state is more common than in men, the induration of chancres is much less frequent; and it is also observed that a chloro-anæmic state of the system, however produced, delays the specific induration even in men. Secondly, a certain degree of activity or concentration of the virus seems to be necessary; for, according to several observers, the induration produced by inoculation through the medium of the blood, or with the fluid of certain secondary lesions, is not so well marked as that which results from *the action of the* virus implanted directly from a chancre. Thirdly, certain

purely local conditions seem to be favourable, although by no means essential. Thus the process of induration is usually much more complete on the prepuce and on the lips, for example, than it is on the trunk, and it may perhaps be owing to a similar special aptitude or affinity that the lymphatic glands are so constantly and readily affected by it.

The sequence of events in syphilis would seem to be nearly the following:—

1st, Induration of the part inoculated, equivalent to increase of virus by molecular or cell development.

2nd, Absorption and transmission of virus to the contiguous glands; induration and irritation of these organs; leucocytosis; and, consequent thereon, modification of nutrition, owing to which the capability for induration diminishes, and for the most part ceases.

3rd, Passage of the virus into the blood; its elimination through the skin and mucous membranes, and, consequently, eruptions and ulcerations.

4th, Absorption by the lymphatics of the syphilitic virus not wholly eliminated; irritation of other glands; transmission of virus from one set to another; further changes in the blood; lesions of nutrition; repeated passage of poison into the blood, followed by fresh eliminative efforts; and so on.

5th, Elimination failing; alteration in quality of virus and its deposition in certain structures; profound lesions of nutrition.

Syphilis in fact seems to stand midway between small-pox and cancer,—the capacity for elimination of their respective morbid poisons constituting the main difference; and certainly, if we accept the proposition of Virchow, that "every dyscrasia is dependent upon a permanent supply of noxious ingredients from certain sources," and that the blood neither is nor can be its real seat, and then look to the fact that the lymphatic glands are the only structures which in secondary syphilis remain permanently indurated, we are almost forced to the conclusion that they constitute "the local depots from which new quantities of noxious matter are continually being introduced into the blood." This theory is certainly more tenable than that which supposes the poison always to exist in the blood, and at certain periods, viz., at the outbreak of cutaneous or mucous manifestations, to undergo a kind of fermentation or zymosis; but it would occupy too much space to pursue it farther in this direction for the present.—*Edinburgh Medical Journal, July*, 1864, *p.* 10.

84.—TWO CASES OF SYPHILIS SHOWING A PROLONGED INCUBATION PERIOD AND COMMUNICATION OF THE DISEASE BY SECONDARY CONTAGION.

By Berkeley Hill, Esq., Assistant Surgeon to University College Hospital.

On the 5th of March, 1864, John J., aged thirty-three, ostler, applied, among my out-patients, for relief for a painful affection of the right eye. He said that about fourteen days before Christmas last, while fighting, he received a blow on the right eye and cheek, which drew blood; his antagonist sucked the wounds for him, after which they quickly healed, and, as far as he knew, the marks also disappeared. He experienced no further inconvenience until the latter end of January, when he observed some pimples appearing where he had been hit, and presently some scabs fell off, leaving a reddish pimple beneath each; but there was no ulcer, nor any discharge from these pimples. His eye next became troublesome, growing red and bloodshot, and smarting occasionally; and on Feb. 1st he applied for some eye-water, with which he bathed his eye, but without improvement. Finding the eye-water of no service, on the 5th of March he came to me, anxious for other treatment. I examined him, and found the following state of things:—At the outer corner of the right eye was an oval coppery patch, slightly elevated from the skin around it, especially so at the edges; it was smooth and dry. There was also about the middle of the margin of the lower eyelid

another smaller patch, which desquamated freely, and whence the eyelashes had dropped out. Two more similar patches existed on the cheek, over the malar bone. All these tubercles were indurated, and surrounded by an areola of coppery tint. The conjunctival membrane of the right eye was congested and the palpebral part thickened; whence the discomfort for which relief was sought. The lymphatic glands beneath the jaw and in the neck on that side were severally enlarged, but painless. A coppery roseolous rash extended over the forehead and trunk. The penis was quite free from sores or cicatrices of any kind, and there was no history of any. The inguinal lymphatic glands were also quite normal; likewise those of the body generally, with the above-mentioned exception of the submaxillary ones. Though he complained of sore throat when questioned, the soft palate and uvula were only somewhat congested. He was ordered to take four grains of blue-pill with a little opium twice daily, and to attend frequently at the hospital.

On the 12th of March I saw him again. He was then under the influence of mercury. His gums were swollen, his breath was fetid, and he had a bad taste in his mouth. His throat was not sore. The areolæ round the hardened tubercles less spread and paler; the roseola much fainter; more of his eyelashes had fallen. To continue his pill.

March 19th.—Induration of the tubercles less defined, and the glands under the jaw smaller. He looked paler, and felt weaker than before—probably from the combined effects of scanty diet with mercurialisation, as he had been out of work that week. I ordered him to take a little quinine-and-iron, and to take his pill once a day only.

30th.—The glands under the neck were less enlarged, the coppery tint of the patches on the face were much fainter, and the induration withering. The throat still not sore; and no other eruption visible on the body.

April 20th.—The coppery discolouration remained in two places only, and was of very small extent. The induration of the tubercles was perceptible in one cicatrix only. The eye was quite well, except that the lashes had not grown again. The lymphatic glands were of their usual size, and the man felt quite well. Having found work in the country, he had been unable to show himself at the hospital, and had not taken medicine for a fortnight. He was ordered to continue his pill once every day for a little time longer.

On the 26th of March the patient led in his late antagonist for examination, of whose condition I took the following note:—F. M., aged thirty-one, a wheelwright of intemperate habits. He stated that when he sucked John J.'s eye he had a sore at the corner of his mouth (where there is one still), and that he had some sores on his penis at that time, which had existed for six weeks or two months before the time at which he gave the blow to John J.; but they did not trouble him much. However, he recollects that the lumps now in his groin were there then, and that they were rather tender. He has never observed any rash on his skin or soreness in his throat, and has felt very well ever since. On examination, the sore proves to be a fissure at the left angle of the mouth, with one or two enlarged papillæ round it, which are moist and scaling, not possessing any induration however. There is no ulceration on the mucous surfaces of the mouth and fauces; the lymphatic glands are not enlarged under the jaw, or at the back of the neck. The body is free from eruption of any kind, save that one or two acne spots on his shoulder have a coppery tint. On the penis, behind the corona glandis, are two scars, with well-marked induration—the sites, the patient says, of the sores he had last December. The lymphatic glands in both groins are plainly enlarged, but not at all tender, and the skin covering them is of its usual colour. He had never taken any medicine for his disease; and the only inconvenience of which he has been conscious is the persistent sore on the mouth. I ordered him a small quantity of bichloride of mercury, with iodide of potassium. This treatment has been pursued during the month of April, and the sore on his mouth has healed. The induration has much diminished on the penis, and the patient has preserved his good health.

These cases show very clearly:—First. That there exists an incubation *period in syphilis* between the moment of inoculation and the manifestation

of its effects, which has a duration not very exactly known. In the cases of V. Bärensprung, where inoculation was purposely practised, the incubation lasted twenty-eight or twenty-nine days; Sigmund and Rollet both estimated it at about three weeks, with extremes of fourteen and forty-two days; Hunter relates an instance of two months intervening between the time of contagion and the appearance of the disease; and Aimè Martin, in his thesis for 1862, mentions the case of a girl incarcerated in the St. Lazare Prison, in close confinement on the 16th July, 1861, on whose labium a syphilitic sore made its appearance on the 25th September following—a period of seclusion of seventy-two days. The case I am relating had a long period of incubation, about five weeks or rather more, as nearly as can be estimated, but not of an extreme length if compared with those related by other observers. Secondly. The kind of primary lesion produced by the disease when other disturbing causes are absent is here well shown. The indurated tubercles grew at the point of inoculation, and never ulcerated, perhaps because they were free from irritation, resulting from the moistening by secretions, urine, &c.; though I do not mean that such irritation is in all cases the cause of ulceration in primary syphilitic affections, but that these are probable causes of it in many instances. These papules or tubercles—for some resembled one and some the other—had been slowly developing themselves for six or seven weeks, and had become indurated to such an extent that one was as large as a sixpence; yet their surface was unbroken in all that time. In this case at least the primary affection bore no resemblance to a Hunterian chancre. Thirdly. The disease was communicated by contagion with secondary secretions—namely, the fluid exuding from the sore at the angle of the second patient's mouth was inoculated into the open wounds of the cheek of the first patient; and that this sore was a secondary affection is shown by the pre-existence of the primary disease in another part of the man's body, which, since we cannot produce a primary lesion of syphilis on a person already subject to the disease, prevents the possibility of the sore at the mouth being one.

I have brought forward these cases for the readers of the Lancet, because they are examples of an unusual mode of communicating the disease—namely, by suction of open wounds, though a precisely similar means of contamination takes place when nurses are inoculated by their foster-children, in which cases the primary lesion is a chancre on the breast.—*Lancet, June* 18, 1864, *p.* 696.

85.—ON THE TREATMENT OF SYPHILIS BY MERCURIAL VAPOUR.

By E. W. Pollard, Esq., Brompton.

The milder influence of mercury when endermically applied, compared to the effects of its internal use, has long been acknowledged. Its inunction, except for personal reasons, was generally preferred to its administration through the alimentary canal. To avoid the inconvenience of inunction, the late Mr. Thomas Pearson employed the red sulphuret of mercury (cinnabar) in a state of vapour, but which was found to produce irritating effects, and was consequently generally abandoned in this country. Mr. Henry Lee, a successor of Mr. Pearson at the Lock Hospital, has recently revived this mode of treatment with great success, substituting calomel for cinnabar, and adopting a simple apparatus by which the calomel is volatilised, and mixed with the vapour of water.

Sir Ranald Martin also long since insisted upon the necessity of making the skin a perspiring and absorbing organ, in order that medicine may act upon the general system when applied to it. The skin, however, in certain persons, especially when thickened by disease, will not absorb readily. Acting upon the suggestion of Mr. Henry Lee, I have lately tried, upon a somewhat extensive scale, the effect of softening the skin by producing gentle diaphoresis previous to employing the vaporised calomel. Possessing

great facilities for this purpose in our establishment, we have modified and improved upon the apparatus employed by Mr. Lee—first by allowing the patient to remain for a short time in the tepidarium, when the skin, becoming moistened, is prepared for the action of the metallic vapour, which is applied in a carefully-constructed box, where the calomel is volatilised at a regulated temperature with the vapour of water, and the sublimed particles are equally diffused and deposited on the person of the patient. Inhalation not being always necessary, the head is excluded. For some months we have had daily opportunities of observing the effects of fumigation so carried on in the cases sent to us by our medical friends, and by their permission we are enabled to record the following cases. In these records our duty has been strictly limited to a faithful statement of facts; we have indulged in no speculations, leaving the profession to draw their conclusions.

"A gentleman, who had come from abroad on purpose to obtain the benefit of medical advice, had taken all the ordinary internal remedies for constitutional syphilis, but still remained a sufferer from most painful and intractable ulcerations of his legs, and at times most severe pains in his head. It was found by him that the use of the calomel bath in the ordinary manner produced very little effect. Mr. Henry Lee, therefore, recommended him to have a certain amount of diaphoresis induced previous to taking the calomel bath. He commenced with one three times a week (half a drachm of calomel). He speedily found relief from his pains, and the ulceration rapidly healed up. He then went into Scotland for two or three months. Upon his return he came to the baths, and stated that he continued well, and has now gone abroad."

"*Inveterate psoriasis.*—Mr. A., a young man in the army, had been afflicted with a most inveterate form of psoriasis for nearly nine years. The eruption extended over the whole body and extremities. The scales were so thick as to make motion of the arms difficult, and he was unable to perform his duties in the service. He has been under every kind of treatment; has taken large quantities of arsenic, iodide of potassium, &c., but only experienced slight temporary relief from the remedies. His gums were spongy, with great fetor of the breath. There were doubts whether the affection had a syphilitic origin.

"Previous to his coming to London in March last, he had taken several calomel baths with the ordinary apparatus, with some improvement. Mr. H. Lee ordered this patient to take calomel baths (half a drachm), to be preceded always by a moderate amount of perspiration in the hot room (temperature 120°). In one week after the commencement he was greatly improved; and he was ordered to continue them daily. In three weeks he was free from all scales, the skin was quite smooth, and only a slight discolouration remained. He continued to take the bath for some time, and has had no return of the complaint. His general health has greatly improved, and he considers himself effectually cured."

"Mr. D., a medical practitioner, had primary symptoms thirteen years ago, which readily yielded to inunction. Sore-throat and eruptions followed, which were relieved by the iodide of potassium, in combination with tonics. Subsequently the tongue became tuberculous, fissured, and ulcerated, rendering the taking of food painful. The ulcerations healed after long-continued treatment with the iodide, but the tubercles and indurated fissures remained. If the iodide were omitted for any length of time, the ulcerations were certain to return; so that he was always obliged to be more or less under the influence of the remedy, of which he must have taken many pounds. Mr. Henry Lee advised the calomel bath, with the previous perspiration. The tubercles began to subside after the third bath, and at the end of three weeks very little remained of the tubercles. The bath was taken very irregularly. He had then a slight return of the ulcerations, which yielded immediately on the resumption of the bath. For some time past he has had no return of this troublesome disease, and has not taken any of the iodide."

"Mr. H. had a primary sore four months ago, which was followed by *sore-throat and eruptions*. He was treated in Paris with the photo-iodide of mer-

cury, but not being satisfied he came to London. The eruption is over the body and face, with some sore-throat. Dr. Cahill and Mr. H. Lee recommended a calomel bath (half a drachm of the mineral) daily. At the end of three weeks the eruption was entirely removed."

"Mr. S. had a chancre on the penis, accompanied with syphilitic bubo, which suppurated. There was a dark copper-coloured eruption over the whole body, which had existed for several months, and first appeared after a previous syphilitic affection. This patient was in a weak and debilitated state. His health improved under a short course of tonics, and he was then ordered to take the calomel bath. The chancre disappeared after six baths, the bubo healed up, and the eruption was entirely removed. At the present time he has not a trace of disease upon him, and has not, in fact, been in such good health for a very long period. This patient had also been very deaf since his first syphilitic attack; he has quite recovered his hearing."

"Mrs. S. This patient was contaminated by the previous one. No primary sore could be detected. Had syphilitic bubo, sore-throat, and eruptions over the whole body. She took the calomel baths for three weeks, at which period all the symptoms had disappeared, and she remains quite well."

"W. H., a soldier, had a large sloughing chancre. The vapour of calomel was applied directly to the surface of the chancre. After three applications the ulceration stopped, and the surface assumed a healthy appearance. He was only able to take twelve baths, being under orders to embark on foreign service. The chancre was healed up, and all induration had disappeared."

Dr. Cahill has kindly sent us particulars of some of the cases treated by the hot air in combination with the calomel bath.

"*Secondary syphilitic sore-throat.*—Mr. D. had been subject to several attacks of severe dyspnœa. On examination of the throat I found traces of the previous disease, for which he had taken iodide of potassium. Ordered a quarter of a grain of bichloride of mercury and two minims of tincture of opium twice a day, under which treatment he was much benefited; but still the disease constantly recurred, yielding to the same remedies. I then requested him to take the calomel baths, and to have slight diaphoresis produced previously by remaining in the hot room for ten minutes. Mr. D. continued the baths regularly three times a week for three weeks, at the end of which period he became entirely free from his complaint, and has had no return."

"*Secondary sore-throat with rash.*—Mr. C. On being called to this patient, I found him suffering from gonorrhœa, with severe orchitis, which yielded to the ordinary treatment. On the phimosis being subdued a sore was found on the glans, beside the frenum. Not having all the characteristics of an ordinary chancre, which, however, healed on the application of the red precipitate, still leaving a thickened, warty base, I ordered him quinine and steel for some time. At the end of a few weeks he returned to me with a severe sore and copper-coloured eruptions. I then put him under the proto-iodide of mercury, and inunction of half a drachm of mercurial ointment every night. The disease was checked, but did not make much progress towards recovery. I then recommended him to commence the mercurial fumigations and the hot-air bath. After taking twelve baths all the symptoms disappeared, and he has continued well now for several months."

"*Secondary sore-throat; rashness followed by deafness.*—Mr. E. contracted gonorrhœa in 1862. Subsequently an excoriation (or chancre) was discovered, followed by pain in his limbs and sore-throat He was sent to Brighton for bathing, from which place he returned quite deaf. He first came under my care at the latter period. I found induration where the sore had been. This, with the length of time that had elapsed since his first being taken, confirmed me in the supposition of the syphilitic taint. Other symptoms developed themselves, which corroborated the opinion I had formed. All the symptoms were relieved by appropriate treatment except the deafness, which remained till I ordered him to take the mercurial vapour baths, which speedily re-established his hearing."

"*Deafness following secondary symptoms.*—Mr. A., a young gentleman,

had syphilis in 1862. The primary sores were treated with the exhibition of mercury. The secondary sore-throat was treated by me by the use of the iodide of potassium; and subsequently another medical practitioner put him under the bichloride of mercury. All the symptoms were removed except deafness, for which everything was tried—iodide of potassium, blisters, &c., but in vain, so that he was necessitated to resign his commission. On his return to London, at the beginning of this year, he tried by my advice the mercurial vapour-baths, from which he derived the greatest benefit. He can now hear distinctly the notes of the piano, whereas before he could only hear a confused drumming noise."

"*Tertiary sore tongue and mouth; sores on the head.*—Mr. B. had primary syphilis in 1861, for which he was treated in France; subsequently, sore-throat, rash, and nocturnal pains. He has taken mercury, iodide of potassium, and the waters of Aix-la-Chapelle. At the time he came under my care he was in a miserable condition from his mouth, tongue and cheeks, which had whitish sore places all over them, and were very painful upon taking food. The sores were partially relieved by the application of the sulphate of copper. The mercurial vapour-baths produced a most salutary result, and he has had no return of his complaint, though several months have passed away."

"*Early secondary rash and sore-throat.*—Mr. H. had syphilis in November, 1863, followed by rash and sore-throat. He had been treated by mercury and iodide of potassium previously to my seeing him. A dozen of mercurial vapour-baths completely removed all his symptoms, and he has continued well up to the present time."

I might multiply cases in which cures have been effected by the mercurial bath, but enough have been cited to direct further attention to the subject.—*Lancet*, *July* 30, 1864, *p.* 116.

86.—THE TREATMENT OF SYPHILIS BY THE MERCURIAL VAPOUR BATH.

By Langston Parker, Esq., Birmingham.

[Mr. Parker was the first to draw attention to the advantages to be derived from combining the vapour or fumes of various preparations of mercury with aqueous vapour, and to recommend a bath, which he denominated the "mercurial vapour bath," in which the patient was exposed to the influence of three agents—the vapour of some preparation of mercury, heated air, and common steam.]

I recommended in the first instance the vapours of the bisulphuret of mercury, the gray oxide, the binoxide or the iodide; and to these Mr. H. Lee has recently added calomel. The advantages to be derived from this mode of treatment can hardly be over-estimated; for there is no doubt about the fact, that the way to cure or eradicate syphilis is by remedies applied to the skin, and not taken by the mouth, and that the treatment by moist mercurial vapour is, in a vast majority of instances, the method to be preferred before all others. I have now personally superintended the treatment of many thousands of cases in this way, and I could record instances of success in many cases which really appeared hopeless. The preparations of mercury best suited for moist fumigations are, doubtless, calomel and the bisulphuret. In scaly diseases of the skin I generally use these combined. I have seen one case indeed of most extensive secondary ulceration, with disease of the bones of both legs, entirely cured by the vapour of the biniodide, used by mistake. Should this ever be employed, it should be used with extreme caution, and care should be taken that the vapour is not inhaled.

The mercurial vapour-bath is much more certain in its effects upon some forms of syphilis than others, and it is chiefly applicable in secondary or constitutional syphilis, and especially in the scaly forms of skin diseases. It *finds its application*, however, in a great number of other forms of disease

besides those I have mentioned. In tubercles before they are softened, in secondary ulcers succeeding rupture of pustules, and in diseases of the testes, and many forms of affections of the bones, it is exceedingly useful. The moist mercurial vapour employed in the way I have recommended is a perfectly safe remedy; it does not depress or debilitate the patient, if properly used. The disease commonly disappears without any of the usual effects of mercury being evident, and I have no doubt this is due to the diaphoresis going on during the time that the patient is exposed to the action of the vapour, and it is from the same cause that ptyalism is rarely induced. —*Lancet, August* 20, 1864, *p.* 212.

87.—ON INFANTILE SYPHILIS.

By J. Vose Solomon, Esq., Surgeon to the Birmingham and Midland Eye Hospital.

[The following case by Mr. Solomon is worthy of attention.]

On the 11th of September, 1860, an infant (L. L.), 8 months of age, was admitted under the care of Mr. Solomon, for a small unhealthy ulcer, with an indurated base, situated at the inner extremity of the lower lid, close to the roots of the eye-lashes. It had been there a fortnight.

The child was somewhat strumous-looking, fairly nourished, and could not be said to be unhealthy. It had not suffered from "snuffling" or any sort of disease. The mother, who brought the patient to the hospital, was in appearance healthy, and close questioning failed to elicit a syphilitic history. The father also was healthy, and denied having suffered from sore throat, falling off of the hair, or disease of the skin. The ulcer on the infant was considered by Mr. Solomon to be syphilitic; but, in face of the foregoing facts, soothing topical applications only were ordered. In three days later, no amendment having taken place, quinine was ordered, and the sore to be dressed with diluted citron ointment. Change for the worse, rather than the better, having taken place, inquiry was made whether any one had been in the habit of nursing or fondling the child who was suffering from a skin eruption or sore throat. It was replied, "Yes; her aunt is so affected." When this lady presented herself at the hospital, she was found to be covered with copper-coloured blotches: and her tonsils were fissured with ulcers, which afforded a discharge. These diseases had been attributed to bathing in the sea, and permitted to run a natural course. The inference Mr. Solomon drew from this examination was, that the aunt, in kissing the infant, had inoculated its eyelid. The inner extremity of the lid, close to the muco-cutaneous margin, is, according to his experience, more susceptible to primary syphilis than any other part of the palpebra. Any one who has once seen syphilitic tubercle of the eyelid could not mistake it for a primary ulcer.

Small doses of mercury were now given twice a day, in combination with Dover's powder and sugar. Nevertheless, on the 28th of September, six weeks from the appearance of the primary affection, which had healed, the child's buttocks presented well-marked syphilitic blotches; and on the 23rd of October, a rather large, ill-conditioned, and deep ulcer had invaded the external labium. Syrup of iodide of iron had been ordered on the 19th, in addition to the mercury, which was given once a day only.

After a while the child's mouth presented a fissure at its commissure. An ulcer appeared on the mother's breast, and she had secondary symptoms.

The case of the infant was carefully watched by my friend and late pupil, Mr. Walker, now of the firm of Bull and Walker, of this town; and that of the mother by Mr. Baines, who attended the family, and to whom the case was referred for treatment.

Remarks.—The fact of this little patient having attained nearly the eighth month of its age before the appearance of the primary chancre, excludes all grounds for believing that the disease was contracted during the parturient process. How then was the inoculation effected, and from what source?

The answer is found in the common practice among some women of fondling children by *sputtering* upon their faces, and so strange is taste, upon the genitals and nates of infants. By these acts, the secretions of the mouth are frequently deposited upon the place of contact. It was probably in this way that the discharge from the aunt's tonsils was conveyed to the eyelid of her infant niece.

In opposition to this theory, we have the high authority of Dr. Diday, who declares he has been unable to discover evidence of the occurrence of a primary syphilitic sore as a result of inoculation from a secondary disease. The case before us is, in this respect, *unique*, inasmuch as the affection of the mouth of the suckling was a secondary non-congenital disease, and by it the nipple of the mother was inoculated. The specific character of that infection having been placed beyond dispute by the occurrence of secondary symptoms. We therefore conclude that the discharge from a secondary sore has given rise to a syphilitic ulcer, which, in its turn, produced secondary symptoms.—*British Medical Journal, March* 7, 1863, *p.* 236.

88.—SYPHILITIC ERUPTIONS.

By Dr. TILBURY FOX, Senior Physician to St. John's Hospital for Skin Diseases, and Physician to the Farringdon General Dispensary.

The syphilitic poison once introduced into the system is apt to be followed by certain eruptive manifestations: these are only the naked-eye evidences of a deep-seated change in the system at large. The functions of various organs are deranged, "the blood is charged with a poisonous principle, and all the organs and structures supplied with that blood suffer to a greater or less extent. The brain evinces its suffering by mental dejection; the nerves by a general feeling of prostration and debility . . . . there is often neuralgia (nocturnal) . . . . the pulse is quickened . . . . the tongue coated, white, broad, and indented by the teeth. The fauces are more or less congested, the tonsils and soft palate being frequently swollen; there is irritation of the larynx, producing a mucous cough and often nausea . . . . the conjunctiva is congested and muddy, and the whole skin remarkable for its yellowish and dirty appearance, looking as if saturated with impure and discoloured humours.

Syphilitic eruptions have certain peculiarities:

1. There is *a history of syphilitic inoculation*, which tells its tale by the numerous symptoms (due to the circulation of the poison), noticed at the opening of this chapter; and, in addition, by the presence of cicatrices indurations, scars, and stains about the penis and groin.

2. *Their colour.*—It is described as copper coloured; in reality "*a reddish yellow brown*" (Wilson). It is dull red at first, and becomes coppery after awhile, and as the eruptions vanish, a dull red or yellowish dirty stain remains for a varying length of time. In the early stages of disease the tint may be violet, but this soon becomes replaced by the coppery hue.

3. *Their form*, which is peculiarly circular. This feature is not perhaps of much moment, *per se*, but in conjunction with other points is of some aid in a diagnostic sense. It may be destroyed or prevented by the confluence of other patches, but even then the typical form can be recognised in the component parts of the patch of disease. Syphilitic *scaly* eruptions are composed usually of small circular spots. Scales or squamæ are thin, oftentimes very fine, gray, and few in number; fewer and lighter than in the typical aspects of eruption. Crusts are thick, greenish, or black, and firmly adherent. Vesicles are flattish, and do not readily rupture. Ulceration is a common feature; the ulcerated surface is ashy gray, covered with a pultaceous substance, and bounded by sharply cut edges. Cicatrices are whitish and reticulated, or dull and brownish, leaving in their place on disappearance a yellowish stain. Fissuring is marked in the squamous forms.

4. *The Absence of Pain or Itching.*—With the exception of mucous tuber-

cles and some forms (moist) of infantile syphilis, syphilodermata are generally unaccompanied by heat or pruritis during their existence. In their tubercular forms, just prior to ulceration, a process of softening apparently depending upon a low kind of inflammation, goes on, and the tubercles may be painful and tender.

5. *Their Polymorphism.*—This is very characteristic of syphilitic disease. Several different kinds of eruption may coexist, and this is a rule of general applicability, if we except the squamous class of eruption. It is no unusual thing to see papules, pustules, and squamæ coexistent on the same syphilitic subject.—*Dr. Fox on Skin Diseases, p.* 267.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

89.—INDUCTION OF LABOUR BY DR. BARNES'S DILATING BAGS—ARTIFICIAL DELIVERY—SUCCESSFUL RESULT.

By William Elliott Porter, Esq., Lindfield, Sussex.

During the summer I was engaged to attend Mrs. ———, in her fourth confinement. She is a short, thick-set woman, aged thirty-four. Her first child was born naturally, alive, after a very tedious labour. The second was alive also, delivered with difficulty by the long forceps after the head had been at the brim of the pelvis several hours. This was my first attendance on her. In the third pregnancy, I recommended the induction of labour at the thirty-fourth or thirty-sixth week; but the patient would not submit to it, and she went her full time. I delivered her under chloroform by turning, after having failed with the long forceps. This child was dead.

I would here observe that in a case I attended with a Professional friend a few weeks ago we succeeded in delivering by turning, after having been unsuccessful with the long forceps. This child we lost also. In the fourth pregnancy I again proposed premature delivery; and it was consented to.

I obtained a set of three sizes of Dr. Barnes's bags: each had a stopcock and tube at one end, and on one side of the other end a finger socket. In consequence of the os uteri at this period of pregnancy being very high, I thought an introducer of some kind would be useful. I, therefore, had one made of a piece of lancewood; it was about as long and curved like a sound for the bladder. A sound would do; but wood looks less formidable than steel. I found, though not essential, it very much facilitated the introduction of the bags into the os uteri.

On August 30, at 2 p. m., I commenced by persuading one finger into the mouth of the womb. I then lubricated the smallest bag, and put the beak of the introducer into the finger socket, and laid the bag along the convexity of the introducer, where it was retained in its position by making slight traction on the tube. Thus it was passed along the palmar surface of the fore and middle fingers of the left hand, which were in contact with the os uteri, and with great ease into the uterus as far as was required. At 2.15 the first distinct expulsive effort took place, and shortly afterwards the bag was expelled; the second was then introduced, and came away about 3.15. The pains were now frequent. I waited, perhaps, half an hour, when they flagged a little. About 4 o'clock I introduced the third bag, which was not retained many minutes. The os was at this time as large as a crown piece, and the pains were quite established. They increased in strength till 7 o'clock, when dilatation being complete and the membranes protruding, I ruptured them. The head came to the inlet, and there remained. The pains were most severe and frequent for more than an hour; no progress was made; and the patient was getting exhausted. I gave chloroform, and delivered easily by the long forceps at 8.30. Child alive.

The recovery was unchecked. Both mother and child at this date (September 30) quite well.

The case above recorded affords additional evidence of the effectiveness of Dr. Barnes's dilating bags; and the Profession are, I think, much *indebted to him* for a very simple and safe means of inducing labour. It also

instances the advantage of turning over the long forceps; not that I for a moment repudiate the long forceps. I think where there is a possibility of delivering by them they should be used, because they give the child a greater chance of life than turning does, and are more easily applied than the child turned when the membranes are ruptured and the uterus in strong action.—*Medical Times and Gazette, Oct.* 22, 1864, *p.* 435.

90.—ON THE OPERATION OF TURNING IN CASES OF DISPROPORTION BETWEEN THE FŒTAL HEAD AND THE PELVIS.

By [Dr. Graily Hewitt, Physician to the British Lying-in Hospital, and Assistant Physician-Accoucheur to St. Mary's Hospital.

[In cases of pelvic narrowing, or of disproportion between the fœtal head and the pelvis, there are always a certain number of alternatives. First, to wait and see what nature can do without assistance; second, to apply the forceps; third, to perform the operation of turning; or fourth, to perform craniotomy. Respecting each of these Dr. Hewitt proceeds to make some remarks, and observes of the operation of turning:]

By the adoption of this alternative we avoid the extreme disadvantage of destroying the child necessarily involved in the performance of craniotomy. With respect to the value of the operation of turning in these cases of slight deformity, I would state in the outset that it is my belief that turning is destined to replace the operation of craniotomy in a very considerable number of instances. Turning is a comparatively old operation, and was formerly much resorted to, but it fell into disuse on the invention of the forceps. Dr. Simpson has revived the practice of turning, and set forth the advantages which will accrue from its adoption in a very admirable and valuable paper, the arguments and deductions contained in which, although attacked, have remained unanswered. More recently the matter has attracted the attention of other distinguished authorities. Dr. M'Clintock has brought before the Obstetrical Society of London a valuable contribution of facts bearing on this interesting question—viz., a series of cases in which the operation of craniotomy would have been performed had not turning been had recourse to. The result was, that in seventeen such cases nine children were extracted alive. Dr. M'Clintock comes to the conclusion that turning has been overrated; but it appears to me that the facts related by Dr. M'Clintock himself are hardly consistent with this conclusion, for these nine children which were extracted alive would otherwise have been destroyed. Dr. Barnes has published a series of four cases in which the value of turning is similarly scrutinised. In the first of these cases, after induction of premature labour, turning was employed, the child being saved; in the second, the pelvis was only three inches and a half, and the child, at full term, was extracted alive; in a third case there was a footling presentation, and the child was dead; in a fourth case the pelvis was three inches and a quarter, and the child was extracted alive. Professor Wilson, of Glasgow, has also published cases illustrative of the advantages of turning under the circumstances.

The mechanical advantage obtained by turning arises from the fact, pointed out by Dr. Simpson, that the diameter of the base of the fœtal skull, which has first to pass the brim of the pelvis after the operation of turning, is less than that of the upper part. Further, the transverse diameter, which is the one requiring to be lessened, is capable of being reduced to a greater extent when the head is made to enter the pelvis with the base lowest; the other parts of the head are also more readily moulded to the shape of the pelvis in such a way as to allow of its passage through the brim. Dr. M'Clintock does not, as I do, admit the validity of Dr. Simpson's arguments on this part of the subject. Another obvious advantage derived from turning is, that we have the body and lower extremities of the child to pull by, although I need hardly remark, we cannot make use of this advantage to its full extent without danger to the child.

In comparing turning with the high forceps operation, the first remark

which is to be made is that, on account of the contracted condition of the os uteri, often present in cases of pelvic deformity, it frequently happens that the forceps cannot be applied. In these cases the operation of turning is, however, generally practicable, except in some very few instances, and even then it is only a question of waiting a short time. In turning, carefully performed, we avoid the liability to laceration of the uterus, connected with the application of the forceps in the high operation. On the whole, taking into account the average dexterity of operators, there would seem to be less danger of doing harm by the hand in turning, than by the steel instrument, in the forceps manipulation. What would be the result, as regards the child, of the two operations, supposing there were equal skill and dexterity brought to their several applications? Without doubt, under such circumstances, the forceps operation is the better of the two, because in extraction after turning, the child is liable to suffer in a variety of ways. The pressure of the forceps does not appear to exert any injurious effect on the child's head; whereas, in extraction after turning, the head and neck are often so twisted in their passage through the pelvis, that life is destroyed. The pressure of the head on the funis is probably often the cause of death, but in many cases the death of the child is more probably dependent on injury to the neck. While, therefore, the delivery can be effected more easily, taking the run of cases, by turning in these cases of deformity, this mode of delivery is not so safe for the child as the use of the forceps. That the use of the forceps (the high operation) is attended with considerable difficulty in many cases I have already pointed out, and this it is which renders the acceptance of the other alternative necessary. Turning is an operation which I regard as comparatively safe, for the accidents and bad results which have been ascribed to it do not belong, I believe, to the operation itself. At least, I have never myself seen any untoward result therefrom.

I will now conclude with a few observations on the method of operating in cases where turning is resolved on. How long may you wait before you give up the idea of turning? I believe it can be performed very late. A considerable degree of impaction is no bar to the operation, for when the hand is introduced into the vagina the head generally recedes before the hand with a readiness which is surprising. Contraction of the uterus is a difficulty, but not an insuperable one. I do not consider chloroform to be absolutely necessary, for I have performed the operation under circumstances otherwise very unfavourable without giving it; but it is generally advisable to administer it. The bladder and rectum must be emptied. In all cases it is necessary to bring down both legs; I have seen great difficulty produced by neglect of this precaution. In some few cases you may fail, after turning, to bring the head through the pelvis. Craniotomy then becomes necessary; but the operation under these circumstances is not difficult. In extracting the child, it will be found that a lateral motion, together with extraction in the direction forwards, will facilitate the passage of the head. The great point is to delay the first part of the extraction, but to hurry the delivery of the head. The method of turning—combined internal and external version—recently introduced by my esteemed friend, Dr. Braxton Hicks, will doubtless prove of much service, especially in cases where we are called early to the patient, and where the membranes are unruptured. This method avoids necessity for introduction of the hand into the uterus. In cases, however, where the waters have escaped, the operation in question is less applicable, the fœtus having become more fixed, and the uterus more tightly contracted around it. By careful and steady introduction of the hand into the uterus, the operation of turning can be effected after the lapse of a very considerable time from the commencement of the impaction.—*Lancet, Aug.* 27, 1864, *p.* 232.

91.—ON SHOULDER PRESENTATIONS OF THE FŒTUS.

By Dr. John Christie, Lecturer on Midwifery and the Diseases of Women and Children, Aberdeen.

Shoulder presentations are, in certain circumstances, among the most dangerous occurrences which present themselves in midwifery. In a woman who has already borne children, in whom the pelvis is roomy, the membranes unruptured, and the os uteri dilated or dilatable, or where the membranes have only just given way, and the uterus has not yet contracted firmly round the fœtus, little or no difficulty is usually experienced in turning the child, and completing its extraction in a brief space of time. But it is far otherwise when the pelvis is narrow, or the liquor amnii has been for a long time discharged, and the uterus has, consequently, come to be closely contracted around the body of the fœtus, even when the os uteri is fully dilated, or, if not so, readily gives way to the dilating force of the operator's hand.

But, setting aside the more common presentations of the shoulder, and the consideration of the circumstances which may complicate them, and render their treatment difficult, I would direct attention to a class of cases which do not seem to have been much noticed by obstetricians. This is possibly, of late years more especially, due to the simplicity aimed at in describing shoulder presentations. Madame La Chapelle and Nægele were undoubtedly right in limiting malposition of the full-grown living fœtus to arm and shoulder presentations. The latter, also, judiciously divided these malpositions, according to the order of their occurrence, into two, viz., the *first position* of the shoulder, in which the back of the fœtus, compared with its anterior surface, is turned more than twice as often to the anterior wall of the abdomen of the mother; and the *second*, in which the same surface is turned towards the posterior wall. In either of these positions, we are taught that the head lies towards the one or the other iliac fossa, and the body in an oblique direction over to the other side, the presenting shoulder forming the most depending part of the fœtus, or that with which the examining finger necessarily first comes in contact.

In the great majority of instances this is true, but there are cases in which the shoulder is the presenting part, and yet the body of the fœtus does not lie in the transverse direction to anything like the degree almost universally assumed in regard to shoulder presentations. Smellie, in this, as in so many other points of practice, seems to have been far more accurately informed than either his contemporaries or successors. In his "Sett of Anatomical Tables," &c. (London, 1754), the thirty-second plate represents a shoulder presentation with the left arm prolapsed, and the uterus closely contracted around the body of the fœtus. In this state of matters, the uterus, with its contained fœtus, forms a roundish mass, having its greater diameter running from the right side somewhat obliquely up towards the left; and here we can, for the most part, move "the fœtus with the hand, so as to turn the head and shoulders to the upper part, and the breech and legs downwards."

But Smellie shows, farther, that the shoulder may present, and the fœtus nevertheless occupy a very different position in the uterus than that commonly ascribed to it. To understand this, let us see under what presentations nature is generally believed to be of herself able to effect the expulsion of the fœtus. Wherever, accordingly, the long axis of the fœtus corresponds to the long axis of the uterus, no matter whether it be the vertex, the face, the breech, or the feet which presents, experience has fully proved that labour, as a rule, terminates without any interference on the side of art with the view of removing obstacles to its due progress. True, Dr. Churchill, with others, still ranges face and breech presentations under the head of "natural labours," but when he tells us of the former, that "of late years it has been established as a rule, that assistance is unnecessary merely on account of the malposition," and of the latter, that "the less interference the better for the patient," we at once see his practice is far in advance of his logic. Dr. Murphy adopts the same arrangement of these presentations, and is so influenced by it, that, in the general case, after the labour has reached

a certain stage, he recommends a very "meddlesome midwifery." Dr. Tyler Smith shies the question of arrangement, but agrees generally in the views expounded by Dr. Murphy, and unmistakably, therefore, leans to the idea that pelvic are preternatural presentations, requiring in the end, as a rule, rather active interference.

But, notwithstanding the views of these able and distinguished men, it is now-a-days, on almost all hands, virtually, if not formally, admitted, that the correspondence of the long axis, both of the uterus and fœtus, is sufficient to constitute a natural position of the latter, whether it be its cranial or its pelvic end which is the presenting part; and that, wherever this happens to be the case, labour is practicable by the unaided powers of nature. Accordingly, in the so-called cases of transverse presentations, it is the aim of art to bring about this correspondence by means of certain manual proceedings, and thus reduce the process of labour as near as possible to one of its natural types.

But, even in the worst cases of transverse presentation, nature may herself overcome the obstacle to labour, as we see in the examples of spontaneous evolution and expulsion of the fœtus recorded by Denman and Douglass, and in those of spontaneous rectification and turning, by other observers. In all cases of shoulder presentation, says Braun, in order to make labour possible, the fœtus, whether mature or not, must undergo a change into the longitudinal position, or be subjected to dismemberment. These are effected in the following ways:—1. By rectification of the fœtal position; 2. By spontaneous turning; 3. By spontaneous evolution; 4. By the technical turning by the head, the breech, one or both feet, and one or both knees; and, 5. By the decapitation of the fœtus, or by embryotomy, or its evisceration. But neither of these modes include all that nature can do in bringing about a longitudinal position of the fœtus in shoulder presentation. Another means of effecting this has been almost wholly overlooked. Smellie has, indeed, admirably figured the state of things (Tab. xxxiv.) which may lead, in a shoulder presentation, to the advance of the fœtus through the pelvis with its long axis in a longitudinal direction, but he does not appear to have ever witnessed, or to have even suspected such a thing to be possible.

In speaking of the difficulties which may be encountered in presentation of the shoulder, and in contrasting the round form of the uterus, contracted close to the fœtus in the usual state of things, with another state of the organ, in which it resembles "a long sheath," with the child lying in it "lengthways" (vol. i. p. 334), the head being "reflected over the pubes to the right shoulder and back, and the feet and breech stretched up to the fundus" (Tab. xxxiv.), he describes a position of the fœtus in which, as will presently be shown, it may be extruded by the evolution, or the other processes alluded to by Braun, taking expelling powers alone, without anything resembling spontaneous place. For example, early in the morning of the 7th January, 1861, I was called to Mrs. A., 16 Long Acre, a primipara, who had been in labour from 5 o'clock a.m. of the preceding day. The os uteri was about two-thirds dilated, so that the presentation could readily be made out to be that of the cranium in the first position. In a few minutes the membranes burst, and soon after a very small but male living child was born. After separating it from the mother, when a vaginal examination was made for the purpose of ascertaining the position of the placenta, the membranes of a second fœtus were found projecting through the os uteri, but the presentation could not be made out because of its height above the inlet of the pelvis. In half an hour, the pains, which had subsided on the birth of the first child, returned very actively, and on an examination being made the presentation was found to be a shoulder one, an arm having already come down into the vagina. At this moment the infant already born, being very feeble, required my attention for a few minutes. During my absence from the bedside the membranes ruptured, with the escape of a large quantity of liquor amnii. On now making an examination, the left arm was found to be that prolapsed into the vagina, the corresponding shoulder being at the brim of *the* pelvis. The nature of the case being now quite clear, I passed my right

hand up along the arm till the trunk of the fœtus was reached, and while cautiously but firmly endeavouring to get at the feet, for the purpose of turning, I found the head of the fœtus to lie forwards over the horizontal ramus of the left os pubis, while the neck was so bent that it lay in contact at the same time, with the right shoulder. Moreover, the breech and feet were turned upwards to the fundus uteri; and the uterus contracted so closely and so powerfully round the body of the fœtus, in the form of a "long sheath," whenever I attempted to force my hand onwards, that I could not pass it so far up as to reach the feet, or even the flexure of a knee. Attributing this in some measure to the rather awkward position of my hand, I tried, however, before withdrawing it, to push up the presenting part, with the view of bringing the head down to the inlet of the pelvis; but the very powerful contractions, induced by every movement I made, rendered the attempt fruitless. Accordingly, I withdrew the right hand, for the purpose of substituting the left, but on introducing it into the vagina, I found the shoulder, neck, and head pushed down into the cavity of the pelvis, all compressed together, much in the form of the breech, and advancing so rapidly under the action of very powerful pains, that I at once determined not to proceed farther in the attempt I was about to make to turn. In a few minutes the presenting parts came down to the perineum, and in half an hour swept over it, and passed through the outlet of the pelvis, with the shoulder jammed hard against the arch of the pubes, until the upper part of the trunk, with the head turned over towards the right shoulder, and, as it were, embedded in the neighbouring parts, was born, when the rest of the body followed in the usual manner. The child was dead, and considering it was a twin, large, since it weighed fully 6 lbs.

I made a note of the case at the time, and on consulting the authorities I had at hand, I found no similar one recorded except by Smellie, with a reference to, and copy of his plate in the excellent work of Dr. Lee. And although, since the occurrence of this case, I have had several instances of shoulder presentation to deal with, there was nothing in them to remind one of its peculiarities, until, on the 22d September last, I was requested by my friend Dr. Ferguson of the Cove, to see, in consultation with him, a case of transverse presentation, in a fisherwoman at the Downies, a fishing village on the Kincardineshire coast, and who was in labour with her second child.

On my arrival at the Downies, at 3 o'clock a. m., Dr. Ferguson informed me that his patient had been in labour since late in the afternoon of the previous day, and that on his arrival, about 8 o'clock of the evening, he found the left shoulder, with the arm and navel cord prolapsed, presenting, the membranes having been ruptured some time before he reached the place. As the pains were not by any means severe or active, and the os uteri was soft and well dilated, he proceeded at once to turn the fœtus. In this, however, he was baffled; for although, in one or two attempts made with the right hand, he could reach one of the legs, he could not bring it down, in consequence of his hand becoming paralysed under the action of the uterus, excited by every attempt to gain his object. In this position of the case, he resolved to have a consultation, and, as already stated, I joined him about 3 o'clock of the morning in the management of it; and on making a vaginal examination, I found the state of things exactly as represented by Dr. Ferguson. The uterus, too, was still acting feebly in the absence of manual interference; and but for the alarm natural to her in such circumstances, the patient was otherwise in a very favourable state for an attempt to turn being still made. Acting under this conviction, I accordingly passed my right hand up along the fœtus, but by no justifiable force could I reach the feet, or any part, indeed, by which turning could be effected. This was due in part to the close contraction of the uterus round the fœtus in the form of "a long sheath," exactly as described by Smellie, in part to my hand becoming numbed under the strong contractions of the uterus whenever an attempt was made to carry it up towards the fundus uteri, and in part to the palmar aspect of the hand not being readily turned towards the anterior surface of the fœtus. For the latter reason I next tried the left hand, but,

although it could be passed with much greater ease up through the inlet of the pelvis, and the position of the fœtus could be far more readily ascertained, viz., that "the left shoulder, breast, and neck" were presenting, I did not consider myself justified in using such an amount of force as would have enabled me to reach a part by which the pelvic extremity of the child might be brought down, and so abandoned the attempt.

On consulting farther with Dr. Ferguson, I was inclined to recommend him to give a full dose of opium, and to wait for a few hours, when, chloroform having meantime been procured, a new attempt might be made to turn under its influence, and, in the event of that failing, recourse might be had to evisceration of the fœtus, or even immediately to proceed to that operation rather than repeat the attempts already made to change its position. My friend, however, thought that one more attempt should be made to turn, and I advised him to try the left hand, since I had been able to use my own with so much more freedom than the right. Accordingly, he introduced the left hand, and after some time spent in cautiously but perseveringly forcing his way towards the lower extremities of the fœtus, he succeeded in hooking the forefinger into the flexure of a knee, which afterwards proved to be the right. With great difficulty, in consequence of the cramped state of his hand, he contrived to retain his hold, and while I assisted by external manipulation, he brought the knee so far down that at last he got a blunt hook passed over the ham. Being quite exhausted, or rather I should say, his left hand being quite benumbed, he passed the hook to me, when I proceeded to extract in the following manner,—Guarding with the forefinger of the left hand the end of the hook, which otherwise would have come in contact with the brim of the pelvis about two inches to the right of the symphysis pubis, I made traction with the right hand in the direction of the axis of the inlet of the pelvis, Dr. Ferguson at the same time so manipulating the abdominal tumour externally as most efficiently to aid my effort to bring down the pelvic extremity of the fœtus. In a short time this was so far effected that the right foot passed through the vulva, when a loop of tape was thrown round the ankle, the blunt hook, of course, being removed whenever the leg came down so as to permit of the escape of the foot. The left hand being now set free, I coiled the end of the tape encircling the ankle round the fingers, and, with the right hand introduced into the vagina, pushed the shoulder upwards, while I at the same time made traction on the delivered leg, and thus in a few minutes the version was ultimately effected. The body and shoulders of the fœtus were next brought down, and after a brief delay, and with slight difficulty, the head was brought through the pelvis and delivered. The woman recovered without a bad symptom.

What is remarkable in this and the preceding case, is the longitudinal position of the fœtus, and the corresponding elongation of the uterus in the form of "a long sheath." In the first case, nature clearly overcame the difficulty by compressing the head and upper part of the fœtal trunk into a mass of such dimensions, as permitted it to be forced through the pelvis, in accordance with the mechanism which regulates the transmission through it of the fœtus under ordinary cranial or breech presentations. This was, no doubt, greatly facilitated by the dilatation of the parts of the mother, consequent on the birth of the first child. But, although it shows how fertile nature is sometimes in overcoming apparently insuperable obstacles to labour, such a result is no more to be calculated upon than is spontaneous evolution, and such like, in the ordinary run of so-called transverse presentations. In the second case, the process had gone no farther than merely bending the head over into contact with the shoulder; but had the expulsive pains been at all effective, I have no doubt the upper part of the fœtal trunk and head would have been pressed together as in the first case, and that they would have come to present at the inlet of the pelvis condensed, so to speak, into one mass, even if they had never come to be so modelled or reduced in dimensions by compression as to become engaged in or to pass through it.

Of the propriety of turning, in either the one or the other case, there will,

I presume, be no question, even although nature in the first stepped in so unexpectedly to the relief of the patient. To turn is but to do what she herself indicates, whether by rectification of the presentation, spontaneous evolution, and so on, or by the process described in the first case, and that is, the substitution of a directly longitudinal for the transverse, or, to speak more correctly, *oblique* position which obtains in shoulder presentations. In these cases, as Foster well observed, "considering the size of the fœtus and pelvis in a natural view, it is just to hold cross presentations absolutely impracticable by the force of labour alone." Still, it is of importance to understand how nature proceeds under the difficulties attending such cases, and to recognise, in the various modes by which she sometimes surmounts them, a single underlying principle, which seems to direct her in effecting the birth of the fœtus when it presents in labour, more or less transversely, at the inlet of the pelvis. Her whole energies here seem to be directed to ultimately bring the fœtus into a longitudinal position, and the obstetrician but imitates this when by art he brings either the cranial or the pelvic extremity of the fœtus to take the place of the presenting part in a cross presentation.—*Edinburgh Medical Journal, July*, 1864, *p.* 26.

92.—CASE OF RUPTURE OF THE UTERUS IN WHICH GASTROTOMY WAS SUCCESSFULLY PERFORMED.

By Dr. R. W. Crighton, Chapel-en-le-Frith.

Mrs. M., aged twenty-eight, had been three times delivered by the forceps previously to her present confinement. On all of these occasions the children had survived birth only a short time, on account of the strong compression that was necessarily applied to the head to effect delivery.

Having attended her during her last confinement, and ascertained from the great projection of the promontory of the sacrum, that it was impossible to deliver her of a living child at the full term of utero-gestation, I advised her in future to have premature labour brought on between the seventh and eighth months.

Her ordinary medical attendant, Mr. Davenport, of Hayfield, finding, during the early part of last winter, that she was again pregnant, had strongly urged on her the necessity of this proceeding; but, from some cause or another, his intentions were not carried out. On the 28th of January last, about 2 o'clock, A. M., I received a note from Mr. Davenport, requesting me to come as quickly as possible to Mrs. M., as he wished me to apply the long forceps. I arrived about 4 A. M., and found that rupture of the uterus had occurred about two hours previously. The patient was rather flushed, and had an expression of great pain and anxiety; pulse 120, of fair strength. As the least pressure caused great suffering, I could not ascertain the exact position of the child, but the unequal, nodulated surface of the abdomen, the complete recession of the presenting part, the free discharge of blood from the vagina, and the total cessation of labour pains, which had previously been very severe and constant, left no doubt as to the occurrence of rupture of the uterus, and the escape of the child into the cavity of the abdomen.

The patient stated that during the last severe pain she had, she felt something give way with a distinct snap.

Mr. Davenport informed me that she had been in strong labour since 10 P. M., on the previous day; and that, before my arrival, he had made two unsuccessful attempts to deliver, first by applying the forceps, and subsequently by turning. After the last measure had failed, he gave a large opiate, as the pains were very severe, and it was during one of these that the uterus gave way.

I explained to the patient and her relatives that the only prospect of saving her life was by extracting the child through an incision in the abdominal wall; but, notwithstanding my urging them as strongly as I could to have it

performed at once, I failed in obtaining their consent. I left the patient most reluctantly, and ordered her half a grain of muriate of morphia every two hours. About 2 p. m. I received a note from Mr. Davenport stating that the patient and her friends were anxious to have the operation performed. I arrived about 4 p. m., and found her in great agony, but with the pulse of wonderfully good strength.

After administering chloroform, Mr. Davenport introduced the catheter, and drew off about a pint of bloody urine. I now carefully examined the abdomen, and found the child lying with its head downwards, resting on the firmly contracted uterus. Before operating, I pointed out to Mr. Davenport the emphysematous condition of the cellular tissue over the hypogastrium, distinct crepitation being both felt by the hand, and heard through the stethoscope. I made an incision in the middle line from below the umbilicus to within an inch of the pubes through the skin and cellular tissue; the peritoneum was opened at the upper part, and the incision downwards completed with the knife resting on the forefinger of the left hand. About a pint and a half of dark-coloured bloody fluid escaped, and the child was at once seen lying in the position above mentioned. It was removed without any difficulty, and also the placenta, which was loose under it.

The uterus was seen to be well contracted, the line of rupture being almost directly transverse about the junction of the body with the cervix, and covered by a layer of coagulated blood.

Several large clots were removed, and some bloody fluid sponged out, but much disturbance of the viscera was carefully avoided. There were no signs of inflammation of the peritoneum except at one point in the left iliac fossa.

The edges of the wound were accurately brought together by twisted sutures on long acupressure needles passed through the peritoneum. Warm-water dressing on pledgets of lint were applied, and a bandage was passed round the abdomen.

The patient remained under the influence of chloroform for half an hour after the extraction of the child, and on awaking, expressed herself as feeling much relieved.

Pulse 112; no sickness; skin acting freely. To have one-sixth of a grain of morphia every second hour, and two minims of hydrocyanic acid with a little alkali.

No nutriment was allowed except occasionally a tablespoonful of cold toast-water, or arrowroot gruel. I saw her daily for the first twelve days, along with Mr. Davenport (whose intelligent care and attention were unremitting), and shall give a condensed report of the progress of the case, as I find it in my note-book.

29th Jan.—Has slept a great part of the night, and expresses herself as feeling very comfortable. No flushing; pulse 108, of good strength; has made an effort to vomit; about half a pint of dark-coloured urine was drawn off by the catheter; abdomen not much distended: moderate tenderness on pressure. Wound looks well; edges in correct apposition, except at one point below, where bloody fluid escapes on slight pressure. Fresh pledgets of lint were applied, and the same medicines and diet continued, with the addition of a solution of chlorate of potash for drink (gr. iv. to ℥i.), and small portions of ice occasionally to relieve thirst.

30th.—Has had pain during the night in the region of the liver and right shoulder, for which eight leeches were applied by Mr. Davenport with subsequent relief; urine twice drawn off since last visit, clearer, about half a pint each time; pulse 112; tongue covered with white fur; jaundiced tint of the skin and conjunctiva; vomited once during the night some brownish-coloured fluid; discharge from vagina less; wound looks well, seems uniting; abdomen not much distended, except at the epigastrium and right hypochondrium. Continue water-dressing to wound: to have an enema of warm water immediately, and another in three hours, if first does not act; turpentine epithems to be applied to the epigastrium and right hypochondrium. If the stomach is not irritable, ten minims of the muriated tincture of iron to be *substituted* for the hydrocyanic acid. Continue toast-water, &c.

31st.—Has passed a good night; slight sickness once, but only a small quantity vomited; urine thrice drawn off since last visit, lighter coloured and more copious; abdominal distension not much increased, except at epigastrium; jaundiced colour less marked; pain in right hypochondrium gone; tongue moist, cleaner; respiration nearly natural; pulse 108, of good strength; bowels have not acted, though she had an enema twice. Wound seems firmly united except at one point below, where the edges are slightly gaping. Four needles removed; no opening of line of incision; pads applied with warm-water dressing and a bandage. An enema to be given immediately, another in the evening, if first ineffectual; if still no evacuation, gr. v. hyd. c cretâ, with gr. x. bismuth. The enemata failing, the powder was given, and soon afterwards the bowels acted freely; great sickness, however, soon followed, bursting open completely the line of wound in its whole length. Slight hemorrhage occurred from the wound, but not from the peritoneal cavity. Mr. Davenport reintroduced several needles with twisted suture, and applied straps of adhesive plaster. The hydrocyanic acid was given more frequently.

1st Feb., 8 a. m.—Colour of skin much improved, jaundice nearly gone; has slept two hours since the sickness; tongue cleaner and moist; complains of feeling great distension of the abdomen, which, however, is reported as being less than during the night; pulse, 120, firm; urine drawn off several times since last visit, reported copious; plasters partially removed, so as to examine the wound, which is seen to be without any gaping of its edges, but not so accurately in apposition as before the vomiting; has retained the beef-tea since 11 p. m. To have tincture of iodine painted over the epigastrium and right hypochondrium, turpentine to be reapplied if required. Continue prussic acid, with ice, &c.; beef-tea in small quantities to be given by the mouth, and, every four hours, a teacupful, with ten minims of the muriated tincture of iron, by the rectum.

2nd.—Has passed an uneasy night; vomiting again occurred about midnight, with pain in the region of the liver, for which four leeches were applied, with relief; has not vomited since; bowels acted well during the night; has taken about a pint and a half of beef-tea since yesterday—the greater part by the rectum; prefers it now by the mouth; tongue moist, and nearly clean; countenance of natural colour; expression good; pulse 130, of fair strength. Straps of adhesive plaster removed, and fresh ones applied; wound has a tendency to open superficially, but not in its whole depth, except at one point below, from which dark-coloured fluid escapes; abdominal distension less, except at epigastrium; tenderness on pressure less; urine reported paler coloured, and more copious, drawn off thrice since yesterday. Two long needles substituted for two shorter ones, and twisted suture applied. Continue warm-water dressing; painting with tincture of iodine, and other medicines. A pint and a half of beef-tea to be given during the twenty-four hours.

3rd.—Bowels have acted five or six times during the night; an opiate was given by Mr. D. at 6 a. m.; since then the diarrhœa has ceased. Tongue moist and clean; jaundice quite gone; urine copious and clear, drawn off thrice; abdomen much reduced in size, except at epigastrium; pressure well borne; pulse 112. Wound dressed as before; healing at upper and lower points; edges separated for about three inches between; free discharge of dark-coloured fluid from the cavity of the peritoneum, with strong fœtor. Ordered two teaspoonfuls of brandy every two or three hours, with one quart of beef-tea per day. A solution of carbolic acid to be sprinkled on the bed.

4th.—Has passed a good night; no action of the bowels, except the frequent passage of flatus per anum; pulse, 112, soft; wound dressed as before; no tendency to unite in the middle; all the needles removed; discharge free and very offensive. Continue the application of the iodine, and the other medicines, &c.

5th.—Has had a restless night, and slept but little, although two opiates were given; has had intense pain in the epigastrium and left hypochondrium, which was relieved by hot fomentations; at the same time experienced great

pain and difficulty in swallowing; pulse rose in the night to 144, and is still 140; urine drawn off as usual; no action of bowels except the free passage of flatus. Has taken little by the mouth since yesterday, but had an enema of beef-tea, with brandy. The cardiac region was carefully examined, to ascertain whether the dysphagia might be occasioned by effusion into the pericardium, but no signs of fluid there were discovered. To have an enema of beef-tea and brandy every three or four hours. Wound dressed as before, is now discharging pus freely; a cloth wet with a solution of carbolic acid to be applied over the pubes, and changed frequently; this seemed to destroy the fœtor very thoroughly.

6th.—Has had a good night; general symptoms improved; pulse 120; wound, healing above and below, still gapes in the middle; can now take the iron every three hours, and swallows better. Continue the beef-tea and brandy, with a little milk occasionally.

7th.—Was much troubled during the night with pain in the left hypochondrium, which is more distended than yesterday; bowels acted freely this morning after an enema; passes flatus freely per anum; complains much of a sore over the sacrum, which has been dressed regularly for some days past with calamine cerate; appears more irritable and desponding than she has done before; pulse 120; wound discharging healthy-looking pus, is contracting. Continue iron, &c.

8th.—Vomited once since yesterday, after taking a dose of muriated tincture of iron, which was, therefore, discontinued for a time; had an opiate twice during the night and slept well; expression much improved; pulse 120, of better strength; distension of epigastrium and left hypochondrium less, and pain there much less acute; pressure over abdomen generally well borne. While dressing the wound, its edges were rather widely separated about the middle, and what was considered the lower edge of the transverse colon was distinctly seen at its upper part; no tendency to protrusion of any part of intestinal canal. Straps of plaster were reapplied more firmly than before, and the edges brought into exact apposition with dry lint and a bandage round the abdomen. To have an occasional dose of hydrocyanic acid and another enema; the iron to be tried again in doses of five minims; beef-tea, &c., as before. After the dressing she expressed herself as feeling very comfortable, and for the first time said that she had some hope of recovery.

9th.—Has had a good night; no vomiting since yesterday, though she has felt sick occasionally. Pulse 120: tongue drier, slightly brown; distension of abdomen gradually diminishing. Wound united for upwards of two inches; discharges healthy pus; still a tendency to open widely at upper part; dressed as before, and calamine cerate applied to the pubes, as there was some erythema from the discharge. Has not taken the iron since yesterday; to have instead, the prussic acid, with a little soda, in infusion of calumba; beef-tea and brandy as before, and bread and milk occasionally, of which she is very fond.

11th.—Appearance much improved since last visit; pulse 112, of good strength; slept during past night from 11 p. m. till 5 a. m.; no vomiting; bowels have acted several times; has passed water by the natural efforts; appetite better; tongue clean and moist; distension of abdomen less; wound contracting, though still open to the extent of two inches; erythema over pubes less. Plasters, &c., reapplied; has taken the iron regularly yesterday and to-day; to have at her own request, a small mutton-chop.

18th.—Has improved steadily since the date of last report; sleeps well; tongue clean and moist; bowels opened by occasional enemata; pulse about 100, of good strength; size of abdomen much reduced, no tenderness. Edges of wound well together under the plaster; when this removed but little tendency to gape, except at the upper part; is evidently healing kindly; a little healthy pus at the lower angle. Takes half a grain of quinine several times a day; has had for some days beef-tea and mutton-chop, with a little fish occasionally. After dressing wound she felt so well as to express a desire to *get up soon*; can now pass water regularly without the catheter.

22nd.—Continues to improve; pulse 98; bowels opened by enemata; appetite has failed considerably for past two or three days notwithstanding the administration of half a grain of quinine every four hours; abdomen now quite flat, no tenderness; on removing plasters, the wound found quite united; a free discharge of pus from the vagina replaces that from the wound; no fœtor. Ordered the same dose of quinine, dissolved in ten minims of the muriated tincture of iron; to have porter frequently, up to a pint a-day, and as much animal food as the stomach will digest.

From this date she continued steadily to improve till 3rd March, when she had a rigour, with pain in the left iliac fossa; but after free action of the skin, and the reapplication of the iodine, she began to improve again in a few days.

On 24th of March the pulse rose to 120, and slight jaundice appeared, with increased dulness in the right hypochondrium. Tincture of iodine was painted over the dull space, and nitro-muriatic acid given internally, and in the course of two or three days she was so much better as to be able to be lifted out of bed to the sofa for several hours at a time.

During the first week of April she was strong enough to sit up daily, and was so well on my visit to her on the 4th of that month, that I discontinued my attendance. She mentioned that the menses had appeared a day or two previously.

On 2d June she walked from Hayfield to my house, a distance of four and a-half miles (returning again the same evening on foot), and assured me that she had not been in better health for years.

Remarks.—The carefully compiled statistics of Dr. Trask of New York, give great encouragement in undertaking the apparently hopeless operation of gastrotomy in cases of rupture of the uterus; yet, since the date of the publication of these (1856), I cannot find any successful case recorded as having occurred in Great Britain. I have, during the past thirteen years, seen only one case in addition to that recorded above. In that case the practitioner in attendance was not aware that rupture had occurred, and did not fully sanction my proposal to perform gastrotomy. The patient was abandoned and died undelivered about thirty-six hours after the occurrence of rupture. She was a primipara, and apparently free from pelvic deformity. Ergot had been given freely during the progress of the labour.

In Mrs. M.'s case several points deserve, I think, especial notice; the long interval that occurred between the rupture and the extraction of the child—fourteen hours; the recovery from acute jaundice occurring in the circumstances; and the apparently good effects of the application of the tincture of iodine externally in causing absorption of the products of inflammation affecting the serious covering of the different abdominal viscera.—*Edinburgh Medical Journal, Aug.* 1864, *p.* 133.

93.—ON THE SCIENTIFIC TREATMENT OF POST-PARTUM HEMORRHAGE.

By Dr. J. Lumley Earle, Obstetric Surgeon to the Queen's Hospital, Birmingham.

In the employment of cold, we should bear in mind the following facts—that cold, applied for too lengthened a period, acts as a depressant and loses its effect; that the colder the water, the greater the shock, and the more powerful is its effect in inducing permanent uterine contraction. We learn from them that, in bad cases, the continued application of cold requires care lest it increase the exhaustion; that it is most important to have the water as cold as it can be obtained, as the effect depends upon the coldness, and not upon the quantity of water used. Water, fresh from the pump, is generally sufficient; if we can obtain ice easily so much the better. Dr. Tyler Smith recommends in some instances the application of cold and warm water alternately. I should say it would be most useful in cases where cold water had been applied for some time, and was losing its efficacy.

Ergot.—When good in quality, ergot is a most valuable remedy; it arrests the flooding chiefly by exciting contraction of the uterus. One fault in the drug is, that it sometimes fails in producing its specific effect. The failure may be due to the ergot being inert, or to some peculiarity of the patient, rendering her insusceptible to its action. It very rarely fails from an idiosyncrasy, but from the absence of the active principle (whatever that may be) of the ergot. There is great difficulty, first, in getting good ergot, and next in keeping it good. Prior to the purchasing of the drug, we should examine its quality. The grains of ergot are from half an inch to an inch and a half in length, of a dark purple or black exterior, and ribbed in the longitudinal direction. On its surface, longitudinal and transverse fissures are generally present. On breaking a grain across, the interior is either of a white colour tinged with purple, or of a dirty yellow hue. The difference of colour depends probably upon the age of the grain. Ergot has also a very strong peculiar odour, irritating to the nose. The best indications of good ergot are the large size of the grains, both in length and breadth, and the strong oppressive odour. Good powdered ergot is known by its not being as fine as dust, and by its possessing the odour in a strong degree. The insect which feeds upon it, and destroys it, is a species of acarus, and may be seen sometimes by the naked eye, although its form cannot be defined without the aid of a microscope. These creatures will, in a very short time, reduce coarsely-powdered ergot to a fine dust, and render it perfectly inert. The best way to prevent ergot from spoiling, is to keep it in the form of grain in a glass stoppered bottle, with a piece of camphor about the size of a pigeon's egg thrust into the centre of the mass. If a cork instead of a glass stopper be used, the acari will soon riddle it through, and allow free ingress of air. Not more than an ounce or two of ergot ought to be crushed for use, as it rapidly deteriorates in the powdered state. It is given in the form of infusion. One drachm, or a good tea-spoonful, is placed in a tea-cup of *boiling* water, and allowed to stand ten minutes, when half the quantity may be administered, and the other half in twenty minutes or half an hour after, if the desired result should not have been produced. Before taken, the infusion should be stirred up, that the patient may swallow the ergot grounds as well as the fluid. Besides the infusion, there are other fluid preparations, and an extract called ergotine. I have used the latter, and one of the former, and the results have prejudiced me against their use. In some cases, uterine contraction followed apparently their employment; while in others no effect at all was produced. I could not depend upon them as I could upon the infusion of good ergot. One great objection to them is, that we cannot be certain that they are made from ergot of good quality. I have lately heard the extractum ergotæ liquidum B. P., very highly spoken of, and I intend to give it a trial. The dose is one tea-spoonful. A trustworthy fluid preparation of ergot would be of manifest advantage. The active principle would be preserved from the ravages of the acari, and it would be ready for immediate use—a oint of considerable importance in an urgency like flooding. Although ergot is about one of the most valuable remedies we possess in the treatment of post-partum hemorrhage, there are cases in which no good will be attained by its administration, and others in which it will do harm. I shall particularise them in a subsequent communication.

The introduction of the Hand into the Uterine Cavity.—This is one of the means of arresting hemorrhage which requires care and judgment in its use. I do not agree with the opinion of many writers, that it should be put into a practice only as "a last resource." I have in several cases made use of it as a first remedy, and in a few moments, by so doing, have placed patients from a state of imminent danger into that of comparative safety. The ease or difficulty with which the hand may be passed into the uterus, depends very much upon the size of the hand. We know what a slight difference in the size of an infant's head, or of the pelvic cavity will make the labour easy or difficult; and just so in the introduction of the hand. If a medical man *has a very* large hand, and his fingers are all thumbs, he will find great diffi-

culty in passing his hand into the uterus, and will cause his patient great suffering; but if his hand be small, it will pass easily, and give very little pain. I have noticed, and it is what we would expect, that the men who deprecate the procedure are generally those who possess big hands. The effect upon the patient will also depend a great deal on the size of the hand. The smaller it is, the easier it will pass, the less pain it will cause, and the less shock and exhaustion will ensue. Let it not be thought, from the above remarks, that I look upon the introduction of the hand as a light matter, to be undertaken indiscriminately, without any reference to the case before us, for I do not; I never pass my hand into the uterus unless I have a good reason for it: but if, from the nature of the case, I feel satisfied that that is the correct proceeding, I do not hesitate, and go on trying a number of other remedies, with the hope that the introduction of the hand may be avoided. Such hesitation, in some cases, would prove fatal. Dr. Tyler Smith says:—"I believe, in the present day, more mischief is caused from a kind of fear of the uterus, and of interfering with its natural action, than from bold and intelligent efforts to guide and control it."

The introduction of the hand may be necessary for the application of internal pressure and irritation, or for the removal of substances which keep up the flooding by distending the cavity of the uterus, and preventing its due contraction. Before it is undertaken, brandy should be given to the patient, and the nurse told to continue its administration from the time that the hand commences to pass through the vulva until it is removed. In flooding cases, this precaution should never be omitted. It averts the faintness and shock which otherwise might ensue, and it also assists in drawing away the patient's attention from what is being done. Since I have taken the precaution of having brandy given *during* the operation, I have not observed the slightest depression or fainting result.

The hand may be introduced as follows:—If the patient is lying on her left side, the left arm and hand will be more readily passed than the right. They should be well greased, and the fingers and thumbs so packed as to be made to occupy as small a space as possible. The first and fourth fingers should be placed in front of the second and third, and the tip of the thumb at the base of the cleft between the third and fourth finger. The usual plan of placing the tips of the fingers and thumb together is not so good, as it takes up more room. The hand should be then passed gently and slowly, feeling the way as one goes on through the vulva and vagina into the uterus. The hand, as it passes through the vulva, generally produces more or less pain on account of the soft parts being pinched against the descending rami of the pubis. The mere presence of the hand in the vagina or uterus does not give rise to much pain. It frightens more than it hurts the patient. The hand should never be removed until it has effected the purpose for which it was passed; for instance, in adherent placenta, to pass the hand into the uterus, and then to take it away again without bringing away the placenta, on account of the patient's or friend's entreaties, would show a great want of presence of mind, and it would be acting cruelly instead of kindly to the patient, because the introduction of the hand would have to be repeated. I shall postpone the description of what has to be done with the hand while in the cavity of the uterus until I come to consider the causes for the removal of which it has to be passed.

In a very bad case, where the patient is exceedingly low, it may be a question as to whether the hand should be passed into the uterus before the patient has somewhat rallied. The cases are quite exceptional in which it would be well to wait. If a great loss of blood is going on, we cannot expect that the patient's condition will improve, but just the reverse; and if the hand has to be passed at all, the sooner it is done the better. During the operation, in these cases of great depression, the brandy must be poured down the patient's throat undiluted with water. The introduction of the hand, and especially whatever it has to do in the uterus, should be done gently and deliberately.—*Medical Circular, June* 8, 1864, *p.* 375.

94.—ON UTERINE HEMORRHAGE

By I. Baker Brown, Esq., Senior Surgeon to the London Surgical Home.

[This paper is an abstract of a clinical lecture by Mr. Brown, and embraces the subjects of hemorrhage arising from polypus of the womb and from fibrous tumours.]

A polypus may exist at the mouth of the uterus, in the cervix, or within the body. In construction it may be either vascular or fibrous. It may be so diminutive in size that the points of a pair of forceps will not hold it, or if sought by the finger, it is scarcely to be discovered by touch. It may be so small, indeed, that it would seem almost impossible that a structural change so slight in appearance should give rise to formidable bleeding. But it is a fact that these diminutive polypi serve to keep the lining membrane of the uterus in a constant state of irritation, liable at any moment to give rise to hemorrhage. These very small bodies can often be scooped away, when they cannot be laid hold of and ligatured. An ordinary scraper something like that which Prof. Simpson uses, or a long nail fastened to the finger, giving it a couple of inches or so more length, according to Sir Charles Locock's ingenious suggestion, may be had recourse to. The polypus is readily detached, and the moment it is separated hemorrhage ceases. The polygus may be of a larger kind; and I may here remark that the true distinction between a polypus and a fibrous tumour consists in this, that a polypus has a neck to it and a fibrous tumour has not. The true polypus of the womb is a fibroid growth, precisely of the same structure pathologically as the fibrous tumour. A polypus is much more easily removed than a fibrous tumour on account of its pedicle. A polypus is generally removed by the scissors or knife, by ligature, or by pulling down the growth and twisting it off. Some advocate twisting, some tying, and some cutting. Sir Charles Locock tells me that throughout a long practice he has never seen any hemorrhage arise from cutting with scissors; still it is a fact that cases are on record where dangerous hemorrhage has followed removal of a polypus by the scissors. The moment a polypus is removed the uterus contracts; and if hemorrhage take place from the cut pedicle, serious consequences may result from the effused blood being retained in the womb. Again, if the polypus be ligatured, and allowed either to remain in the uterus or to hang out of its mouth, a series of the gravest consequences are apt to occur. The polypus quickly dies and breaks up, giving rise to a peculiarly offensive discharge, which sickens both patient and attendants, and renders the room where the patient lies unbearable. In cases under my care in St. Mary's Hospital where I removed the polypus by ligature, twice I was obliged, from the poisoned atmosphere of the ward, to cease operations there. In one instance two of the house-surgeons became seriously ill from the stench. I therefore adopted, in place of the ligature alone, the following plan, which may be recommended to the timid surgeon, who fears to use the scissors or knife simply. I tie the polypus, either with the fingers or a Gooch's canula, as high up as possible. Then I cut off the growth anterior to the ligature. By this plan the stump alone is left to slough away. The use of the écraseur is advocated by some surgeons in these cases. Much as I value the écraseur, I do not think it suited to polypoid growths of the womb. The operation with this instrument is long and tedious, and offers no advantages which outweigh the brevity and simplicity of removal with the scissors or knife, or the combination of ligature and knife or scissors, as I have suggested. I must omit details of cases of polypus, lest these remarks should extend beyond reasonable limits.

I would now direct your attention to fibrous tumour—a most frequent source of uterine hemorrhage.

I shall confine my observations to the treatment of these morbid growths by breaking up their structure. This operation is performed by first dividing the mouth and neck of the uterus up to the inner mouth by the hysterotome; and then, when the cut surfaces have healed, gouging the tumour *so as to break* the capsule. After I had performed this operation in several

cases, I noticed that hemorrhage invariably ceased after the incision of the mouth and neck of the uterus. It occurred to me that there must be something more in this sequence than immediately met the eye. I instituted a series of experiments upon the influence of incision of the os and cervix uteri in checking uterine hemorrhage, and discovered that by means of this operation the bleeding occasioned by fibrous growths in the womb could be readily and effectively checked. About the same time M. Nélaton, of Paris, and Dr. M'Clintock, of Dublin, each independently of the other and myself, make the same discovery.

I adopt the following practice in incising the mouth and neck of the uterus:—I press the perineum down with the bent speculum, which I term Boseman's (although Dr. Marion Sims objects to the instrument being so designated). Then I seize the superior lip of the os uteri, and hold it firm, an assistant steadying the speculum. In this way I obtain a perfect view of the os and cervix. Having determined to what extent I will incise, I introduce Simpson's hysterotome within the os and cervix, and cut freely first on one side, then on the other. If the uterus be not steadied by the forceps, it will recede before the hysterotome, and it will be difficult to make the incisions of the requisite depth and with sufficient freedom. Having made incisions from the outer to the inner os, including the whole length of the cervix, I next plug the cervix and the vagina most thoroughly and firmly with lint soaked in sweet oil, fixing the whole with cotton-wool, so that hemorrhage is prevented and air entirely excluded from the cut surfaces and the uterus. Upon the care with which the plugging is effected will depend the subsequent favourable progress of the case. In the last case of uterine hemorrhage treated in the Surgical Home by incision, the vagina was very large, and, as it were, pouched. I did not plug it sufficiently with lint or cotton-wool, and, for the first time in this institution, severe hemorrhage occurred, and subsequently pelvic abscess. This is a lesson not to be lightly forgotten. The impunity with which I incise the os and cervix uteri so frequently as I do, arises from the fact that I am extremely careful to plug well afterwards.

The following cases will illustrate the happy results of incising the mouth and neck of the uterus:—

A., suffering from fibrous tumour of the uterus, giving rise to severe hemorrhage. The patient was forty-one years of age, married, and had four children. She was admitted into the Home on the 24th of July last. About a year and a half before admission she fell violently, and suffered from retention of urine for some time. Ever since the accident she has been subject to constant and severe attacks of flooding. I first used sponge tents, but without giving relief. Frequently a sponge tent will check the hemorrhage. On the 10th of September I freely incised the os and cervix uteri in the manner which I have described. Three months afterwards the report states that there had been no return of flooding.

B., aged thirty years, married, has had six children and miscarried five times. For several years she has suffered from excessive menstruation, and she has been laid up some time in consequence of her extreme weakness. She acknowledges constant habits of delection, and is subject to frequent attacks of hysteria. The habits of delection complicated the case, for I knew well that if I did not remove the clitoris, to cure these habits, at the same time that I incised the os and cervix uteri, temporary relief would alone be given. I removed therefore the clitoris, and incised the os and cervix. The patient steadily improved from the time of the operation. The hemorrhage ceased, she regained her flesh and strength, and within twelve months after leaving the Home she gave birth to a child.

C., aged forty-seven years; twelve years a widow. Has two children, the youngest aged seventeen years. Suffers from a large fibrous tumour within the uterus, and a small bean-like polypus at the os, giving rise to flooding at the catamenial periods. I removed the small polypus, and divided the os and cervix freely on each side. A month afterwards the menses appeared, normal in quantity, and lasting three days. Before the operation they lasted

seven or eight days, and were excessive. She left the Home wonderfully improved in health, and without any return of the hemorrhage.

D., aged thirty-five years; married thirteen months; no children. She noticed an increased size of the body during the last nine months. Every fortnight subject to what she describes as "fearful flooding." When she was admitted into the Home she was highly anæmic. On examination, the cavity of the uterus was found to be occupied with a large fibrous tumour. I incised the os and cervix freely. The tumour gradually diminished in size; menstruation became regular and normal in quantity; and the patient subsequently gained a condition of health which she had not experienced for fifteen years, and became equal to all her household duties.

H. R., aged thirty-eight; unmarried. The abdomen had been enlarging gradually during six months; and flooding had been almost constant. The patient was exceedingly weak and highly anæmic. On examination, a large intra-uterine fibrous tumour, the size of the head of a nine months' child, was discovered. In this case the os and cervix were incised, and, after the operation, the hemorrhage stopped. A month afterwards I broke up the tumour. The patient recovered completely.

My note-book contains reports of at least fifty such cases. I shall add, however, but one case more to those which I have already briefly mentioned —a case of recurrent fibroid tumour. This form of disease is very rare, and its successful treatment is still rarer.

The patient, a needlewoman, was thirty-three years of age, and single. From the age of thirteen she had menstruated, generally profusely, and on each occasion from seven to eight days. For two years she had been rarely free from flooding, had suffered much pain, and become so weak that she was almost constantly laid up. For nine months she had not passed a day without some loss of blood. She was under the care of Dr. Holt Dunn, who recommended her to place herself under my care for the sake of being operated on; but she first consulted Dr. Robert Lee, who expressed an opinion highly adverse to operative interference. In the end, however, she was admitted as a patient into the Surgical Home on January 15th, 1861. On examination, I found a good-sized intra-uterine fibrous tumour. The patient was in a most miserable condition, almost bloodless, constantly flooding, and with a face that looked as if she had malignant disease. On the 30th January I incised the os and cervix, and she had no hemorrhage for some time after the operation. When examined on the 18th February, the tumour was found to be decidedly diminished in size, and a small portion was protruding at the os. On the 20th February, under chloroform, I removed with a pair of curved scissors a piece of the tumour as big as a bantam's egg. On the 25th the patient went to her own home, very much improved in general health. The flooding had at this time entirely ceased, but there was a great deal of offensive discharge. Shortly afterwards there was a slight return of the hemorrhage, and on March 4th I removed with an écraseur another large piece of the tumour, the size of a small orange, injected the uterus with tincture of perchloride of iron, and gave tincture of ergot. The injection was repeated every two or three days. On the 27th March I removed another large portion of the tumour by means of a knife and curved scissors. After the removal of the pieces, the uterus would contract and the flooding stop. Then in a few weeks the tumour grew again, opening the os and cervix, and protruding into the vagina; then the flooding would come on once more. On this last occasion the tumour had opened the os completely, and I was able for the first time to grasp and pull it down. I then found that the mass grew exactly in the centre of the fundus. I had never been able fully to "get at it" before. I excised as completely as possible, and I am not sure that I did not remove some of the fibres of the uterus itself. I then applied the tincture of perchloride of iron, and plugged the whole of the uterus and the vagina. After the operation the patient was treated medically with bichloride of mercury and bark. The flooding did not return, and she gradually, though slowly, recovered strength. At the present time, one year *and nine months* after the last operation, she is quite well; and Dr. Dunn,

who sees her constantly, says that she is stout and strong, and able to return to her occupation as a needlewoman.

It is rare to find a case of recurrent fibroid tumour end so satisfactorily as in this case, and it is important as showing what may be done under the greatest difficulties by perseverance. Drs. Savage and Routh have insisted on the value of tincture of perchloride of iron applied locally in the treatment of fibroid tumours, and I think that this case corroborates their opinion.

I would now direct your attention to flooding arising from vascular growths. In these cases the os uteri is found to be patulous, the posterior lip elongated and hypertrophied, and the flooding constant. If the uterus is examined with the speculum, a seemingly ulcerated surface may be seen. But in these cases it does not do to depend upon the eye alone in seeking to determine the nature of the morbid change. The finger, as a rule, affords more accurate information; for the sensation given to the touch by a vascular growth is so peculiar that once known it cannot well be forgotten. The altered part feels spongy, somewhat rough, and it has a certain degree of elasticity. The apparent structural change is so slight that some practitioners hesitate to accept it as a source of serious hemorrhage. The results of treatment, however, remove all reasonable doubt on this head.

H. B., aged twenty-three, married, had had five children, and had miscarried thrice. She was admitted into the Surgical Home on the 6th May, 1863. Her last confinement took place two years previously, and ever since she had been subject to attacks of flooding, coming on at the catamenial period, lasting many days, and producing great weakness. On examination, there was found general hypertrophy of the lips of the uterus, and a vascular growth on the anterior lip. The case was treated in the following manner:—The os and cervix up to, but not through, the inner os were divided; then the anterior lip was seized with a pair of strong forceps, and, with curved scissors, the entire vascular growth was cut out. The os and vagina were next plugged with lint and sweet oil. The patient recovered perfectly. The catamenia became normal; and there was no return of flooding after the operation. In cases of this character it is very necessary, in order to secure a successful result, that the morbid growth should be entirely extirpated.

The ovarian irritation which gives rise to flooding is usually brought about by habits of delection.

M. A., aged twenty-eight, was sent to the Surgical Home in June of the present year. She had suffered from flooding for three years, and during that period she had rarely passed a day without more or less loss of blood. She had been a patient at different times in several metropolitan hospitals, but she had received no permanent benefit from the treatment which had been adopted. She was supposed to be suffering from an undetected fibroid growth. When she came under my care she was anæmic in the highest degree. From her aspect I was convinced at the first interview that she was addicted to habits of inordinate delection, and she freely confessed, on being questioned, that such was the case, and said that it was impossible to give up the habit, as she suffered from so much irritation in the vulva. On an occular examination she indicated the clitoris as the centre of irritation. No uterine disease could be detected, but the left ovary was large and indurated. I concluded that unless the habit of delection were put an end to, no relief would be given to the ovarian irritation and the flooding. I therefore determined to excise the clitoris, and at the same time to incise the os and cervix uteri. These operations were performed, and from that time the flooding never recurred. A week after the operations the patient menstruated, the courses extending over three days only. A month from this time the patient again menstruated normally, no blood having been lost in the interval. She now began rapidly to recover colour, flesh, and strength, and she left the hospital perfectly well.

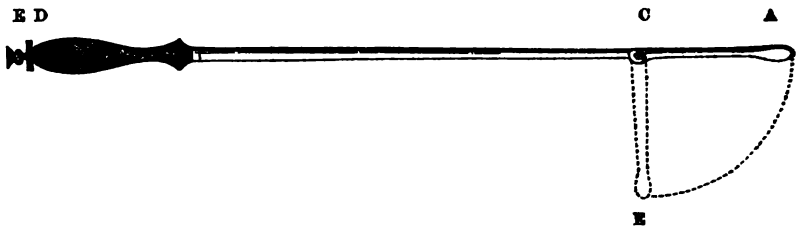
I have not yet mentioned the after-treatment of the class of cases which have been under consideration. Where there is hemorrhage, without any anæmia, it is best to keep the patient on a milk diet, avoiding stimulants. But when the patient is anæmic, a meat diet will be required, with wine.

The wine at first should be given in small quantities—half an ounce three or four times a day, the amount being gradually increased as the patient improves. Absolute rest for many, usually twenty-one days, is essential. I do not allow the patients to leave their bed, or to sit up for any purpose; and, for the first few days, I have even the urine drawn off. I do not, also, allow the bowels to act, so that there may be no disturbance of the hemorrhoidal vessels, and no excitement of the sacral nerves—nothing, in short, to agitate the parts which have been operated upon. Another important point is attention to the skin. In every case operated upon in this institution, the patient is sponged from head to foot every day. Upon the normal action of the skin the rapidity and certainty of the patient's recovery largely depend. By attention to these points in the after-treatment the recovery of the patient is greatly promoted.—*Lancet, July 2 and 9, 1864, pp. 5, 34.*

95.—ON A UTERINE SOUND WITH MOVABLE HEAD.

By Dr. J. Lumley Earle, Obstetric Surgeon to the Queen's Hospital, Birmingham.

By referring to the woodcut, the working of the instrument will be readily understood. The portion of the sound, from A to C, is 2½ inches in length, the ordinary length of the normal uterus. It is set in motion by an internal



rod, one end of which is connected with the joint at C, the other being shown at E. The head of the sound may be made to move to any point along the dotted lines from A to B by turning the screw at D from right to left.

The instrument was devised chiefly to obviate the constant bending which is necessary in the immovable sound to make it tally with the varying obliquity of the uterus in different individuals. It possesses the advantage of being able to be set in motion while in the vagina or uterus. It may be passed perfectly straight to the os uteri, and then, gradually as it passes along the canal of the uterus, may be made to take the angle suitable to the case before us.

By using the movable sound at certain intervals for some time it may prove beneficial in curing some cases of anteversion or retroversion. Take an instance of anteverted uterus, in which the movable part of the sound lies at right angles, C to B, to the shaft of the instrument. By reversing the action of the screw at D, turning it from left to right, the uterus may be made to assume the perpendicular position; and, if necessary, even may be retroverted by turning the handle of the instrument round, and setting the bulbous portion again in motion.

There are other cases in which a movable uterine sound might prove useful. It would probably remove a great deal of the difficulty generally experienced in bringing on labour in certain cases of retroversion of the pregnant uterus, as it can be passed to the os in a straight position, and made to take a suitable angle gradually as it passes through the os and cervix.

The instrument may be procured from Mr. T. P. Salt, Surgical instrument maker, 21 Bull-street, Birmingham.—*Medical Times and Gazette, June 18, 1864, p. 685.*

96.—ACTIVE CONSTITUENTS OF ERGOT OF RYE.

Mr. W. T. Wenzell, of La Crosse, Wisconsin, has investigated the proximate chemistry of ergot, and he states (American Journal of Pharmacy, May 1864) that he has succeeded in isolating two vegetable nitrogenized alkaloids, the first of which he has provisionally named "ecbolina," and the second "ergotina." Mr. W. gives the following account of the physiological action of the alkaloids.

"The experiments were made by comparison with the powdered drug. Unlike most authors, who believe that ergot has no obvious action on the male, I have come to the conclusion that it has as powerful an influence upon the spinal column of the male as it has upon the female. I find a half grain of *ecbolina* to possess the same therapeutic action as thirty grains of ergot. From either the alkaloid or the powdered ergot in the doses mentioned, the following effects have been experienced upon myself:

"The functions of the brain were excited to a species of intoxication, in which participated the muscular system, causing involuntary contractions of the muscles, soon followed by nausea, a loss of appetite, a sense of weight and shooting pains through the head, stiffness and soreness of the muscles of the neck and extremities, a creeping sensation along the course of the spine; finally, a state of general relaxation and debility, soreness of the muscles, particularly those of the extremities, and a gnawing sensation in the stomach, with hunger. From the beginning to the end of the ergotic influence, which lasted about three hours, the pulse was not materially affected until the stage of debility supervened, when the pulse fell about four beats per minute. On doubling the dose, the only difference observed was, that the state of excitement was of shorter duration, but was followed by a greater amount of debility, greater weakness, with trembling of the extremities and pain through the chest.

"Half a grain of chloride of ecbolina was given to a strong, muscular man, weighing 180 pounds, and in perfect health. He complained of shooting pains in the head, nausea, frequent calls at micturition, pain and tightness across the chest, followed by a reduction of the pulse, depression of the mind, a dull pain with a sense of pressure above the orbits, and general debility.

"Experiments instituted with ergotina in a physiological point of view were less complete, owing to the loss previously mentioned. From the effect produced upon myself, I believe it to be less active than its congener, and although capable of causing some cerebral excitement, and a reduction of the pulse, I did not observe the same specific action upon the spinal column and muscular system."

Mr. W. gave a physician a solution of the chlorate of ecbolina to test its medicinal qualities in uterine hemorrhage and in parturition, and he reports having used it in several cases of uterine hemorrhage, with satisfactory results, but says that, "from the symptoms produced in the doses I had directed him to give, he was compelled to lay it aside, from the energetic and poisonous action it evinced, causing great nausea with distressing vomiting and intense headache." He thinks the ecbolina to be a powerful agent. —*American Journal of Medical Science, July*, 1864, *p.* 278.

97.—ON BORAX AS A SUBSTITUTE FOR ERGOT.

By E. Howard Moore, Esq., Cambridge Heath.

I have lately given borax in place of ergot during labour, when circumstances called for the exhibition of the latter drug, with so satisfactory a result, that I am desirous (with your permission) to make it known to the profession.

The effect produced by an ordinary dose (℈ij., dissolved in a little water), *when the uterus is disposed to act*, is constant and unremitting contractions of

that organ, which speedily puts an end to the labour, such as is followed by ergot *when* it acts; but I consider it has the advantage over ergot of seldom causing sickness (unless the dose be very large). I am also inclined to believe that its action is specific, the pulse being affected secondarily from the uterine excitement; but I would not advise it to be persevered in under that idea, if two doses (twenty minutes intervening between the first and second) do not produce the desired effect. I hope other practitioners will be induced to *try* borax, and furnish the profession with the result, whether favourable or otherwise, as I consider further proof of its uterine properties are required before it will supersede the use of ergot. Its uterine action only appears manifest at the time of labour, therefore it cannot be said to produce contractions *ab initio*.—*Medical Circular, June* 29, 1864, *p.* 429.

98.—ON THE DIET OF CHILD-BED.

By Dr. Graily Hewitt, Physician to the British Lying-in Hospital.

[The subject of the diet of child-bed is one which has been of late forcing itself on professional attention.]

The text-books most generally in use are those of Dr. Churchill, Dr. Ramsbotham, and Dr. Tyler Smith. The principles laid down in these works in reference to the diet of the patient during child-bed are to be gathered from the following quotations.

Dr. Churchill says, in reference to the diet: "Excess, by inducing feverishness, may retard the convalescence. The patient should be confined to slops—gruel, panada, arrowroot, milk, whey, weak tea, &c.,—with bread or toast and butter or biscuit, for five or six days, when the excitement produced by the secretion of milk has subsided; and if there be no counter-indication, she may take some broth, and on the seventh or eighth day some chicken or a mutton chop, with some wine-and-water." (4th edit., p. 234.)

Dr. Ramsbotham directs that nothing but tea, toast, or farinaceous food be given until the bowels are freely opened. A little beef-tea or broth is then allowed. To this, in a day or two, a light pudding is to be added; "and in a week she may be allowed a small quantity of solid meat." Stimulants of any kind are forbidden, under ordinary circumstances, until near the end of a fortnight. (p. 151.)

Dr. Tyler Smith says that no solid food should be given until after establishment of full secretion of milk and action of the bowels; but he at the same time adds that "cases sometimes occur in which the exhaustion is so great that animal food and stimulus are required from the first." (p. 319.)

From these quotations it is evident that the principle of practice recommended by these standard authorities is one of low diet from the first: Drs. Churchill and Ramsbotham ordering a low diet for as much as a week after labour has taken place; and Dr. Tyler Smith concurring in the principle of low diet as a rule, but admitting the exceptional necessity for deviation from this rule. The practice is, as I hope to show, wrong and unnatural. Nevertheless, the rules which I have mentioned to you are followed by a majority of practitioners. We are so grown up in the practice that it has hardly seemed to be extraordinary that a woman should be allowed little more than gruel, *ad nauseam*, for a week or more after her labour is over.

Why is it that it has been considered necessary to place a woman recently delivered on a low diet? It was thought that the adoption of a low diet was likely to be the means of preventing puerperal accidents and diseases. This is the principle on which these rules are based. Is this principle true? Are known facts in consonance therewith? I believe the principle to be entirely wrong; I am quite sure that facts do not bear it out—nay, that they distinctly contradict it. Let us consider for a moment what is the condition of a woman directly after delivery. The nervous system is much agitated; she is often much exhausted; her muscular system has been exercised powerfully and to an unwonted extent; she has lost a certain quantity, in many

cases a considerable quantity, of blood. The rational treatment of a patient presenting such symptoms would be a restorative one; it would involve (first) rest, and if possible, sleep; and (secondly) the administration of such nourishment as would replace what has been lost; and it is obvious that the patient will require food in proportion to the amount of loss sustained. Further, it must not be forgotten that in many cases the patient, although not giving any obvious external sign of weakness or prostration, is nevertheless in a state very closely approaching to one of exhaustion; and this is particularly observed where the constitution has been undermined by rapidly succeeding pregnancies in women who are insufficiently fed and badly cared for. The rational treatment then, I would repeat, is to administer food such as will restore what has been lost; and by "food" I understand whatever tends to support and maintain vital power—animal food especially, combined or not, according to circumstances, with liquid containing alcohol. So far as the condition of the patient immediately after labour is concerned, there would seem to be no reason for depriving her of such food and restoratives as would be administered under circumstances apart from the parturient state altogether, and with the view of alleviating similar symptoms.

But, it is argued, the patient must be kept on a low diet in order to prevent mischief arising, and to ward off certain evils to which she is liable. A low diet will prevent, it is said, the occurrence of what is called "inflammation." Let us consider these various "inflammatory" conditions liable to arise after parturition, with the view of ascertaining how far they are likely to be prevented, or the reverse, by the adoption of a low diet.

1. *Milk fever.*—This is usually described as an affection which comes on about the third day, when the breasts begin to swell, the pulse rises, and there is a feverish heat of the skin, these symptoms subsiding in the course of twenty-four hours, more or less. From what we read in books, we should conclude that this is a common disorder; but the fact is that it is a very rare disease indeed, so much so that an eminent authority, M. Pajot, of Paris, almost doubts the existence of the affection. As bearing on this question, I may mention that out of the last fifty cases which have been under my care in the British Lying-in Hospital there were only two in which the symptoms present had any resemblance to those of "milk fever." This disorder is, you will perceive, ephemeral; no bad effects result from it. And now an important question arises—Would this disease be observed if the patient were well fed? My own experience has led me to the conclusion that milk fever is less likely to occur when the patient is well fed than under the opposite conditions. In the two cases which I have just mentioned as observed recently by myself there was present a markedly defective state of the nutritive functions, and both patients had been, prior to their admission into the hospital, very indifferently fed. I strongly suspect that "milk fever" is in some cases connected with the practice, prevalent with some nurses, of not putting the child to the breast until one or two days after labour. This practice is one which I believe to be highly improper, and one calculated to lead to the production of sore nipples and milk abscess. On this point, however, I do not wish to enlarge at this moment. The point to which I wish particularly to call your attention is, that it is very questionable if a low diet tends in any degree to prevent the occurrence of milk fever.

2. We come next to the more serious puerperal diseases—"*puerperal peritonitis*," "*puerperal fever*, *phlegmasia dolens*, &c. With respect to the pathology of these diseases, there is very much more to be said than can be compressed into the short space now at my disposal, and I can only state those conclusions respecting them which may, as I believe, be made a satisfactory basis for the application of therapeutics. It was formerly considered, and the idea is still prevalent to a wide extent, that the essence of these serious puerperal affections was "inflammation." Thus when, two or three days after labour, the patient began to complain of shivering, of pain over the uterine region, when the pulse became frequent, these symptoms were considered to indicate the presence of inflammation of the uterus or of the peritoneum. It is now known, however, although not sufficiently generally ad-

mitted, in the first place, that these symptoms frequently indicate the passage of poisonous material into the blood, really a form of pyæmia; and, in the second place, that while mischief of an "inflammatory" kind may be set up in consequence of the introduction of such poison, or in consequence of violence sustained by the uterus during parturition, the best method of combating the inflammation is, not by employing remedies formerly considered anti-inflammatory, such as bleeding, antimony, mercury, administration of low diet, and the like, but by supporting the strength of the patient, and by exhibition of remedies of a soothing and sustaining nature. So, again, in cases of puerperal fever: the condition actually present is a poisoning of the blood, attended with symptoms of extreme depression, in the prevention and treatment of which low diet and lowering agents of whatever kind are, in my opinion, noxious and injurious in the last degree. In phlegmasia dolens, another accident of the puerperal state, the essence of the disease being erroneously considered to be "inflammation," it was supposed that a low diet would tend to prevent such inflammation. The word "inflammation" has much to answer for in respect to the injurious influences it has exercised on the treatment of puerperal diseases. It is responsible for the low-diet system which has so largely prevailed in the lying-in room—a system which, by weakening the patient, has rendered her liable to become a prey to the poisonous influences by which she may be surrounded, and has induced a mode of treating puerperal diseases calculated to neutralize and negative the efforts nature will always make to overpower and throw out the subtle agent creating mischief within. In the prevention of puerperal fever, the first thing to do is to prevent contact with septic agencies from without; the second, to secure the patient from the operation of septic agencies within. The latter indication is best fulfilled by securing early, good, and permanent contraction of the uterus. A relaxed uterus readily becomes the medium of absorption from the inner surface of the organ through the open extremities of its torn vessels. Perfect contraction of the uterus is, I believe, an almost complete safeguard against introduction of septic matter into the system, and contraction of this kind is best maintained by keeping up the vital powers of the patient, which can only be done by taking care that she is well nourished. Defective contraction of the uterus I have invariably observed to be present at the outset of an attack of puerperal fever.

Modern pathological research has removed phlegmasia dolens from amongst the affections requiring an antiphlogistic treatment and prophylaxis. The substance which fills the hardened vein was formerly believed to be the product of inflammation, but we now know that it results simply from coagulation of the blood. The blood coagulates in the veins; the clot may soften, and become converted into a soft, puriform material, which, though looking like pus, is only broken-down fibrin. Phlegmasia dolens may occur in men as well as in women who have not had children, and it is not unfrequently observed in cases of phthisis. Phthisis is, as we all know, not an inflammatory disease, its distinguishing element being defective nutritive power. It has been shown by Professor Humphry, of Cambridge, that this tendency to coagulation in the veins, apart from puerperal influences, is associated with a depressed condition of the vital powers, and he has offered abundant clinical evidence of the correctness of this statement. Now, in the case of a woman recently delivered, a depressed condition of the vital powers is very far from uncommon. If the uterus does not contract, an unusual quantity of blood remains in its vessels, and there coagulates. The coagulum spreads upwards by extension, and when it reaches the common iliac vein the circulation in the external iliac vein may become stopped at any moment. Undue loss of blood during or after parturition necessarily depresses the system, and facilitates coagulation in the uterine veins, a tendency still further increased by the circumstance that the uterus in such cases does not contract well. That phlegmasia dolens is more often observed after parturition, in cases where much blood has been lost, is a matter of observation; that it has been noticed to have occurred very frequently in cases where the vital powers *have been inadequately* sustained by nutritive material will become also evi-

dent to those who will take the trouble to inquire into the matter. The evidence to be collected, pathological as well as clinical, is all in favour of the proposition that by a generous diet will the tendency to phlegmasia dolens —supposing it to exist—be likely to be counteracted.

If, for the sake of argument, we admit that these puerperal accidents are inflammatory, the utility of a low diet cannot be maintained in face of the great alteration which has come over the professional mind in reference to the treatment of inflammation. The practice of bleeding has very largely gone out; mercury and antimony are far less relied on than formerly. There is certainly much doubt as to their efficacy in these cases. The absolute dietary formerly insisted on has equally fallen into disfavour.

It may be urged that I am arguing on theoretical grounds; but I can state, as the result of very careful personal observation, that the conclusions I have enumerated as to the bad effects of the low-diet system in the prevention and treatment of the puerperal diseases alluded to are amply borne out by the facts in my possession. I have also—and this is perhaps more to the point—abundant evidence of the most practical kind of the value of a generous, sustaining, and supporting diet and regimen, both in cutting short puerperal mischief of the worst kind, and in preventing its occurrence under circumstances most threatening to the patient. What I have seen of puerperal fever and allied disorders has, indeed, induced me to regard with the utmost horror all remedies of a depressing, lowering character. In the treatment of these affections, large quantities of food and brandy, or an equivalent, I have employed most successfully. It is rational to suppose, and it is consistent with my experience, that this gives a clue to the prophylaxis of these diseases. I say nothing of cleanliness, ventilation, separation from contagious influences, &c.; the necessity for these it must be superfluous for me to expatiate upon.—*Lancet, September* 3, 1864, *p.* 259.

99.—ON IODIDE OF POTASSIUM AS AN ANTILACTESCENT.

By Dr. Frederick H. Morris, Cheltenham.

It was in consequence of the unsatisfactory results obtained from the external application of belladonna, in arresting the secretion of milk, that I was induced to try the internal administration of iodide of potassium, first recommended, I believe, by M. Roussel, of Bourdeaux.

I have resorted to its use in upwards of a dozen cases, during the last five years, where it has been advisable to arrest the secretion—e. g., where the child has been still-born, or has died a few days after birth; where, from sore nipples, injury to the breast from prior abscess, or from some accidental cause, congestion of the breast has been set up, and lacteal abscess threatened,—and in no instance have I been disappointed, even when belladonna has failed to afford relief.

The following case may serve as an illustration:—

Mrs. R., was confined of her first child on the 25th of December, 1863. The child, a weakly infant, died a few days afterwards from atelectasis pulmonum. The breasts of the mother became tumid and painful. Extract of belladonna mixed with glycerine was applied round the areolæ for two days, when the pupils became dilated, without, however, any relief to the breasts. The patient was feverish and restless; the breasts were tumid and painful, and exhibited every appearance of speedy suppuration. Three grains of iodide of potassium in a saline draught was given every four hours, and five grains of Dover's powder at bedtime. Next day the breasts were less swollen, and she expressed herself as feeling much better. In the course of three days all tumefaction had subsided, and she discontinued the medicine.

The plan I adopt is to give three grains of the iodide in an ordinary saline draught every three or four hours. In from twenty-four to thirty-six hours the fever and engorgement have ceased, and in from two to three days all tumefaction has subsided, even where abscess seemed unavoidable.—*Lancet, August* 13, 1864, *p.* 179.

100.—ON SOME POINTS HAVING REFERENCE TO THE DIFFERENTIAL DIAGNOSIS OF FIBROUS TUMROUS OF THE UTERUS.

By Dr. C. H. F. Routh, Physician to the Samaritan Hospital for Women and Children.

The symptoms of fibrous tumour most dwelt upon in speaking of the disease are an enlarged abdomen, with a hard, rounded and solid tumour to be felt through the abdominal parietes in and about the uterine region, extending sometimes as high as the epigastric region, both lumbar regions being clear on percussion, and the umbilicus sunken in, not projecting. The tumour is also to be felt *per vaginam*, involving the uterus; an occasional loud and systolic souffle is to be heard over the tumour; menorrhagia is more or less intense; there is absence of fluctuation; the length of the uterine cavity is increased; and, lastly, owing to the close union of the uterus and bladder, the growth is drawn so near the abdominal walls, that it precludes the passage of the hand low down between the tumour and the walls of the abdomen. I will speak of these symptoms as I go on, to test their relative value.

1. *Physical Hardness. Roundness and Solidity of the Tumour.* The feel of a fibrous tumour has been considered by many as almost pathognomonic. This, in connection with its rounded and lobulated character, is such, that the combination of the two characteristics would seem to make error almost an impossibility. Any one, however, accustomed to examine cases of cellulitis and hæmatocele, must know the contrary. I know that the same thing has been observed in cases of ovarian dropsy, and it may be laid down as a general proposition, that a *thin* fluid, and *a fortiori* a *viscid* one, may be included in an inelastic bag and convey to the touch the impression of a solid.

I will illustrate this law by an experiment; and then mention a remarkable example in the living subject. Here is an ox's bladder contained within a calico bag. This bladder has been kept in spirit for a week, and is quite inelastic. The calico bag I put around it lest the bladder should burst. This bladder is now full of water and fluctuation is quite evident everywhere. At its opening however, you see I have affixed a tube connected with this pump, by which I can force in water *ad libitum*, securing its retention by this stopcock in the connecting tube. As I pump in water, you will find that fluctuation will gradually cease, till none of you shall be able to detect it; on the contrary, it will feel quite like a solid. If this be so with so thin a covering as this membrane and piece of calico, *a fortiori* will it be difficult in many cases to recognise fluctuation through the abdominal parietes; for I am quite sure fluids are contained within cavities of the body quite as tensely as in this bladder.

The following case, however, is so satisfactory on this point that I cannot but mention it.

An inmate of Bedlam Hospital, about thirty-five, declared herself pregnant; and Mr. Lawrence requested Dr. Greenhalgh to see her. Dr. Greenhalgh kindly allowed me to accompany him in his visit. We found a woman with a large tumour extending to above the umbilicus, very hard, nodulated, and giving altogether the impression of a solid tumour. The breasts, however, gave no indication of pregnancy. An examination *per vaginam* conveyed the same impression. The os, however, was hard; evidently that of an unimpregnated woman, and high up. The enlargement was chiefly in front of the uterus; and conveyed generally the same fibrous sensation. She passed urine freely; and indeed some was shown to us. On more carefully examining the tumour *per vaginam*, we thought that at one part we could detect some very obscure fluctuation, or at least a part less hard than others. It was thought wise that the bladder should be emptied of its contents. This was done; and about a chamber-pot and a-half of thickish urine was drawn. On examining the abdomen after the operation, the tumour was gone; and yet, had we trusted to mere manipulation, both Dr. Greenhalgh and I would almost have positively asserted that, if ever we had before felt a fibrous tumour, this was one.

2. *The Auscultatory Signs of an Uterine Fibrous Tumour* are four in num-

ber; *a*. Two souffles, one a tubular, another a vesicular murmur; *b*. A thrill; *c*. A single or double cardiac sound; *d*. Absence of multilocular arrangement indicated on percussion.

a. Fibrous tumours are sometimes the seat of a souffle. Most writers do not discriminate between the two, and appear to refer them to the same cause. Thus, it is described by Dr. M'Clintock as always

"Synchronous with the pulse; sometimes short and abrupt, a mere whiff accompanying each arterial pulsation; at other times, prolonged and musical, and not to be distinguished by the most acute and practical ear from the *bruit placentaire*. Like it, it is occasionally loud and intense for some pulsations; then becoming feeble and almost inaudible. Again, it can be diminished and suppressed by moderate pressure of the stethoscope over the spot. It is present in those who have sustained no loss of blood, as well as in the anæmic." He adds that, "although a very interesting phenomenon, it is not one of any special diagnostic value, being common to pregnancy and ovarian disease, which are the two conditions most likely to be mistaken for fibrous tumour of the uterus." (M'Clintock's *Diseases of Women*, p. 131.)

However unwilling to differ from so high an authority, I cannot but take exception to the sweeping conclusion drawn. Nothing can be more graphic and true than the greater part of this description; but I think the fact is, that two very different souffles are here spoken of. The true *tubular* souffles are, so far as I know, very rarely, if ever, heard in pregnancy and ovarian disease. The *bruit placentaire*, or one very like it, may be, perhaps often is.

Now, I believe that these two souffles are produced by two different causes. The first, or *vesicular*, *bruit placentaire*, if you like, is produced by the combined action of the large vessels immediately coming into and supplying the organ. The latest writer on the uterine appendages, Dr. Savage, believes these to be the only cause of souffle in fibroids, which are supplied largely by veins and arteries, usually at each side.

But, I imagine, these produce only the vesicular murmur. The same cause produces them in pregnant uteri: and it is not due to placenta, since it is heard after delivery, as long as the vessels supplying it are in the hypertrophied condition. It was long since shown by Nægele (*On Obstetric Auscultation*, 1838), that, in aneurismal varix, where a direct communication for the blood exists, so that it passes from the artery to the vein, the opposed or interfering currents gave rise to a sound which may exactly resemble the *bruit placentaire*. The sound is also heard loudest at the point where the uterine arteries, reaching the broad ligament, become thicker in diameter, more tortuous, and sink into the uterus. How far the connexion between the arteries and veins in the pregnant or hypertrophied uterus may assist in the production of this sound, or whether the general circulation throughout the uterus must be taken into account, because so extensive, is a point for future inquiry. If we consider, however, that the arteries in the hypertrophied organ directly communicate with the large uterine veins; either, according to the Hunters through the cavernous structure of the placenta; or, according to Weber, through the network of colossal capillaries; or, according to Goodsir, through a great cavity, which is everywhere traversed or intersected by filamentous prolongations of the uterine veins—we see how, physically, a sound like that of aneurismal varix could be produced by causes somewhat analogous. The later researches of Dr. Savage will rather explain the production of the sound from an extended circulation, if we assume the same structure to be present in the impregnated organ, only more extended in the hypertrophied or pregnant uterus. Now, it is unquestionable that, in many cases of fibrous tumours at their connection with the uterus, we have, as in pregnant uteri, elephantine venous sinuses. Such was found to be the case at the *post mortem* examination of Dr. Matthews Duncan's example of fibrous tumour, to be hereafter noticed.

This uterine murmur, or *bruit placentaire*, in either case originating from the same cause, is destroyed by pressure, if this be sufficiently strong; it may be by the stethoscope; it may be by intense uterine contraction, par-

ticularly in the vicinity and all around the *locale* of its production, exactly as it is abolished at the height of an uterine pain. Auscultation proves, then, that the souffle, after gradually rising in pitch up to the moment of greatest uterine contraction, becomes at last inaudible, except at the groins, where it is still heard, though more feebly so. For the same reasons, also, it is not heard immediately after delivery, when the uterus is firmly contracted, but again becomes audible in twenty minutes or half an hour afterwards, when, the uterine muscles having somewhat relaxed, the vessels again enlarge; a state which persists, except at intervals, for forty-eight hours or so, till such time as, in the process of involution, the vessels become permanently smaller. This, then, is one souffle which is occasionally heard in fibroids, and in every way resembles the placental souffle; and it is heard loudest at the groins, or over the entrance of the uterine arteries.

The *tubular* souffle, however, is different. It bears the same relation to the former as bronchial respiration does to vesicular, and is, I believe, due to pressure upon the aorta, and direct transmission of the aortal sounds through the fibrous growth. It needs only the continuity of a solid body between the aorta and abdominal parietes, and the aorta to be slightly compressed thereby, to give rise to these sounds.

A remarkable case illustrative of this explanation, is given by M. Hérard *(Bull. Soc. Anat.*, xxv, p. 148*)*, where a very thin woman complained much of epigastric pulsations. There was no tumour; but auscultation revealed a souffle and a thrill. The case was diagnosed as one of aneurism. She died shortly of phthisis. The *post mortem* examination revealed a voluminous and also indurated left lobe of the liver. This it was that transmitted the aortic beat, and, by expression, produced the souffle.

We have, doubtless, all met with analogous cases, particularly in weak females, and especially where the middle lobe of the liver was indurated. Pressure over any large artery by a stethoscope will give rise to the same sound.

The position in which this sound is heard loudest also varies from that where the vesicular murmur is most audible. This is strongest at the inguinal region; the tubular souffle is generally heard loudest at some prominent point of the tumour, or within the vagina upon the inferior portions of the growth. It requires also some strength to compress the aorta through a large fibrous tumour; and hence one reason why it is rarely, if ever, destroyed by pressure of the stethoscope.

b. This tubular souffle is occasionally accompanied by a *thrill*. This thrill is very similar to that which is communicated to the ear in the case of an aneurism of the aorta, as M. Hérard's case proves. Indeed, in well marked cases, the musical note which accompanies the souffle and thrill are so exactly alike the sounds heard in cases of aneurism in the neighbourhood of the heart, that it would seem impossible to detect a difference. Now, so far as I am aware, a thrill is never felt either in ovarian disease or in pregnancy. This is also the case in regard to the musical note. Once only have I heard this sound in the case of pregnancy. Then it was heard with the stethoscope upon the sacral region, the patient lying on her abdomen; and we can understand this. The aorta was pressed upon by the enlarged uterus, and the musical note, which could not be transmitted through the liquor amnii, was not heard in front. Behind, through the solid sacrum, it was readily heard. So far, this exceptional instance is a good explanation of its occurrence in fibroids.

c. Closely related to the tubular souffle is the *single* and *double* cardiac sound. This one sign, revealed by auscultation, I think most important; the more so, as it has not been hitherto sufficiently dwelt upon.

Let us remember that a fibrous tumour is, in most cases, a solid tumour. If it be of moderate size, and in any way can be or is compressed against the aorta directly or through the uterus—if the fibroid be placed in front of the organ—then the sounds of the aorta should be transmitted through the tumour. I do not know why it is; but these sounds are observed in practice to be of two kinds; first, a *single* sound synchronous with the systole of the

heart; secondly, two sounds synchronous with and representing the double sound of the organ. I believe that whenever either of these sounds occur, you have a solid tumour between your stethoscope and the aorta; and so that it is a favourable argument, *cæteris paribus*, for believing the tumour to be fibrous. It may occur in scirrhus of the uterus also. This sound is very distinct from *impulse*. I have seen and felt impulse in ovarian dropsy; but then the sound is not heard. This is especially true in regard to the *double* cardiac sound. Ovarian tumours, except in very exceptional cases, and only where they are solid, if at all, do not transmit the cardiac sounds; fluids being generally non-conductors of sound.

I have said that the tubular souffle is often heard loudest within the vagina. Occasionally it may be heard here with a common straight stethoscope; and if this be long, and made of glass, it is very easily applied. The patient is put on her left side; the nates are brought to the edge of the bed; and the thighs are well flexed on the body. If the woman be not too fat, and have a good perineum, and pressure be made on this part by the stethoscope, the instrument impinges the wall of the vagina against the tumour, and the souffle may be heard through it. In stout women, the stethoscope may be safely introduced *per vaginam* and made to press upon the tumour; and, because made of glass, the rubbing of the clothes upon it is scarcely heard, so that no exposure is needed.

I have also used a curved stethoscope made of glass with advantage. It may then be passed when the patient lies on her back.

As a rule, however, I prefer the instrument I now show you, which I call a *vaginoscope*. It consists of the ordinary double stethoscope, to the distal end of which is attached a wooden speculum; this may be introduced as a speculum; and the ear-pieces being applied to the ear convey any sounds heard in that region. At any rate, in obscure cases, you will find it useful.

I cannot better exemplify the truth of the preceding statements than by quoting the following remarkable case.

Miss C., aged forty-two, had pretty good health up to five years ago, since which period she had not felt so strong. The catamenia had always been regular; but until lately rather under time than over, and lasted five or six days. For the last two years she had been more unwell; the period had lasted a week altogether, lingering a little for the succeeding week; recurring every three weeks or twenty-three days. This increase was due, not to clots, but to an unusual quantity of water, which obliged her to use two napkins at the same time. The function began at the age of 17 or 18. Before 22, she had had dysmenorrhœa for one day, not since.

About three years and a half ago, on her lifting up a heavy weight, she felt something give way in her side as if she had twisted herself there; and she could not raise herself erect for the pain for a few minutes. Three years ago, while rubbing herself for a stomach-ache she felt a round tumour in the left iliac region. Even before this she had felt some bearing down in front. There was no alteration in the breasts at the time.

The *breasts* presented no areola. There were some small follicles around, whitish; becoming whiter on tightening sideways.

The *tumour* had enlarged, because formerly she could not feel it in the erect position; while now she could. At times it was very painful; always more so before a period. She thought she could connect the enlargement with the increased catamenial flow, and the colour was less dark than it was now.

On *external palpation*, there was dulness for four inches above the pubes. There was a projection half-way between the pubes and umbilicus, of the size of an apple, quite pushing forward the abdominal parietes at this point. This was rounded, and the hand could grasp it superiorly. The dulness extended on a level with this quite to the left side. On the right side, about two inches beyond the tumour, the cæcal region was clear, as well as the two lumbar regions. All the dull parts were hard to the feel. The central projection was, however, movable upon the growth below it. It could be moved on the right side, quite near to Poupart's ligament; on the left side

only half an inch. In either case, when so stretched, tight bands might be felt uniting it to the posterior growth.

Auscultation.—On the front of the first growth, extending to the right side, there was quite a musical murmur, with a thrill, ceasing on deep inspiration. Over the projection superiorly, there was no sound; just below it, however, and over the pubes, there was distinct *tubular* souffle. There was a less loud sound on the left part of the posterior growth, and *vesicular*.

Vaginal and Rectal Examination.—The os was small, high up behind the pubes; the lips were small. The whole pelvic cavity behind the vagina was filled by a large hard mass, smooth, somewhat lobulated; at the lower part, it was semi-elastic. Cystic sounding proved that the bladder was rather to the right side. The uterine sound penetrated to its normal length; an anteverted uterus, and turned rather to the right side. The finger in the rectum detected the tumour between it and the vagina, in the posterior *cul-de-sac*.

The *Vaginoscope* gave no indication of murmur. An exploratory puncture made at the inferior portion gave exit to about half an ounce of serum. This proved to be so on microscopical examination. The puncture afterwards bled freely; but the bleeding was arrested by the actual cautery.

The diagnosis was, extra-uterine fibrous tumour, with a pedicle attached to the posterior wall of the uterus, inferiorly infiltrated with serum. *British Medical Journal, May* 28, 1864, *p.* 579.

101—ON THE TREATMENT OF FIBROUS TUMOURS OF THE UTERUS.

By Dr. C. H. F. Routh, Physician to the Samaritan Hospital for Women and Children.

[The surgical treatment of Fibrous Tumours of the Uterus is of two kinds — 1. Enucleation of the tumour. 2. Removal of the tumour by gastrotomy. Enucleation may be primary where it is completed at the time or within a day or two, or it may be effected by inducing gangrene or death of the tumour by sloughing. In 1857 Mr. Hutchinson published some papers on this subject. (*Retrospect*, Vol xxxvi., p. 241.)]

The principal rules laid down by Mr. Hutchinson are: "1. The tumour must be well depressed into the pelvis by an assistant. 2. The first incisions must be very free, and pass deeply down into the tumour; thus not only completely dividing the capsule, but facilitating its bisection, should that afterwards be found requisite. 3. The opened capsule must be separated by the fingers, or, if needful, by blunt-pointed scissors, the finger being used as a director. Strong and large vulsella, with midwifery forceps, should be at hand, to be used for traction, if necessary. 4. The grand object is to draw down, after separation of the tumour, the uterus inverted with the tumour to the external parts, or as near as possible to them, which facilitates the operation. 5. After eversion has been completed, an examination should be made with the finger, also *per rectum*, &c., so as not to cut through an inverted pouch of peritoneum in separating the final attachments of the uterine tumour. 6. The uterus is then returned. Ice, ergot, stimuli, &c., are to be given as indicated by the symptoms."

The views here epitomised are so very much in accordance with common sense and sound surgery that I do not wish as a whole to controvert them. To one point only of these I would take exception. I do not believe that the grand object is to draw down the inverted uterus after separating the tumour, so as to bring the tumour as near as possible to the external parts because this renders the operation easier.

It cannot be doubted that simple traction of the uterus will often suffice to determine inflammation in and about the peritoneum around the organ. In the fatal cases of enucleation; death seems to have been due to phlebitis or peritonitis. Upon this practical point we can learn a lesson from the experience of practitioners in the removal of *polypi*, as a rule, a far easier operation *if the uterus* be pulled down.

One case is mentioned by M. Gaubrie (*Bull. de la Soc. Anat.*, vol. xvii. p. 209) where a polypus, supposed to be attached to the cervix (but found after death to be attached to the fundus), was pulled down forcibly. The pedicle yielded and it came away; yet that patient died from latent peritonitis. After death, the uterus was found healthy; but pus was found in the pelvis. Another case is mentioned by M. Demeaux (*Ibid.*, vol. xviii., p. 41) in which an enormous polypoid growth filled the cavity of the uterus. It was pulled down to the vulva, removed, and the parts replaced. There was no bleeding. Peritonitis followed and death two days subsequently. The *post mortem* examination revealed pus in the pelvic cavity. No uterus could be found in the pelvis; but a puckering and a sunken-in portion, within which the ovaries and a vaginal tumour, which was the inverted uterus, were found. This inversion, it is true, was supposed to have been the result of the weighty polypoid growth before traction had been employed. Yet death from peritonitis was the result of the operation. A more remarkable case, however is quoted by M. Pigné (*Ibid.*, vol. xiv., p. 11). This was a polypus as large as a fist, which was taken from a dead woman. During life an attempt was made to remove it; but the finger being first introduced within the rectum, no uterus could be felt. All extraction was now stopped, on the supposition that the tumour was an inverted uterus. Peritonitis and death followed. The *post mortem* examination revealed a pediculated polypus within a very small and anteverted uterus. No injury whatever of the peritoneum had taken place. The traction alone produced the peritonitis. Dr. Greenhalgh has informed me of a case which he saw at Würzburgh, where the mere traction of the uterus downwards produced peritonitis and death.

In the recorded cases of enucleation, where traction is mentioned particularly as having been made, the results may be referred to two heads; viz., where the uterus or the tumour was pulled down by fingers, a string, or other violent means; and, secondly, where this was done by forceps. The result in the two cases is very different. Of eight cases, in all but two of which the operation was that of primary enucleation—one being secondary, the other failing altogether—in three, the uterus was for the time completely inverted. Three cases died; in one of these, eversion had occurred; in another, forcible attempts were made to pull down the uterus, which failed.

In eight other cases, in all of which but one (where the operation failed, although the tumour sloughed away afterwards) the operation was primary enucleation, and the forceps were used, no deaths occurred. In two, the uterus was inverted. In two cases, it was the uterus which is stated to have been pulled down by the forceps. In the remaining six, it was the tumour which was so drawn down. All did well, except one patient, who had phlegmasia, and whose convalescence was not established till two months. All the tumours, however, where it is stated, were small; except in the patient who had phlegmasia, where it was large; and in another, where the tumour weighed eighteen ounces, and the diameters were four inches by six.

Was this difference in the result due to the better regulated traction which can be exerted by a forceps, or to the better direction given by it? Probably, where the forceps were used, the tumours were low down in the true pelvis, and so easily included in them. Short forceps were used in every case; and, therefore, it would seem that the traction was less forcible. Whenever these were used, the results were more satisfactory. The fact is indubitable. Still it seems difficult to say to what cause the occurrence of peritonitis is due, where it so occurs; when we know that in very many other cases where traction is employed it does not occur.

In explanation, I have but three suggestions to make. In cases of retroversion, I have never seen peritonitis follow traction to restore the organ to a straight direction, so as to allow the admission of the sound. Is it because in these cases the peritoneum is no longer in its normal state, already changed by the very unusual disturbances to which it has for some time past been subject? This is certainly true in many cases of ovariotomy, where numerous adhesions exist. Secondly, peritonitis, and accidents of like nature, are rare, if our patients have been well purged and prepared by

remedies before operation; a precaution, which, in the case of polypoid growths, is often neglected. Will this explain the difference? Thirdly, there is doubtless, in some cases, idiosyncracy and a predisposition to peritonitis.

The practical conclusion, at any rate, to be deduced is, that the very traction which is necessary in these cases is often in itself a source of danger, and a cause of death; and therefore, if possible, it should be avoided.

Enucleation by the Induction of Gangrene, or *Secondary Enucleation*, as it is called, is a much more tedious operation. Out of fifteen cases mentioned by Mr. Hutchinson, nine recovered, or 60 per cent.; and six died, or 40 per cent. In my table, of ten cases, in one the result was not stated. Of the remaining nine, four died, or 44.4 per cent.; and five recovered, or 55.6 per cent.

Mr. Hutchinson has concluded that, if other unpublished cases were taken into account, the results of the two modes of operation would be identical. The above figures, however, so far as they go, prove that enucleation by the induction of gangrene is positively less fatal than primary enucleation. Is this due to the traction which is employed being less marked?

The modes in which this operation has been practised have been various. The French method consists in using the knife, in most cases without the use of ergot at all. Dr. Atlee's consists in using, in the first place, ergot in repeated doses, so as to influence the uterus to contract and forcibly eject the tumour. Then the incision is made through the capsule, the ergot being continued, and the tumour gradually separated from the cyst by the finger, cutting, by the knife or scissors, any adhesions which may interfere with such separation. This is continued from time to time; the tumour in the meanwhile sphacelates and comes out by pieces, until the whole has come away, or what remains is capable of enucleation and removal. There can be no doubt that Dr. Atlee's method is an improvement upon that of the French school. It provides a *vis à tergo* to assist the operator.

Again, Dr. Atlee does not limit his operation of enucleation to the *uterus*. In several cases where, from the size of the tumour or its position low down in the pelvis, while the uterus is high up or difficult to reach, and in cases of extrauterine fibroid, which occupies similarly a true pelvic position, he does not hesitate to cut right through the *vagina*, so enucleating the tumour. This he performed in several cases; and, so far as I know, he was the first to attempt this mode of procedure.

In estimating the degree of mortality due to enucleation by the induction of gangrene, we learn again a lesson from what occurs in some cases of polypi, which are tied and allowed to slough away.

The experience of Drs. Robert Lee and M'Clintock gives the following numerical results of cases in which the ligature was applied.

	Cases.	Deaths.	Per Cent.
Dr. Robert Lee	20	9	45
Dr. M'Clintock	10	3	30
	30	12	40

And, no doubt, with a large sloughing mass in the vagina, exposed to the air and putrefaction, the absorption of putrid pus is a natural result. The same is true with regard to the fibroid within its cyst, only to a greater degree. In the case of polypus, we have merely mucous membrane, which is our absorptive surface. In the case of the enucleated fibroid, we have a raw ulcerated surface of the cyst, that part which is the vascular part of the tumour, and therefore eminently absorptive, ready to take into the system the vitiated pus. The surprise is that, in such cases recovery ever occurs. Fortunately, the *vis medicatrix naturæ* is often powerful enough to accomplish it. It is for this reason that I think Mr. Brown overrates the beneficial results of his modification in the operation, in first incising the os and allowing it to heal before he proceeds to gouging or enucleation. It is of use, *doubtless, as it* removes one additional source of absorption; but others *remain.*

The practical lesson which is taught by these remarks, is the necessity of frequent injections and washing with disinfectants, and obviating the production and long contiguity with ulcerated surfaces of effete and putrid matters.

Enucleation by Gouging, and inducing gangrene subsequently, is a modification of Mr. Brown's.

The instrument which he uses I now show you. It has already been exhibited before this Society, and, therefore, need scarcely detain us long. It was originally devised by Mr. Philip Harper. It consists of a hollow tube of steel with cutting knives. Contained in this tube is a hook, which can be pushed up by a spring, and thus grasps the tumour, whilst circular knives are carried through by means of a screw. In this way, a piece can be actually cut out of a tumour much in the same way as the central piece is cored out of an apple.

In the *Obstetrical Transactions*, Mr. Brown has published several cases exemplifying this mode of treatment. I may remark, however, that it is restricted by him to *intrauterine fibrous tumours of the non-pedunculated form*, growing from the inside of the uterus from a broad base. Mr. Brown has given us four cases in which he gouged in this manner. In three, a cure followed; but in one, the operation led to a fatal result. This death he attributes to the absorption of putrid pus through the cut edges of the cervix, which was previously laid open to expose the contained tumours. Hence, at present, his mode is in the first place to lay open the cervix, and to wait two or three days, or longer, till this has healed, before he proceeds to gouge.

The method pursued after the gouging, is to plug up the hole thus made by oiled lint, to arrest hemorrhage and provoke sloughing of the tumour.

In three of his published cases, however, the tumours were not gouged. In one the tumour was broken up by a pair of sharp scissors.

I may say that, through the kindness of Mr. Brown, (who has given me the notes of several of his cases, even hitherto unpublished), I know of two more examples in which, after the incision of the os, the tumour was broken up by scissors; in all of which recovery, in one case disappearance of the tumour, followed.

But a method which is likely to supersede in great measure these more bloody operations, is simple incision of the os, subsequently carried right through a portion of the tumour.

A published case of Mr. Brown, given in the *Obstetrical Transactions*, (vol. iii., p. 76,) is, however, peculiarly interesting, as the cure was open to ocular inspection, a piece of good fortune not always met with.

This was the case of a single woman, in whom, some seven years previously, he had removed from the os uteri a fibrous growth of about the size of a walnut; and in whom, at the time of the second operation, he had distinctly made out three fibrous tumours just within the os and projecting into the vagina. The os and cervix were freely cut open; and then each of these tumours was deeply cut into; the cut surfaces being dressed with oiled lint, and the vagina plugged. All these dressings were removed in forty-eight hours, and the vagina daily injected. A month afterwards, two out of the three growths had entirely disappeared, and the third was reduced to half its size.

In two other unpublished cases furnished to me by Mr. Brown, the same result was as conclusively observed. I do not dwell on these, because they are as yet unpublished. Altogether, he has mentioned to me ten cases in which this simple plan of incision of the external os was practised with the best results. As these are, however, only a few among several which will shortly be made public, I only ask you to wait and judge for yourselves.

Here we have, then, a practical lesson taught us. The mere incision will suffice to cause the absorption of the tumour. It is a lesson thus taught, from which Mr. Brown appears himself to have profited, as I now learn that in most cases he no longer gouges, but makes free incisions. The plan is safer. Sloughing in the tumour is thus set up, and this is disintegrated, and diminishes in size, and finally disappears.—*British Medical Journal*, July 16, 1864, *p.* 53.

102.—TWO CASES OF UTERINE TUMOUR MORE OR LESS SIMULATING OVARIAN DISEASE.

By Dr. W. T. Gairdner, Professor of Medicine in the University of Glasgow.

[The study of abdominal tumours has, since the remarkable success of ovariotomy and its adoption by the profession at large, assumed an importance not previously possessed by it. Is a given tumour cystic, or solid, or partly the one and partly the other? Is it ovarian, uterine, renal, splenic, hepatic, peritoneal? If ovarian, is it free, or adherent?—one cyst, or many?—simple, or malignant?—unbroken or ruptured?

The two following cases, in both of which ovarian disease was supposed to be present by persons of considerable experience, are of more than ordinary interest.]

The first case I shall relate is one in which a large and apparently homogeneous tumour, arising from the pelvis, was supposed by the majority of those who examined it to be either in whole or in part ovarian; the presumption to this effect arising from some points in the early history of the tumour, and from the peculiar elasticity of its resistance to pressure, which to some highly skilled observers appeared to convey an impression of its cystic character. The other case is one in which a pelvic tumour, originally solid and of moderate size, became afterwards the centre of a large fluid effusion; and this secondary development of the tumour being, not unnaturally, supposed an ovarian cyst, was about to have been tapped, when a spontaneous change took place, which averted the operation, and restored the tumour to its original character. In both of these cases it is very evident that a mistake in diagnosis, rashly adopted by a too sanguine ovariotomist, might have led to very serious results, and the details of the differential diagnosis, though perhaps not containing anything absolutely novel, will therefore, I trust, be received by the Society as not devoid of interest. In both cases I have been careful to preserve, as much as is consistent with the object of this paper, the very form and words of the original notes made at the time of observation:—

Case 1.—*Large Abdominal Tumour, of doubtfully solid consistence, and supposed by some to be cystic. Frequent uterine hemorrhages, leading to marked anæmia. Question of operation, Diagnosis of uterine fibroid. Remarkable effect of the ergo of rye.*—Mrs. R., aged forty-five, admitted to the Royal Infirmary, ward 8, on 9th July, 1863. In the very brief note taken at the time of admission, I find the summary of the diagnosis made at this time in the words, "A uterine tumour of four years' growth." I believe, however, that these words expressed only my general understanding of the facts, and by no means the result of a precise diagnosis. The record is by the late Dr. Hamilton, and was in accordance with my instructions that all cases not reported in detail should be noted in the way of a brief summary. The principal symptoms recorded on admission are, uterine hemorrhage and pain in the back; and the treatment adopted was by cold douches, opiate enemata, and tincture of muriate of iron internally, followed by gallic acid in frequent doses of gr. x., and a moderate allowance of wine. It was also on more than one occasion found desirable to plug the vagina.

In the course of the next six weeks this tumour excited much attention, in consequence of the interest felt at the time in the question of ovariotomy. Many of my friends had an opportunity of examining the case, and, rather to my surprise, some of the opinions given me were very confidently to the effect that the tumour was ovarian, and of the left ovary. As this opinion appeared to me to demand very careful consideration, and as the responsibility of recommending or of declining to recommend an operative proceeding was unquestionably very serious, I determined, before calling any final consultation, to make carefully renewed examinations in detail as to the possibility or probability of ovarian disease. The results of these examinations will be found in the following report, dated August 19:—

"*The tumour* fills a considerable part of the abdomen, from the pelvis

upwards, and has at the present moment a good deal of resemblance, in size and position, to a pregnant uterus about full time. It could scarcely be discovered from its present relations that it had arisen from one side or other of the abdomen, tympanitic percussion being present in both iliac regions; but her personal statements and convictions rather tend to the belief that origiually it inclined more to the left side, and she says it even now, when sitting up, inclines to the left side. Tumour is pretty movable with respect to the parietes, but from its size does not admit of changing place much in relation to the other viscera. Has had sensations of movement in tumour more or less resembling quickening. Percussion in front of the tumour is absolutely dull, from pubes to beyond umbilicus; but between umbilicus and epigastrium, gastric tympanitic percussion is distinct. The resistance given by the tumour to the hand is not that of a dense, solid mass, but neither is it distinctly that of fluid, and there is no fluctuation from side to side, nor even any distinct fluctuation from any one point to another; the sensation is rather that of a somewhat elastic, semi-solid mass, without any continuous cyst, but also without any great irregularities of surface, or abrupt internal changes of consistence. No nodules or hard masses can be detected at any point.

Examined per vaginam, the uterus is found rather high in pelvis, but not so high as to lead necessarily to the belief that it is drawn up by the tumour. The os is rather wide, but otherwise natural. The shape of the cervix is not that of a uniformly enlarged uterus. The cavity is somewhat elongated, probably about an inch, and with the uterine sound the body of the organ can be moved to a certain extent, independently of the tumour. On the other hand, pressure on the tumour from above impinges slightly on the uterus, but not so decidedly as to lead to the belief that the tumour and the uterus are one mass. Breasts are perhaps a very little enlarged, and there is an approach to development of the follicles, but nothing like an increase in the glandular substance. There has been a good deal of hemorrhage since admission, and a good deal of clotted blood has come away: latterly, however, there has been an intermission of the hemorrhage, and patient has been ever since feeling better and stronger. Within the last few hours there has been a tendency to its return, and patient has resumed the use of gallic acid in 10 gr. doses as formerly, to be regulated by the amount of hemorrhage.

There has been at no time any considerable pain, but rather a feeling of weight and burden, with weakness of back, vague uneasiness, and swelling of limbs; this latter symptom, however, being easily under control by lying on her back, and therefore not having been observed since admission. Handling of the tumour in the various examinations has not caused much pain; but on one of the occasions when the uterine sound was passed she had pain in her back, lasting the greater part of the day. She now states quite decidedly that the tumour was noticed at least four years ago, and that it had when first noticed already attained considerable size, perhaps the size of a ball, in the left iliac and lateral region. At this time she was subject to prolapsus, and she thinks there can be no mistake about this, as she was not only told so by a doctor, but used to feel something hard herself come into the passage when she stood up. At the same time it is to be remarked that she has had this sensation several times since admission, and even so late as yesterday, while nothing observed by any of the physicians makes it probable that there is really prolapsus. Patient is very much blanched, and has a yellowish hue, not unlike that of carcinomatous disease, but there is nothing in the physical examination to make it probable that the tumour is of this nature. General health otherwise good; appetite good; no fever. Pulse after examination 94; tongue clean; respiration quiet.

"It does not appear that there is any very serious obstruction by the tumour, either to micturition or defecation, but she states that at the early period of its growth, micturition used to be very frequent, perhaps as often as forty times in the day; and that latterly there has been at times a tendency to obstruction of the bowels, and a sense of undue pressure during

the act of defecation. In regard to the hemorrhages, she states that at first they used to come about the close of the monthly periods, but that latterly the floodings have come at once at irregular intervals, and without any warning. On being particularly questioned, she remarks upon a fall she had five years ago, as being a possible cause of her complaint; the fall was upon the lower part of her back, and caused great pain, laying her up for about a fortnight. She went to a doctor, but got only a liniment, and on after consideration it does not appear that her health has ever been re-established. Her previous history as regards child-bearing is as follows:—She has had five living children, and one miscarriage; the last pregnancy was a living child, and there was nothing unusual in any of her deliveries. Husband died thirteen years ago. On one occasion, however, shortly after the birth of her second child, she felt as if something had given way while she was on her knees doing up the fireplace at night, and some time after this she had a very severe pain in left side, lasting a night. From these two occasions she dates the tendency to prolapsus, but though advised to submit to a medical examination, she did not do so; and her other children were born naturally after this. Since the death of her husband, thirteen years ago, she has menstruated with great regularity on the whole; and even since the severe floodings she thinks the regular discharges have continued, though rather frequent. On one occasion only she has been disappointed of the usual discharge, and this was since the floodings commenced. On being particularly questioned, however, she states that she 'has always had a large quantity,' and explains that this refers to the period since her husband's death. She took little notice of this at the time, though it had not been a habit with her in early life to have more than other women. On looking back, however, she is now satisfied that for many years before she can say there was anything like a flooding, the menstrual discharges were in decided excess, and this to a morbid and sickening extent. Has had more or less leucorrhœa all her life, and this tendency also has increased since she ceased to bear children."

It must be here stated, that although somewhat shaken at one time in my belief, as to the exclusively uterine origin of the tumour, I never adopted the view of its being exclusively or chiefly an ovarian cyst. It was, however, difficult to avoid admitting the possibility of a combination of uterine and ovarian disease; and as the opinions of those who believed the tumour to be, at least partly, cystic in character, were entitled to great weight, I was disposed to the compromise of regarding the larger swelling as, at all events, *possibly* ovarian, while the hemorrhages and pains were, with far greater probability, to be referred to a uterine tumour. Although for obvious reasons I prefer to avoid mentioning names, I may state that this balanced state of opinion was shared by more than one of my colleagues. Under these circumstances the practical questions connected with the case assumed, if possible, a more serious aspect than ever, and in particular, the following difficulties pressed upon my mind the more heavily, that careful inquiries in the best-informed quarters at hand, failed to elicit any very precise information on the subject. It became necessary, *e. g.*, to inquire—

1. Supposing, for the sake of argument, the tumour to be ovarian *only*, would the hemorrhages probably be arrested by its removal?

2. Supposing the hemorrhages to have a *separate* uterine cause (as I all along believed to be probable), what effect would be produced on them by the excision of a diseased ovary; and what effect would the presence of uterine disease have on the probabilities of success in an ovarian operation?

3. Supposing, in case of extremity, an operation performed to remove so much of the tumour as should be found to be removable, and at all events, if at all possible, to remove the cause of the hemorrhages, would it be justifiable to attempt the excision of a uterine growth, believed to be of immediate danger to life?

Having failed up to this period of the case to obtain a satisfactory solution *of these questions*, and being personally still of opinion that the greater

part, if not the whole, of the disease might probably be uterine, it occurred to me to submit a brief statement of the facts hitherto ascertained to the judgment of a wider circle than was accessible to me close at hand; and this, especially in the hope of attracting the attention, and eliciting the opinion, of some of the English surgeons who had given special attention to operations upon the pelvic organs. With this view I determined on publishing a statement, founded essentially on the preceding report, in the *Medical Times and Gazette* of August 29th, 1863; and this paper will be found on examination to reflect very faithfully the doubts and difficulties I have indicated above.

I owe it the kindness and courtesy of the friends who responded to this appeal to state, that their communications by private letter were of the greatest possible service to me, and contributed, in a degree which can hardly be overestimated, to the removal of the difficulties felt with regard to this case. It is with a certain feeling of reserve and delicacy that I allude to opinions given in this manner, as it is probable they may be considered by their authors as not quite fit for publication; but I have fortunately the free permission of Mr. Spencer Wells to make this use of the following valuable letter, in regard to which I will only remark, that it was founded on a private statement of the case to Mr. Wells himself, and not on the subsequent publication in the *Medical Times and Gazette*:—

"I think your case may be explained on three suppositions—

"1st (and most probable). That a fibrous tumour of the uterus, of moderate size, exists with an ovarian tumour.

"2nd. That the tumour is not ovarian, but a fibro-cystic tumour of the uterus; or—

"3rd. That a sanguineous cyst of the ovary empties itself through the Fallopian tube and uterus.

"As to 1st. It is not uncommon to find small fibroid peritoneal outgrowths from the uterus—or larger insterstitial growths—or ingrowths toward the uterine cavity and vagina, complicating ovarian disease, and this I believe to be the usual cause of menorrhagia or uterine hemorrhage, when it is observed in patients suffering from ovarian disease. The rule is (and the exceptions are rare) that menstruation is scanty and hemorrhage rare in simple uncomplicated ovarian disease.

"2nd. It is very difficult—perhaps impossible—to distinguish between a multilocular ovarian cyst and a fibro-cystic uterine tumour when the cysts are large and the connexion with the rest of the uterus elongated. I removed one such tumour, which some men of great experience took to be ovarian. The cyst held twenty-six pints of fluid. The seat of pedicle connecting it with the uterus was three to four inches long, and the uterus moved quite independently of the tumour. Indeed, it was not till I came to divide the pedicle that I knew what I had to deal with.

"3rd. It is not very uncommon to find the contents of ovarian cysts, varying very much in character, emptying themselves through the uterus and Fallopian tube. I have seen (by means of the speculum) ovarian fluid escaping through the os uteri, and I have a preparation in which the communication through the Fallopian tube is visible. The patient was seventy years of age—ceased to menstruate twenty years before, but a year or two before death began to menstruate again (as she thought). But it was rather uterine hemorrhage, recurring with tolerable regularity, than menstruation. At last she died rather suddenly, and after death three to four pounds of clotted blood was found in the peritoneal cavity, which had proceeded from an ovarian cyst, with a very vascular internal coat. It had burst, but the Fallopian tube (as large as a crow quill) communicated freely between the cavity of the cyst and the cavity of the uterus.

"If the life of the patient under your care is threatened by hemorrhage, and this cannot be checked by ordinary means, I should think it would be wrong to allow her to die without trying what surgery could do to save her."

From another very eminent surgeon I received a most instructive letter,

from which I am sure he will allow me to make the following extracts, though I do not consider that I am authorised to submit them as containing a matured and elaborated opinion:—

"As you request facts in reference to your case recorded in this week's Gazette, I feel inclined to mention briefly one or two similar ones. I have three times had patients sent to me with a view to ovariotomy, when on careful examination I came to the conviction that the tumour was uterine. In all hemorrhage had been a very prominent symptom, and in two of them the patients were exceedingly reduced in health by it. Two of these I know to be still living; a period of five years in one, and four in another, having elapsed since I first saw them. In both the liability to floodings has ceased. Both patients remain of the size of the full period of pregnancy, but without other inconvenience. The third I saw but once. The careful perusal of your case leaves on my mind a strong impression that your patient is the subject of an *uterine* tumour. Not a few such have been removed by abdominal section, some in mistake for ovarian cysts, others after a correct diagnosis. I know of very few facts which encourage to a repetition of such operations, and of many which warn against them. Such uterine tumours differ most essentially from ovarian ones in the prospect of life to the patient, for I believe they usually cease to cause hemorrhage and to grow."

A third surgeon of high standing concurs with the last in the opinion that Mrs. R.'s case is probably one of uterine tumour, and adduces several facts in favour of the view that "surgery can give no assistance." He adds, "I may tell you that I once saw a case, of which yours forcibly reminded me, laid on the table, chloroformed, and the peritoneal incision completed, all this by an experienced accoucheur, when the tumour was distinctly seen to be entirely uterine. The wound was then closed up, and the patient is still alive."

From more than one obstetric authority, I received either verbally or in writing, somewhat similar opinions, the tendency of the whole correspondence being decidedly unfavourable to operative interference. In regard to a point of diagnosis which had weighed very strongly with me from the first in this case, but upon which I found considerable difficulty in arriving at a conclusion founded on accurately observed facts, the following concurs very closely with what I am disposed to consider the true state of the case, and may possibly be of use to others called to give advice in circumstances like those here narrated. "I have never known," writes a well-informed and careful observer, of large special experience in diseases of the female organs, "an ovarian tumour cause the hemorrhage you describe. It will often produce menorrhagia, but not flooding; whereas uterine tumours of all kinds are commonly associated with this latter symptom." Several other obstetricians of very large experience have assured me that ovarian tumours scarcely ever cause considerable hemorrhage; and although I am bound to admit that opinions of an opposite character have reached me, I incline to accept the doctrine here stated as substantially correct, as it certainly is in accordance with my own comparatively limited experience.

To return to the case of Mrs. R. In consequence of various suggestions as regards the diagnosis, some renewed observations were made, in which I had the valuable assistance of my friend Dr. Leishman, under whose care I left Mrs. R., during a brief visit to the continent. On passing the uterine sound with some additional precautions, Dr. Leishman found that it "passed for about three inches with ease, the *os uteri* being tilted forwards towards the symphysis, which seemed to render the introduction of the sound even easier than usual, so far. After some attempts to ascertain the size of the cavity, the handle of the sound was depressed, and the point of the instrument at once passed up several inches further than before (about eight inches in all); and even this may perhaps not be the end of the cavity. The sound was not bloody on withdrawal."

Dr. Leishman, also, following out an indication referred to repeatedly in the correspondence above mentioned, discovered for the first time a very *distinct* souffle, heard over a considerable space above the left groin. This

souffle has since been constantly observed, and is, I am inclined to think, quite conclusive as to the uterine origin of the tumour; while the greater length of the uterine cavity than was at first discovered, removes all difficulty in supposing the whole mass to have this origin.

But perhaps the most important observation made by Dr. Leishman in this very remarkable case, is that with which I have now to close this somewhat protracted narrative. On September 16th, in the evening, Mrs. R. had a rather alarming return of hemorrhage; and as she had been very much reduced by previous attacks, twenty grains of ergot of rye were at once given, in the form of infusion. The effect of this dose was a complete arrest of the hemorrhage, but along with this effect severe pains were felt both in the back and in the tumour, which the patient spontaneously compared to those of labour. But the most interesting fact of all is, the coincidence of these changes with a very remarkable diminution in the size, and change in the consistence of the tumour, which I had no difficulty in verifying on my return to duty, and which I find noted in the following terms in the ward journal, under date of 16th September:—"The tumour, which previously had been soft and semifluctuant (see description in former report, p. 240), has become firm and solid, and to-day feels quite like a solid tumour. She spontaneously drew attention to this change in the consistence of the tumour, and she thinks it is rather smaller in size."

I have only to add that this change has been, so far as hitherto observed, a permanent one. During the remaining stay of the patient in the hospital, I took great pains to bring her case under the notice of some of those who at a previous period had regarded the tumour as cystic, owing to the presence, as they considered, of a character resembling fluctuation. The solidification of the tumour was quite apparent to all, and those who had most accurately observed it considered that the diminution in bulk might be about one-third, at the time the patient left the house (December 12th). The last report represents her as greatly improved in health, and although the tendency to bleeding was not entirely controlled, it was very much diminished from the 16th September, onwards. Her own convictions were quite decided to the last in connecting this improvement with the dose of ergot, and she stated to me that the pain experienced after it, lasting for ten or eleven days, and requiring repeated administration of sedatives, was totally different from anything she had experienced before or since, with the exception of labour-pains.

The conclusions I draw from this case shall be very briefly stated, as follows:—

1. The latter portion of the narrative clearly establishes the uterine origin of the tumour.

2. It therefore follows that a uterine tumour, *when the uterus is relaxed*, may resemble an ovarian cyst so far as to give the impression of fluctuation in some degree even to skilful and practised hands.

3. In such cases the coexistence of considerable uterine hemorrhage with the tumour is a strong, if not absolute, proof of its uterine origin.

4. A souffle in either groin is probably conclusive.

5. Operative interference is scarcely warrantable, unless on a very strong proof of immediate danger to life.

6. A dose of ergot of rye may not only restrain hemorrhage, but bring a more or less permanent solidification of the tumour, together with marked diminution in its size.

I have not here entered on the questions connected with the operations proposed in various quarters, for dealing with fibrous tumours of the uterus *per vaginam*, as the great improvement in the health of this patient has not allowed any room for these questions being raised in a practical shape at present.—*Glasgow Medical Journal, July*, 1864, *p.* 178.

103.—ON TREATMENT OF RETROFLEXION OF THE UTERUS BY OPERATION.

By I. Baker Brown, Esq., Senior Surgeon to the London Surgical Home.

[The patient was a widow lady who had had a large family of children, and was suffering from complete retroflexion of the uterus. The treatment ordinarily recommended in such a case is rest and the use of the stem pessary. The stem is introduced into the os and cervix while the cupped extremity rests upon the floor of the vagina. This treatment is, however, exceedingly unsatisfactory.]

It occurred to me that the muscular tissue of the uterus was not passive in retroflexion, and that there was probably active contraction at the point of flexion. If this were the case, incision of the os and cervix should relieve to some extent, and possibly remedy, the retroflexion. I determined to act upon the supposition in the case in question. Proceeding in the manner described in my last lecture, I fixed the uterus, introduced the hysterotome within the os and cervix, and divided the parts freely up to, but not through, the internal os. The result exceeded my anticipations. Without adopting any other means, excepting rest, the uterus resumed its normal form, and in two months the patient left the Home perfectly cured.

Shortly after the occurrence of this case, a very distinguished lady, who had been suffering for fourteen years from retroflexion, placed herself under my care. She had been subjected to every known mode of treatment, and had even worn a stem pessary three years and a half, without obtaining any benefit. The difficulty of relieving the bowels was exceedingly great, and the sense of bearing down, when she stood erect, was so oppressive as to prevent walking exercise, or standing, except to a very limited extent. Finding that the fundus moved freely, and feeling certain that there were no adhesions to prevent the uterus resuming its proper position, I urged her to submit to the same mode of treatment as had proved so successful in the case just stated. After some hesitation she assented. I incised the os and cervix. The os had become so hardened, doubtless from the use of the stem pessary, that it resisted the blade like firm cartilage, and the sound of the incisions was distinctly heard throughout the room. Immediately after the incisions had been made, the uterus seemed to spring into its place. I plugged the vagina well with oiled lint, as I am accustomed to do after incision of the os and cervix. On the third day after the operation an injection was given with great ease, and the bowels acted without discomfort. For fourteen years the bowels had never acted without the use of five or six injections, and great difficulty had been experienced in administering them. In the subsequent treatment of the case the bowels were readily relieved by mild laxatives, which were substituted for injections. Five weeks after the operation my patient was able to walk an hour with comfort and advantage. The uterus had recovered its normal form and position, and the sufferings to which she had been exposed many years were entirely done away with. Since then there has been no return of the retroflexion.

The happy results of these two cases convinced me that the method of treatment I had adopted was sound; and subsequent experience has confirmed my early conviction. I have pursued the same method frequently in private practice and on several occasions in this Home, and in every instance with success. Let me mention, that the introduction of the hysterotome into the cervix uteri in cases of retroflexion is sometimes attended with considerable difficulty, and some little tact must be exercised in order to pass the instrument beyond the flexed part.

Incomplete retroflexion is not uncommon in unmarried females. The following case may be cited as an example:—M. O., aged twenty-six, was admitted into the Home on the 18th of March of the present year. She had never been strong. For nine years she had suffered great pain, particularly in the groin, at the menstrual periods, and menstruation had been very *irregular*. She had also been subject to frequent vomiting for six years; and

for some time she had experienced much pain whilst evacuating the bowels and bladder. On examination, the uterus was found to be slightly retroflexed, the clitoris was large and hard, and there was painful fissure of the rectum. The patient had worn pessaries for fifteen months at a time, but with no permanent benefit. This case is an apt illustration of many often met with. The abnormal condition of the uterus had arisen from, and was kept up by, habits of delection, to which the patient confessed she freely gave way. The hardness of the clitoris was due also to this cause, and probably the fissure of the rectum—a condition often present in these cases. The true treatment, then, would be to remove the great incitement to self-indulgence, the enlarged and excitable clitoris, and cure the fissure of the rectum. This being done, the uterus probably would recover its normal state without further interference. I excised the clitoris, therefore, and divided the fissure, and in two months the patient left the Home cured. The uterus was normal in form and site, the fissure of the rectum had healed, and the tendency to self-indulgence was removed.

The true source of uterine disturbance and displacement in both the married and the single is to be sought more frequently than is commonly supposed in habits of self-indulgence. In cases thus originating, it is only by excision of the clitoris that permanent relief can be obtained. I stand probably alone in attributing so important a part in the production of uterine disturbance and displacement to self-indulgence; but the extended experience I have had of cases which have yielded to no other treatment than excision of the clitoris, and the happy results which have followed early recourse to the operation, convince me that the opinion I have formed, and the practice which I have based upon it, are both sound. Self-indulgence, it is important to remember, is not confined to the unmarried, and it is frequently a source of sterility in the married, chiefly in consequence of the retroversion and retroflexion of the uterus to which it is apt to give rise. In such cases excision of the clitoris is a sure remedy for the sterility, and dilatation of the mouth of the uterus will prove of no utility.

Little need be said of anteflexion of the uterus. The condition is rare, and, on the whole, it gives rise to less inconvenience than either retroversion or retroflexion. The treatment to be pursued is similar to that recommended for retroversion.

While speaking of retroversion, I omitted to mention that the os uteri is sometimes thrown forwards under the arch of the pubis, so as to press upon the urethra and prevent the bladder being readily or fully emptied. Several years ago, an unmarried lady, about thirty years of age, was brought to me from Brighton, supposed to be suffering from ovarian dropsy. On examination, I found the uterus retroverted, the os being fixed tight under the arch of the pubis. Being dissatisfied with her account of the freedom with which she passed water, I introduced a catheter into the bladder. An enormous flow of fetid urine followed, and the abdominal tumour disappeared. The supposed tumour, indeed, proved to be the distended bladder. The retroverted uterus had prevented the proper emptying of the bladder from day to day, and the urine had gradually accumulated so as to cause the distension which had been mistaken for ovarian dropsy.—*Lancet, August* 13, 1864, *p.* 174.

104.—ON TANGLE TENTS.

By Dr. J. Y. Simpson, Professor of Medicine and Midwifery in the University of Edinburgh.

[The following is a resumé of a paper read by Dr. Simpson at a meeting of the Obstetrical Society of Edinburgh.]

Professor Simpson reminded the society that a variety of substances had been used as tents, passed into the os and cervix uteri with the view of dilating the canal, so as to get access into the interior of the uterus for diagnostic and therapeutic purposes. In olden times they had made use of the dried gentian root, which was described by Cooke in his "Marrow of Surgery," and by various other writers, as the best substance for this purpose.

His friend, Dr. Horatio Storrer, of Boston, had a few years ago proposed tents of the bark of the slippery elm. What he (Prof. S.) had long been in the habit of using, as they were all aware, was a piece of sponge, which was brought into the proper shape by dipping it in a solution of gum, compressing it into a conical form by a piece of whip-cord wound round it, and then drying it in an oven. Dr. Braithwaite had afterwards pointed out to him that the gum was unnecessary, for if the sponge were compressed after being simply dipped in hot water, it retained its form on being dried, and all that was then needed was to dip it in a mixture of wax and lard, to make for it an external coating, to facilitate its introduction into the uterine cavity. Latterly, Dr. Sloan, of Ayr, has proposed as a substitute for compressed sponge the dried stems of the sea-tangle (*Laminaria digitata*), which is thrown up in such abundance along all our coasts. In its dried and collapsed condition it could be cut or filed down to any shape or size, and when placed in water, or brought into contact with the fluids of the uterus, it would swell up to such a degree that a piece of tangle would expand in the course of a few hours to about three times its original thickness. For some time past he (Prof. Simpson) had used the tangle-tents, and had found them to fulfil all the indications of a uterine tent so admirably, that he believed they would ultimately replace the sponge and other substances hitherto employed. He had brought with him some specimens of tangle-tents made by Krohne in London, that the Fellows might have an opportunity of seeing them; and also some bougies for the male urethra, and for the lachrymal ducts,—for its application was not restricted to the uterus, but might come into play for the dilatation of any kind of constricted canal. The tangle did not perhaps distend to such an extent as the sponge, but it exerted much greater power of dilatation; for whilst a piece of sponge, which on withdrawal seemed to be of the thickness of the thumb, was found to have left a canal which would barely admit the little finger, the canal left on the removal of a piece of tangle was always found to correspond throughout in measurement with the full size of the expanded tent. The tangle had a further advantage over the sponge in that it does not absorb—or form rather—those fœtid fluids with which a sponge always seems to get charged when left long in the genital canals. The tangle-tent, moreover, was more easy of introduction, because of its greater solidity and stiffness; and though sometimes he had found a little trouble in introducing it in consequence of its becoming slippery whenever it got moistened, yet then he had succeeded in passing it with the aid of a pair of long dressing-forceps, or, better still, of an old porte-aiguille. Then the greater abundance and consequent cheapness of the tangle presented an additional advantage; and if we wanted to be very economical, the same tent might be used a second time after being simply dried,—a process that would be attended with much danger if we had to do with a piece of sponge that had once been impregnated with deleterious discharges. The tangle did not dilate quite so rapidly, perhaps, as the sponge; but the expansion was effected with sufficient speed for all ordinary purposes. He showed a tangle-tent expanded to about the thickness of a little finger, which he had removed from a patient on his way to the meeting at eight o'clock, and which had been introduced in the forenoon. In its dry, undilated condition, it might have been about the thickness of a crow-quill. On the whole, he thought that tangle-tents would come to replace the others in use, because they were,—1st, cheaper in price; 2d, more easy of introduction; 3d, more potent as dilators; and, 4th, cleanlier in their use.

Dr. KELLER remarked that he had lately been using the tangle-tents, and had found them answer the purpose admirably; he felt sure they would soon supersede the sponge-tents in common use. He found that after drying them they were useful a second time. There was a difficulty in introducing them, which he had endeavoured to overcome by the use of a pair of long curved forceps, upon which, however, he was making some improvements, and would exhibit them at the next meeting of the Society. He had used tangle before as bougies, but it did not occur to him to dry it for the *purpose of re-expansion.*

Dr. A. R. SIMPSON had found this kind of tents very apt to slip out. A piece of sponge placed below them held them in position till they began to dilate, but probably a piece of worsted wound round the point would be sufficient to overcome the difficulty.—*Edinburgh Medical Journal, July*, 1864, *p.* 74.

105.—ON OVARIAN EXTIRPATIONS.

By Dr. CHARLES CLAY, Manchester.

[The following two cases are the 109th and 110th operations for the extirpation of diseased ovaries, and the 75th and 76th which have out of this number been successful.]

Case 109th.—Mrs. Meakin, of Burton-upon-Needham, Staffordshire, was sent to me in the autumn of 1863, labouring under ovarian disease of some standing. The abdomen was considerably larger than full-sized pregnancy, very tense, and of equal globose form, more fully prominent on the right side; fluctuation very distinct, and equal throughout the entire surface. I consequently concluded that there was but one large cyst, and in its present condition of tensity, it was impossible to decide as to whether it was accompanied by any solid matter or adhesions. I therefore declined to extirpate the tumour, until better satisfied of its real condition, which I could only ascertain by tapping the sac in the first place; and if, on its being emptied, any solid nucleus presented itself, I would proceed to extirpate the whole within two or three days of the time of tapping; but if no such solid mass appeared, I would then give the patient a chance of cure by the injection of tincture of iodine into the interior of the sac, and wait the result of the inflammation set up, with the view to obliterate the secreting surface of the sac; believing this case to be one of those that had a right to the chance of cure which injection afforded, before the question of extirpation could be justifiably entertained. I proceeded therefore to tap the sac, and removed about thirty pounds of thick, dark, viscid fluid, much coloured. On the sac being entirely emptied, I searched carefully for solid deposit, without finding any; and having been very careful in the mean time that the canula of the trocar was kept well in the walls of the sac, I injected through it about four ounces of the strongest tincture of iodine of the Edinburgh Pharmacopœia, mixed with about as much water, and then closing the mouth of the canula with one hand, taking care the canula did not slip from the sac, with the other hand, by a sort of kneading process I pressed the parietes of the abdomen, so as to secure the probability of every portion of the interior part of the sac having been in contact with the fluid. The mouth of the canula was then opened, and as much of the fluid as would, allowed to escape; which was about half the amount injected in. This is usually the case, so much of it being retained in the walls of the sac. The colouring matter of the iodine had almost wholly disappeared, which is also usual. For the first twenty-four hours after tapping all was quiet, notwithstanding the large amount of tincture of iodine used, and which generally surprises my medical friends. On the evening of the second day, sharp shooting pains set in, accompanied by a tympanitic condition of the abdomen. I allowed these symptoms of inflammatory action to be fairly established before I interfered, and then by active and constant fomentations, first with greasy water, and afterwards with a little turpentine added, until the inflammation was fairly subdued without the aid either of leeches or lancet. The abdomen being now reduced to its natural condition, the inflammation which had been roused for a special purpose had fairly been established; it only remained to watch the result. I had some hope the case would do well; that is, that the sac secretion would be checked, and extirpation unnecessary: for on reference to my former operations of this nature I found I had injected five cases previously, four of which had completely succeeded, and showed no signs of return, although some years had now elapsed. One case, however, had failed, and afterwards had to submit to the operation of extirpation. This case of Mrs. Meakin was, as the follow-

ing report will show, another case of failure; or as the following table will show, two cases of failure to four cases of cure by injection.

But to return to the case of Mrs. Meakin: she went home in high spirits, and for some time afterwards I heard most favourable accounts of her progress, gaining daily in flesh and strength; in fact, I flattered myself the injection would establish a cure. The cessation of the secretion, however, was of but short duration; the sac began to refill after a lapse of three weeks, and in November, 1863, she visited me, in Manchester, larger in dimensions than on her former visit. It was therefore evident that in this case, at least, there was no reliance whatever to be placed on injection; the extirpation of the tumour was therefore decided upon. Before proceeding further with this case, I would remark that there is not nearly so much danger to be apprehended from tapping and injecting ovarian cysts with iodine tincture as is generally supposed (that is, if proper precautions be adopted), nor is the danger increased by using large quantities of the tincture, and which to many of my medical friends appears so hazardous. It must be borne in mind, that in many cases where this operation has proved fatal, it in all probability arises from the fact that the canula of the trocar has slipped out of the walls of the sac, and the iodine tincture has escaped into the peritoneal cavity, bringing on a more dangerous character of inflammation, and exposing the patient to the mischief of iodine absorption into the general system. But if care be taken that the canula be kept from being drawn forward, but rather pressed backwards towards the sac, so as to secure and maintain its position within the walls of the sac until the whole process of injecting is over, the danger will be comparatively trifling; in fact, the inflammation is, when properly managed, *confined* strictly to the inner surface of the wall of the sac, and is always easily subdued by active fomentations (rendered very hot and stimulating by the addition of turpentine), without requiring either leeches or lancet, both of which I studiously avoid. With regard to any danger from absorption of iodine, I consider such could only arise from carelessness, as I before stated, by the slipping of the canula out of the sac walls, and giving rise to peritoneal inflammation as well as to absorption of the tincture into the system, both of which will be easily avoided by confining the contact of the fluid to the interior of the sac only. When the slender connexion of the pedicle of ovarian tumours is considered, and the entire absence of absorbent vessels in such tumours, I conceive it almost impossible that the patient should suffer from the effects of absorption of iodine, from the inner surface of the sac. It is, in fact, this want of absorbing power in these tumour masses, that renders the treatment of ovarian disease by medicine so thoroughly useless. From these remarks, it will be easily perceived why I venture to use such large quantities of iodine tincture for injection: First, because small quantities will fail to secure the object in view, either not rousing the inflammatory action at all, or, being in small quantity, the inner surface of the sac is not wholly covered, consequently the inflammation is only in patches, leaving other portions of the surface in an active state of secretion unchecked, therefore defeating the intention of cure. Secondly, because I have no fear of the effects of absorption into the system, believing this to be next to impossible if the fluid be kept from the peritoneal cavity, and into which it could only escape from carelessness. I may then lay it down as a rule, Better not inject at all, than do it carelessly. I do not believe, however, under the best endeavours, that injection will always succeed, but it has succeeded in some cases, and I believe, may do so to about 70 per cent.; and if so, as it does no great harm even where it does not succeed, I think it is worthy of trial: but only in those cases where there is positive evidence of *one* large cyst, entirely free from any solid deposits; *then*, and *then only*, is it admissible, and in proper cases when carefully selected it may succeed. There is one more popular error to be exposed in the tapping of ovarian tumours, and that is the assertion that adhesions often occur where the trocar pierces the peritoneum and the sac; a more absurd idea could scarcely be entertained; *a moment's reflection* will expose the fallacy. The two points of puncture,

first that in the peritoneum, and secondly that in the sac, are, when the sac is emptied and collapsed, *as far from each other as they can possibly be*, and cannot come in contact again until the sac has refilled to the same dimensions, and long after the process of inflammatory action from such punctures has subsided. In fact, adhesions do not arise from any such cause; at least, I cannot conceive such possible, nor have I seen such results in any of my operations. Adhesions arise from two inflamed surfaces being kept together for a time, that is, sac, or tumour surface, lying closely packed to the peritoneum. As a rule sacs are seldom adherent, and it is only those that come under the operation of tapping. Solid tumours are frequently adherent, and generally confined to those which have but little mobility. In consequence of the failure of injection, in the case of Mrs. Meakin, she visited me early in November last year, 1863, and was prepared for the extirpation of the sac. I operated on the 14th of November; the incision was about eight inches in length; some slight adhesions had to be contended with; the tumour with its contents about twenty-eight pounds; the incision closed by six interrupted sutures; and on the patient being put to bed, three grains of solid opium were given. During the first three days she was kept well under the influence of narcotics, but after the first day I substituted nepenthe for opium (a preparation of opium somewhat similar to the black drop). My reason for this was that it sat more quietly on the stomach, producing less nausea, and the sleep being less disturbed by dreams. The dose, from thirty-five to forty drops, was seldom followed by headache. I have since used this preparation extensively, and found it, I think, more decidedly effective than opium, and accompanied by less unpleasant effects afterwards.

On the third day a smart attack of peritoneal inflammation set in, and active fomentation was persevered with until inflammation was entirely subdued, without any depletion by leeches or lancet, neither of which have I ever used since I commenced operating on these tumours in 1842. From the termination of the attack of inflammation to the thirteenth day, the progress of the case was most satisfactory; all the interrupted sutures were taken out on the fourth day, and the incision healed by the first intention; the pedicle ligature came off on the 5th of December, twenty-one days from the date of the operation, she left Manchester for home, in the hope of enjoying her Christmas amongst her friends, and in which she was not disappointed. I have frequently heard from her since, to the effect that her health and strength gained ground daily, and that she proved a wonder to her friends, who had previously concluded her case to be hopeless. This patient is at present in the enjoyment of excellent health.

Case 110th.—Mrs. C., of Cheetham Hill, Manchester, aged twenty-nine, married; has had no children; consulted me in May, this year, having been advised by different medical men whom she had consulted, not to seek an operation. I found her very large, the abdomen tensely filled with a multilocular tumour springing from the right ovary; one-third of the mass, situated on the right side, very solid, unyielding, and without fluctuation, whilst on the left there was a large cyst, pressing upwards very much against the stomach, and rendering it very uncomfortable; the liver suffered also from pressure of the solid mass on the right side. I pronounced the case, in many respects, a favourable one for the operation, but she resisted me two or three times, and consulted others before her mind was fully made up to meet it. On the 1st of July I visited her at her own home, and learning from me that tapping was not advisable as a curative measure, and could only be sanctioned on condition that extirpation must of necessity follow in two or three days after (the object being simply to reduce the size of the tumour, and render the operation a little less formidable); in consequence of this announcement I tapped her on the 1st of July, the fluid drawn from the sac amounting to eighteen pounds, thus affording her considerable relief, and enabling her to move about with greater activity; she enjoyed her food more, and the stomach was much less irritable. On the 5th of July, having removed her to her parents' house, nearer to me than her own residence, I operated in presence of Messrs. Edlin, Paton, and Walsh, surgeons; the length of the incision

was about eleven inches, and through which with difficulty the solid mass was got away. As I had anticipated, there were no adhesions, but the pedicle was very thick, involving the entire broad ligament. Under these circumstances (as I usually do), I pierced the broad ligament, and passing ligatures round each half, tied them separately. This mode of placing the ligature on the pedicle has its advantage, from the fact of the ligature being thrown off much sooner, and the recovery hastened; and again the system does not rally well towards improvement until the suppuration of the pedicle has ceased, and the ligature is thrown off. My pedicle ligature is always composed of three or four strands of Indian hemp. The abdominal incision was secured by nine silver sutures, and the patient placed comfortably in bed in fifteen minutes from commencing the operation, including the application of chloroform. The weight of the solid mass was sixteen and a half pounds, and with contents thirty-five pounds. Three grains of solid opium were given, and repeated occasionally, during the first three days, amounting to about fifteen grains. After that she had the nepenthe in doses of from thirty-five to forty drops. This narcotic influence was kept up for the first five days, more or less, according to circumstances. For the first three days the urine had to be drawn away by the catheter, after which it passed naturally. On the third day the silver sutures were all removed, and the large incision entirely closed, with the exception of the aperture through which the ligature passed. After the third day enemas of broth, with castor oil, were occasionally administered, with the double object of moving the bowels and affording some little temporary support to the system, at a time when the stomach was too irritable to retain food easily, except of the most simple kind, such as gum-water, toast-water, thin milk, &c. Up to the eve of the eleventh day there was not the least peritoneal excitement or tympanitic state of the abdomen; the progress of the case had, up to this, been most satisfactory. But the twelfth day was commenced with diarrhœa, attended with considerable griping and puffiness of the abdomen; in other words, although the case had escaped peritonitis (which is the general feature more or less), here for the first time in my connexion with these operations the mucous membrane had taken on inflammatory action. This attack, however, gave way without much trouble to grain doses of opium every four or six hours, with active fomentations of turpentine and hot water.

I had some reason to suppose this excitement arose from the patient having taken some Dublin porter, but which was of course discontinued. On the 12th day the ligature of one half of the pedicle came off, the other followed in three or four days afterwards. My patient sat up on the fourteenth day, and began to take her food (with a relish) consisting chiefly of mutton chops, beef tea, &c.; and from this time her progress was rapid towards complete recovery. On the 3rd of August, or just four weeks from the operation, she left Manchester for the country, quite well. Thus successfully terminated my last or 110th operation. My general plan of treatment after operation, as well as the mode of operation, has been too often published previously to require repetition here; but I will in conclusion give the tabular results of all my abdominal operations for the last 22 years, that is, from 1842 to 1864.

Cases from 1842 to 1864.	Number.	Recoveries.	Deaths.
Ovarian Extirpations	110	76	34
Cæsarian section	1	0	1
Entire removal of uterus and appendages, (being fibroids) through the parietes abdominales	3	1	2
Cutting down upon ovarian tumours (in cases of almost universal adhesions), to reduce their bulk from within, and setting up ulceration where removal was impossible	4	4	0
Cysts tapped and injected with tincture of iodine	6	4	Two filled again.
Total Results	124	85	37

It will be here seen, that in my practice of twenty-two years, the recoveries in operations of ovariotomy alone, are about seventy per cent.—a result that I cannot but consider flattering. The reader will also observe, that during the above period I have extirpated three fibroid uteri with their appendages entire, through the abdominal parietes, as in ovariotomy. These I believe to be the first operations known of the kind. The first and second operations were in 1844, and the last in January, 1863, and reported in the London Obstetrical Society's Transactions, vol. v. I mention these particularly, because Professor Koeberl operated similarly on a fibroid uterus on April 20th, 1863, and stated his to be the first operation of the kind in the world; whereas my first and second operations were nineteen years, and my last some months, prior to his operation. The fibroid in my last case weighed eleven pounds.—*Glasgow Medical Journal, October*, 1864, *p.* 257.

106.—ON THE DIAGNOSIS BETWEEN OVARIAN DROPSY AND ASCITES.

By Spencer Wells, Esq., Surgeon to the Samaritan Hospital.

During the past summer the question as to ascites, ovarian dropsy, or the coexistence of the two diseases, arose in several cases; and we are indebted to Mr. Spencer Wells for the following practical remarks upon the means of recognising each of these forms of disease:—

Our senses of sight, touch, and hearing are all required to assist us in distinguishing ascites from ovarian dropsy, the physical diagnosis being established—1, by inspection and measurement; 2, by palpation; and 3, by percussion and auscultation.

1.—On *inspection*, the *size* of the abdomen is seen to be increased both in ascites and in ovarian dropsy; and when an ovarian cyst is large, the abdominal enlargement is general; as it is in ascites. But while the cyst is of moderate size, the abdominal enlargement is often partial, more to one side than the other, more below the umbilicus than above. In *form*, the flanks and sides of the abdomen protrude in ascites, the front not being more convex than in the natural state, or it may be flattened; while in ovarian disease the bulging is generally most evident in front, less so at the sides, and often more on one side than the other. When the different portions of a multilocular cyst can be seen, of course all doubt is dispelled, but these remarks apply to simple cysts only. Alterations in position generally produce a greater and more immediate change in the form of the abdomen in ascites than in ovarian disease, the free fluid gravitating much more readily than a cyst can move. The normal depression of the *umbilicus* is altered whenever the general abdominal enlargement is considerable both in ascites and ovarian dropsy; but in the latter disease, although it may be flattened as in pregnancy, it is never prominent and bulging as it very often is in ascites, or when ascitic fluid surrounds an ovarian tumour. The *superficial veins* may be enlarged from the lower part of the abdomen to the chest on one or both sides in either disease. This appearance only assists in diagnosis when the enlargement is much more evident on one side than the other. Such undue importance has been given to enlargement of the superficial abdominal veins as a distinction between ascites and ovarian dropsy, and between simple and malignant tumours within the abdomen, that the following facts should be recollected:

Congestion of the epigastric veins, when seen merely as a fine network of capillary veins, is usually a simple result of absorption of the cutaneous fat, the vessels becoming visible through the thin and distended skin, and of no diagnostic value. When some of the larger veins, distended or varicose, are seen to run from the inguinal region upwards, and either cease abruptly in the middle of the abdomen, or run to the hypochondriac region, or even up to the clavicles anastomosing with branches of the mammary and intercostal veins, the impediment to the circulation may be of several kinds.

It will be remembered that the epigastric veins are part of the system of the

inferior cava. They are usually very small. They begin in the subcutaneous cellular tissue by small radicles, which unite on each side of the body to form two branches—one, small and internal, runs up on each side of the middle line—one larger and external, can be traced upwards to the side of the thorax and to the axillary vein. These two branches unite on each side close to Poupart's ligament, and open into the internal saphena vein at the spot where this vein and the femoral vein unite. The superficial fascia lies between these veins and the recti musles.

The causes of stagnation or retardation of blood in the epigastric veins are—

1. In the *heart* itself, impeding the return of blood from the cava.

2. In the *trunk* of the inferior cava—(*a*) tumours pressing the vein towards the vertebral column; (*b*) coagula within the vein; (*c*) extension of cancer to the vein. The varicosity of the veins of the lower extremity is often prevented by a free collateral circulation being set up by dilatation of the vena azygos.

3. In the larger *branches* of the inferior cava—(*a*) pressure of tumours, Cruvelhier has seen both iliacs obliterated by the pressure of an ovarian tumour, and the epigastric veins dilated to the thickness of a goose-quill running to the axillæ. (*b*) Coagula; (*c*) pregnancy.

4. In the *Portal System*. As the hemorrhoidal branches of the inferior mesenteric vein (one of the veins forming the portal system) unite in the hemorrhoidal plexus with the hemorrhoidal veins which run into the internal iliac vein, there is a free communication between the portal and general venous systems. By throwing injections either into the vena porta or the pelvic veins, Cruvelhier has proved that the pelvic, hemorrhoidal, internal pudic, obturator, and gluteal veins anastomose very freely. So that mesenteric and other tumours, which may, perhaps, not cause direct pressure on the cava, may lead to dilatation of the epigastric veins secondarily, by their effect on the vena porta or its branches, just as any disease of the liver which leads to impeded portal circulation may have the same effect.

When the integuments are œdematous, the lineæ albicantes become more prominent than the neighbouring portions of skin, and have a knotty appearance, which has led to the mistaken appellation of *varicose lymphatics*. Nearly all the cases in which I have observed this appearance have been cases of tumour surrounded by ascitic fluid. The *movement* on respiration is defective, both as regards the soft wall of the abdomen and the lower ribs; while the respiratory movements of the upper ribs are exaggerated in both diseases. The alteration in movement only assists in diagnosis when it is partial or affects only one side. On making deep inspirations the upper part of an ovarian cyst may often be seen to rise and fall. This appearance is very characteristic. In ascites it may be simulated by some distended coils of intestine moving with the diaphragm; but the resonance of the intestine on percussion instantly settles all doubt on this point.

On *measurement*, the enlargement of the abdomen in ordinary ascites is equal on both sides, or symmetrical; and, although the distance from the sternum to the pubes is increased, the umbilicus retains its normal position —about an inch nearer to the pubes than to the sternum—and is about on a level with the highest point of the crest of the ilium on each side and midway between these two points. In ovarian dropsy there is often a considerable alteration in the measurements between the umbilicus and sternum, and umbilicus and pubes, as well as between the umbilicus and the two cristæ ilii. In ascites the greatest circular measurement is at the level of the umbilicus; in ovarian dropsy it is often some inches below that level.

II.– On *palpation*, the abdominal wall is felt to be harder and more resistant than natural in both diseases in the parts where much fluid is present, but is soft and elastic in other parts. Consequently the variation in the seat of hardness with the position of the patient becomes useful in diagnosis, the fluid in ascites gravitating freely to the most dependent part. *Fluctuation* is perceived with varying distinctness according to the degree of tension of the *abdominal* wall, to the thickness of the layer of fat, to the amount of œdema,

to the thickness of the peritoneum or of the cyst, to the quantity and character of the fluid, and to the amount of tympanitic distension of the intestines. In itself, it offers no assistance in diagnosis, because a thin-walled ovarian cyst, filled with limpid fluid, with a moderately tense and thin abdominal wall, would give a more quick and decided wave than a moderate quantity of ascitic fluid beneath an abdominal wall thickened by fat or subcutaneous œdema. The characteristic peculiarity of the fluctuation in ascites is that it varies with the position of the patient, and is only perceived in the parts where the fluid gravitates towards the abdominal wall; while in ovarian dropsy its situation does not vary with position, but is perceived wherever fluid is to be discovered by

III.—*Percussion.*

The two following diagrams represent the situation of clear and dull sounds obtained by percussion in typical cases of ascites and ovarian disease, the patient lying flat and evenly on her back. The dark parts of the abdomen are dull, the rest clear. In ascites the stomach and intestines are above and in front; the fluid behind and on either side. In ovarian dropsy the fluid is in front, extending in different degrees to either side, and pushing the stomach and intestines upwards and backwards, just as a gravid uterus does. The figure to the right, indeed, would represent either a gravid uterus near the full period or pregnancy, or an ovarian cyst of about the size of such a uterus, and situated centrally, as ovarian cysts often are at this or rather a later period of their growth. But quite as frequently they tend towards one side or the other, in such cases the diagnosis being, of course, easier.

It is seldom that a patient with ascites lies so flat as not to raise the shoulders enough to throw a layer of fluid downwards towards the pubes. Very often the dulness may extend as high as the umbilicus, and generally does so when the shoulders are much raised by pillows. This might lead a superficial observer to suppose that the disease was ovarian, because there was a dull sound in the front of the abdomen; but on lowering the shoulders, and placing a pillow or bassock under the hips, the fluid at once gravitates towards the diaphragm, the intestines float to the surface, and a clear sound is obtained when it was dull before. No such alteration in the position in the situation of dulness can possibly occur in ovarian disease. So on turning from side to side, the fluid gravitates to the side which is low, and the intestines rise to the upper side, with corresponding changes in the situation of dull and clear sounds on percussion. No such alteration can occur in ovarian disease. Again, at any spot near the level where the resonance of the intestines ends, and the dulness of the fluid begins, and a dull sound is elicited by *gentle* pressure and percussion, a *deeper* pressure will displace the fluid, and the resonance of the intestines will be again heard. At the most depending spots the amount of pressure necessary to obtain a clear sound is some guide to the estimation of the thickness of the layer of fluid. No such difference in the sounds from superficial and deep *percussion* can occur in ovarian disease.

It has been supposed that percussion on the loins is a very sure guide in diagnosis,—that when the patient is sitting up and one loin is clear and the other dull the disease is ovarian, but that when the sounds are the same on both sides it is ascites. One dull side is also supposed to be a proof that the ovary of that side is the one diseased. But there are so many exceptions to these rules that they are of no great value, except as corroborating or counterbalancing other physical signs. Auscultation affords little information, but it shows the presence of the gurgling sounds of the intestines in the spots clear on percussion, and the absence of these sounds in the dull spots, except on deep pressure by the stethoscope. In both diseases the fluctuation wave of fluid may be heard as well as felt.

By applying these general rules in any ordinary case, a few seconds will enable the Surgeon to clear up all doubt. But there are various conditions which may lead to the necessity for further examination. The fluid in the peritoneal cavity may be so large in quantity that the front of the abdomen is pushed far beyond the reach of the intestines. They float as far as the mesentery will allow them, but cannot reach the surface of the abdominal wall. In this case percussion must give a dull note in front just as it does in ovarian dropsy. So when the intestines are fixed in the back part of the abdomen by adhesions, or by a thickened omentum, the fluid is circumscribed in front as in ovarian dropsy. Or an ovarian cyst may contain air, either from a perforating communication with intestine, or after tapping and decomposition of fluid, in this case percussion giving a clear note in front or above, and a dull note behind or below as it does in ascites. In these cases physical diagnosis alone cannot solve the doubt, and we have to consider all that can be learned from the history of the case and the general condition of the patient. So, when fluid is free in the peritoneal cavity we must resort to tapping and chemical or microscopical investigation before we can decide whether the fluid is the ordinary non-inflammatory serum which transudes into the cavity in cases of heart, liver, or kidney disease, or the inflammatory exudation of chronic peritonitis in its simple or its tubercular form, or whether it may be ovarian fluid which has escaped from a perforated or ruptured cyst. And when ascitic fluid surrounds a tumour which can be felt either by deep pressure or after tapping, we have to determine whether this tumour is ovarian or uterine, or some form of intra-abdominal cancer, or some hydatid tumour of peritoneum, omentum, or liver; or some tumour of liver, kidneys, or pancreas; or some retro-peritoneal tumour—aneurism, enlarged mesenteric or lumbar glands, enchondroma, cancer of vertebræ, and so on. But these are all beyond the scope of remarks upon the physical diagnosis between ordinary ascites and a simple ovarian cyst of large size.—*Medical Times and Gazette, Sept.* 24, 1864, *p.* 327.

107.—PRACTICAL DETAILS IN OVARIOTOMY.

By T. Spencer Wells, Esq., London.

[This short article is elicited by one from Mr. Furneaux Jordan. Mr. Wells has now discontinued the use of hare-lip pins, as it was impossible by them to bring the opposite divided surfaces evenly into apposition.]

I began to use wire sutures of different kinds—silver, platinum, and iron—as well as catgut, horsehair, india-rubber, and gutta-percha; but I found nothing answer so well as fine, strong, pure silk. It is easily applied, easily removed, and, if not drawn too tight nor left too long, causes no sloughing of the circle of tissue which it encloses. I seldom leave the sutures more than forty-eight hours, and often remove them at thirty-six or forty. Dr. Keith, of Edinburgh, has recorded one case in the Edinburgh Journal for October, 1863, which so strikingly shows one of the great advantages of silk over wire, that I make no apology for asking you for space for his account of the *post mortem* examination. He says:

"*On carefully* examining the line of incision, which had been secured by

seven wire sutures passed through the whole thickness of the abdominal wall, including nearly half an inch of peritoneum, it was observed that the peritoneal line of union was so uniformly perfect that it was impossible to tell where the line of incision had been. The wires were felt under the peritoneal surface; and on cutting one of these from the outside, in order to see how the cut extremity of the wire would behave when passing over this freshly united serous surface, I was surprised to find, though it was withdrawn with the utmost gentleness, and the point kept as much as possible against the upper surface of the wall, that the *point of the wire tore the peritoneal membrane right across*, leaving it ragged, and allowing a *drop of matter* which lay along the track of the wire to *appear on the peritoneal surface*."

It is the fashion to sneer at "petty details;" but a fact like this shows that it may be of immense importance to a patient whether her wound has been closed by wire or by silk.

The question, "what to do with a short pedicle," is one of the most important which can engage our attention; but it can only be determined by the experience of a large number of cases accurately observed and faithfully recorded.—*British Medical Journal, June* 18, 1864, *p.* 676.

108.—ON THE USE OF CERE CLOTHS IN THE TREATMENT OF PAINFUL AFFECTIONS OF THE FEMALE BREAST.

By Dr. Hardy, Dublin.

I mentioned having applied cere cloths to relieve the distension of the breasts in one of the cases detailed in the foregoing paper. Made as they usually are, of bees-wax and sweet oil, spread on linen, they are ill suited for the purpose for which they are intended; they become very brittle and crumble about the patient's bed. In preparing them, if a little glycerine and resin plaster be added to those ingredients, and spread on thin leather instead of linen, they can be removed and reapplied without any inconvenience.

℞. Cer. flav.; ol. oliv. aa. ℥ii.; glycerine ℥ii.; emp. resin, ℥ss.—M. ft. emplast.

Cere cloth with Belladonna:

To the above add emp. belladon. ℥ss.

Cere cloth with Opium:

To the cere cloth plaster add emp. opii. ℥ss.

By spreading over the surface occasionally, glycerine diluted with tepid water, this application may be made to answer the purpose of a perpetual vapour bath on the breast, and thus prevent the often injurious handling and rubbing which it is frequently subjected to from the nurse, or save the risk of cold in fomenting.

By saying so I do not mean to detract from the very beneficial effects of both hand rubbing and fomentations, when properly applied. It is their injudicious and wrong application which I condemn.

Cere cloths are very soothing in painful affections of the breasts of very young infants. I lately applied them in a case of inflammation and enlargement, where the child was about a fortnight old; both breasts were greatly distended. The nurse, when showing them to me, said she had not squeezed them until she first let me see them. No other means were resorted to, and in a few days the disease was quite removed.

In painful and irritable conditions of the breast, either on the setting in of lactation or owing to disease, the addition of extract of belladonna is sometimes most beneficial. It may be spread over cere cloths made as I have described, and in this way be rendered more efficacious; as then it will be prevented from drying on the surface. In two cases particularly I found the application of belladonna most valuable.

In March, 1852, I attended a lady in her first confinement; she was remarkably healthy, and had a good labour. Her breasts were well formed, the nipples very good, and without any trace of tenderness on the surface;

but so exceedingly sensitive that the least touch, in endeavouring to nurse her infant, was intolerable.

The extract of belladonna was smeared over her breasts outside the margin of the areola; one application left on for a short time (less than an hour) removed every trace of irritation. She has given birth to several children since then, and never again had any such annoyance.

I took the precaution, which is very necessary in all such cases, of strictly removing the infant from its mother until after the belladonna had been some time very carefully washed off the breasts.

The second case was a lady who was confined of her first child in November, 1856. During her labour, until she inhaled chloroform, she was most violent. She then became so quiet, her husband, wondering at the sudden return of peace, looked into the room. On perceiving him she said: "Dick, I don't mind it now, I have the chloroform."

On her breasts becoming distended at the commencing of lactation she suffered such intense pain that I was sent for during the night. She was in a great state of excitement.

In addition to internal remedies I smeared her breasts freely with extract of belladonna.

Next morning I found her perfectly tranquil. She said (pointing to her breasts):—"Here they are, like two stones; but not in the least painful;" nor were they from that time. This lady and her sister had the same peculiarities; their breasts became intensely hard, but without any secretion of milk whatever. Neither of them could nurse.

The greatest degree of this peculiar sensitiveness I ever met with was in vaccinating two gentlemen, brothers, both strong healthy men. Neither of them could bear the least touch of the lancet without exclaiming. One of them in particular became deathly pale; the perspiration poured down his face, and he had to be laid fainting on a sofa. Wine and smelling salts were freely used before I could complete the operation, which occupied not less than twenty minutes.

It was only necessary to scratch the skin slightly with the nail to cause the same effect as that excited by the lancet.—*Dublin Quarterly Journal, Aug.* 1864, *p.* 217.

109.—ON THE EMPLOYMENT OF APIOL IN AMENORRHŒA AND DYSMENORRHŒA.

By Dr. Corlieu.

[Apiol is the active principle of parsley. A work has been lately published by Dr. Marrote of the Hospital La Pitié, on the utility of this principle. Dr. Corlieu has now employed it for eight years, and in this paper points out the cases in which it may be expected to prove useful.]

A. In all cases where the menstrual disorder depends upon the derangement of a vital element, where there is plethora or anæmia, apiol should not be used, for, being a nervous tonic, it will only aggravate the condition of the patient. But if the condition of chlorosis be removed, apiol may be prescribed with a good prospect of success. The following case will illustrate this:—A lady, thirty-eight years of age, of a lymphatic and nervous temperament, had suffered for three months from amenorrhœa, complicated with extreme chlorosis. Dr. Galligo at first ordered apiol, but without success. At a later period he combined it with chalybeates, which had previously done no good. The combined use of iron and apiol effected a cure. Dr. Marrote relates the following case:—Miss C., eighteen years old, was of a lymphatic temperament. In childhood she had had measles, hooping-cough, and modified smallpox. Her skin was of a dead white, her face somewhat swelled; the gums were swelled and discoloured; she had very little appetite, and often vomited her food. She menstruated first when fourteen years old; for several months the discharge was white, afterwards it became of a reddish colour, but was accompanied by such severe uterine

pains, that she was obliged to keep her bed. As she was to menstruate on the 18th of October, I ordered her two capsules of apiol on the 15th, two on the 16th, and two on the 17th. On the 18th, the menses appeared, though still in small quantity, but unaccompanied by colics or uterine pains; they only lasted two days, and the blood was still very pale. On the 21st, I ordered a chalybeate which was continued till the 18th of November, when she resumed the apiol for three days. The menstruation was unaccompanied by pains; it continued three days, and the discharge was more coloured and more abundant.

B. When the menstrual disorder depends upon a diathetic condition (dartres, scrofula, &c.), we must, by means of a specific treatment, such as bitters, cod liver oil, preparations of iodine, sulphur, or arsenic, attack the principal malady. Apiol is of no use at first in these cases; but when the cure of the morbid diathesis has been effected, it may be employed with advantage in stimulating the torpid menstrual function.

C. But it is chiefly in disorders which are under the influence of the nervous system that apiol is a heroic remedy, leaving far behind it all the emmenagogues hitherto employed. As a neurotonic it supplies to the nervous system the energy it has lost. Change of life, of habits, or of climate, often determine amenorrhœa. This is a fact which must not be forgotten, and which is well known to the physicians of boarding-schools and religious houses. This menstrual suppression is transitory; it lasts some months, and sometimes only gives rise to slight nervous disorders, or a slight oddity of character. In these cases, two, four, or at most six capsules of apiol will restore the menstrual flux. The following case was observed by Dr. Marrote:—

Miss L. J., seventeen years of age, born in London, arrived for the first time in Paris in October, 1861. She was a tall, handsome girl, of good constitution and plethoric temperament; she had never had any serious illness. She menstruated for the first time at thirteen, but was very irregular up to fifteen. Although she never suffered from colics, or lumbar or inguinal pains, her periods were often delayed for a fortnight or three weeks; the blood, containing fibrin, was pretty abundant. From fifteen to seventeen years, menstruation was quite regular. She then came to Paris to complete her education. From the date of her arrival until the month of March of the following year, her menstruation was completely absent. Nevertheless, no change had been observed in her general health, though I was informed that at times she became duller, more melancholy, and more irritable than usual, and that this state continued for a short time. Taking this as an indication, I administered apiol in the dose of one capsule night and morning in a spoonful of water, beginning on the 11th of March, which seemed to be indicated as a menstrual period. She thus took six capsules in three days. On the 14th of March, the menses appeared without pain, and lasted four days. The lady of the establishment, accustomed to see apiol almost always succeeded under these circumstances, did not repeat it the following month. On the 14th of April, the menses returned, and lasted four days. On the 17th of May, the 16th of June, and the 21st July, the same occurred. Miss L. J. returned to her family in London in perfect health and quite regular.

The sudden application of cold during a menstrual period may suppress the discharge abruptly, and give rise to amenorrhœa, which may last for an indefinite time. In the month of January, 1861, I saw a young lady, seventeen years of age, who had menstruated for two years, but in whom, in consequence of a chill during menstruation, the flow was suppressed. The belly became considerably enlarged; there was, in fact, an ascites, which could only be explained by the amenorrhœa. There was no albumen in the urine. I employed, without success, purgatives, sudorifics, chalybeates, and the ordinary emmenagogues. There was considerable pain at what should have been the menstrual periods. This state continued until the end of April. In May, capsules of apiol given night and morning restored the discharge,

though at first it was pale and serous. Iron was continued, and from that time the abdomen diminished in size. The cure was complete.

It would be easy to bring forward more cases, but the above may suffice. The point I wish to establish is, that apiol is the best emmenagogue with which we are acquainted in all cases where amenorrhœa or dysmenorrhœa have their origin in a disturbance of the nervous element. The principal condition for success in the use of apiol is in the choice of the proper moment for its administration. In almost all cases of amenorrhœa or dysmenorrhœa which depend upon an organic cause, the use of apiol is contraindicated. This is not the place to lay down the differential diagnosis of these conditions. If apiol has succeeded in some cases of plethora, it has been because the plethora was not very considerable. "In order," says Dr. Marotte, "that apiol may succeed, it is an essential condition that the pain which accompanies menstruation depend upon dysmenorrhœa, properly so called, that is, on the vaso-motor innervation of the womb. It has never succeeded in calming nervous pains, dull or acute, which were seated in branches of the lumbo-sacral nerves, and especially in the uterus, pains which appear or become exaggerated at the menstrual period, and may, at first sight, simulate dysmenorrhœa proper." Another condition for success in the use of apiol consists in choosing a time for its administration corresponding to a menstrual period. If the woman has not properly calculated the period, we may be enabled to discover it by noticing the sympathetic derangements which occur under these circumstances.—*Gazette des Hôpitaux.—Edinburgh Medical Journal, August*, 1864, *p.* 164.

110.—ON THE INTERNAL EMPLOYMENT OF ESSENCE OF TURPENTINE IN THE HEAD-ACHES OF NERVOUS WOMEN.

By Professor Teissier, of Lyons.

M. Teissier thus describes the kind of cases of nervous head-ache in which he has found the essence of turpentine to be beneficial. The affection, he says, is a common, but often very severe one, and should not be confounded with ordinary neuralgia, either periodic or irregular, of the face or cranium, or even with hemicrania. This cephalæa is characterised by a much more fixed and continuous pain in the head, and may last not only several weeks, but months, and entire years, without presenting more than rare and slight intermissions. The pain is sometimes dull, sometimes shooting, and sometimes pulsative, occupying only a single point of the head or the whole of the cranium, being accompanied by nausea or even vomiting, and complicated besides with much more serious symptoms, such as vertigo and tendency to syncope, inability to think or to work, despondency, weariness of life, and sometimes numbness in the limbs. It is especially observed in nervous women, with exalted sensibility, of a delicate constitution, somewhat anæmic, and especially hysterical. It often coexists with dysmenorrhœa, amenorrhœa, and also with a tendency to excessive menstruation, although it is sometimes observed in persons of good constitution whose menses are regular. M. Teissier observes that many remedies already exist which are efficacious in this complaint, such as valerian, assafœtida, the ethers, cyanide of potassium, aconite, &c.; and more particularly those which improve the blood, as chalybeate medicines, and different mineral waters. But these means sometimes fail, and then the essence of turpentine may be employed with advantage; although M. Teissier does not assert that it is infallible in its operation. It has been employed in the same kind of cases by Dr. Graves and by Trousseau; but M. Teissier does not think it necessary to prescribe it in such large doses as those physicians have done. He recommends its use in capsules, given at meal-times, each capsule containing eight drops of the essence.—*British and Foreign Medico-Chirurgical Review, July*, 1864, *p.* 245.

III.—ON THE CLINICAL EXAMINATION AND DIAGNOSIS OF A TUMOUR OF THE BREAST.

By Thomas Bryant, Esq., Assistant-Surgeon to Guy's Hospital.

In examining a breast, with diagnostic intentions, the surgeon should take the whole gland in his hand; he should manipulate it gently and in every part with his fingers and thumb; and by these means he will, if an isolated tumour is to be found, in all probability detect its presence; if, however, he be uncertain upon this point, he should make the patient lie down, and if a new growth then really exists he will at once discover it. "If a patient be sitting or standing, and the breast is grasped by the finger and thumb, when induration of the gland itself exists, a sensation is felt as if a tumour were present. If, now, the palmar surface of the fingers be pressed flatly against the chest in the same part, nothing remarkable is distinguishable. If a tumour or new growth exists, however, it is immediately perceptible. But if any doubt arise in the matter, the patient should recline when under examination; and then, if there be a tumour, it is immediately manifest to the touch, and often to the eye."

Having, then, detected the presence of a tumour, that is an independent growth, developed in the neighbourhood of the breast-gland, and probably in connexion with it, the question arises as to its nature. Is it a simple tumour, or is it a malignant one?

If the tumour be movable and hard—if it be quite free, or if it has but an uncertain connexion with the gland structure—and there is a strong probability that the tumour is of a simple nature; and, if it has existed for several months, this probability becomes stronger, for it is an early characteristic of the *cancerous* tumour, even when primarily developed as a tuber, or as an independent structure, to associate itself, and to become connected with the neighboring tissues; and if this has not taken place, the absence of these conditions enhances the probability of the simple nature of the growth under examination. If the patient be also young and healthy, and if no other abnormal conditions, either of the breast or neighbouring structures, are to be detected, the probability becomes a certainty, and the presence of an "adenocele" may be determined on.

For these *adenoceles*, as a rule, appear in the young and unmarried; in the comparatively healthy and robust. They appear during that period of life when the procreative organs, and amongst them the mammary glands, are in a state of "developmental perfection;" and when attacking the married woman, they most frequently are developed during pregnancy or suckling. They are never associated with any other symptoms than such as can be produced mechanically by their presence, they never involve the integument except by distension, and the skin is never infiltrated by any new material. They are never accompanied by any secondary enlargement of the absorbent glands, nor associated with any secondary deposits; they never cause any cachexia, nor undermine the health of their possessor, they affect the patient solely through local influences, and demand treatment chiefly from local considerations.

For although the breast itself may be much pressed on, or even expanded over the tumour, it will still exist, and on careful examination, its presence, as a rule, will be made out. The nipple, although flattened from the extreme glandular expansion, can still be seen, but it is hardly ever retracted. The integuments may be stretched to an extreme point, yet they will still be movable and sound, although some inflammation from over-distension may have made its appearance, and large veins are always to be observed meandering in the healthy tissue. The tumour, if solid, may appear lobulated, and, if containing cysts, fluctuation may be detected. Still, the disease is essentially a local one, and affects the patient through purely local considerations.

The Diagnosis of a Tumour which is evidently caused by some partial or general Enlargement or Infiltration of the Mammary Gland.—Let us suppose,

however, that the surgeon has a case of disease of the breast before him, in which the structure of the gland is itself involved; that there is no independent movable tumour, such as we have been considering; but that it is evident on manipulation that the malady, whatever it may be, is intimately connected with the gland structure.

What is the case? Have we an inflammatory affection only of the organ, or have we some simple hypertrophy or innocent enlargement? Is it a simple disease, or is it a malignant one?

If the manipular indications of the mammary gland are those only of enlargement, is such a condition due to pregnancy, or is it the product of a simple hypertrophy, confining the meaning of that term to an excess of growth?

If the increase be due to hypertrophy, which, by-the-by, is an exceedingly rare condition, this has been to a certainty of a chronic nature, its increase has been slow, and its growth painless; it is simply characterised by an increase in size, aad beyond that can hardly be regarded as a disease; there is certainly no increase in action beyond that which growth demands.

[When due to pregnancy there need be little difficulty in the diagnosis. There will be a general fulness of the gland, and a darkening of the areola, and moreover both glands will be similarly affected. Sometimes a mammary enlargement may be explained by an inflammatory condition—not acute—but very chronic in its nature, and leading to effusion in the structure of the gland.]

In certain patients—that is, in the middle-aged—when cancerous affections are to be looked for, the presence of an indurated mammary gland, whether it be partial or complete, must always be regarded with suspicion, and in certain cases I believe it to be an impossibility to form any certain opinion as to its true nature. If the induration of the gland be the only symptom, and this induration be associated with a sharp pain, or even a dull one, either a simple chronic inflammation of the gland may be indicated, or the early condition of a cancer; under such circumstances it is as well to wait before giving any positive opinion; if, however, much time has already passed, say many months, and no other symptoms have made their appearance, there is some good ground for the hope that the enlargement may be due to inflammation, for infiltrating cancers are not generally inactive—as a rule, are not stationary—and soon give rise to other symptoms, such as some enlargement of the absorbent glands, although this may be slight, some slight dimpling or drawing in of the skin, an important sign; or some more marked symptom, such as infiltration of the integument. A retracted nipple is also a frequent accompaniment of a cancer, but this is only an accidental symptom, such as may be caused by several conditions, and is not by any means characteristic.

If, however, any one or all of these symptoms show themselves soon after the first appearance of the lobular enlargement of the mammary gland, an opinion as to the cancerous nature of the growth may be confidently expressed. If, on the other hand, none of these symptoms make their appearance, and the induration or infiltration of the lobes of the gland remain stationary, or show some tendency towards improvement, the probability of the simple character of the disease gains ground.

On the Development of Cysts in Tumours of the Breast.—In practice, many examples of tumours of the breast come under the observation of the surgeon, the diagnosis of which is much obscured by the presence of cysts, or rather what Mr. Birkett describes as capsules, containing fluid of divers characters; for the development of a cyst in the majority of tumours is a mere accident; it is not a new development, such as the more solid portion of the tumour, nor is it in any way to be compared with the simple cystic formations which are found in the neck or other portions of the body; it is to be looked upon as a collection of fluid, probably serum, more or less blood-stained, and it is by its gradual accumulation that the more solid growths are separated, *and an apparent* cyst is formed. But this cyst is only spurious, for it has

no special structure, its artificial walls being made up of condensed cellular tissue.

The existence, therefore, of false cysts, such as I have briefly sketched, in any of the breast tumours, whether adenoid or malignant, is to be regarded as a mere accident, for accumulations of serum are found to take place in any new growth, and thus may give rise to the cystic tumour.

The presence, therefore, of cysts in a mammary tumour has no weight in determining either the innocency or the malignant nature of the growth under examination; they are the product of a mechanical cause, and may consequently occur in either form; they are not special growths, nor are they of any intrinsic importance. The diagnosis of the tumour containing such cysts rests on other points, and more particularly on such as have been already indicated. As a rule, however, these so-called cysts are found in the less firm and solid forms of tumour, in those that contain less cellular or connective tissue, and in the more rapid developments rather than in the slow.

When found in the *adenocele or innocent growth*, the tumour will be more or less solid, but in the parts in which the false cysts or capsules exist the growth will be more lobulated and loosely connected; loose pedunculated growths will, in some instances, be seen lying within these capsules, their floating extremities being bathed with the so-called cyst contents; the different forms assumed by these tumours depending upon the amount of connective tissue which binds together the several lobes and lobules, and the dimensions of the interspaces which go to form the false cysts.

When these cysts are present in the *malignant tumour*, they are produced in precisely the same way as in the innocent; but as the formation of the former differs from that of the latter, the cystic contents will vary also, the false cysts in the one instance containing the more or less solid characteristic lobules of the adenoid growth, whilst in the other case they are filled with the less developed and more irregular, but equally characteristic, material which goes to build up the cancerous tumour.

On the Diagnosis of the "True Cystic Adenocele" of the Breast.—The remarks I have just made respecting the importance of cysts in the innocent and malignant tumours of the breast are not applicable to every case; for to this rule, as to others, there is an exception, and in the present subject the exception is to be found in that form of cystic disease of the mammary gland which differs from the other forms of adenoid or new growth—developed independently of, although allied to, the breast-gland itself, such as those already dwelt upon—for it is essentially a cystic disease of the gland itself, and is more particularly connected with its secreting ducts. It is, however, of an innocent nature, and pathologically is allied to the genuine adenocele; it is the tumour originally described by Sir B. Brodie as "arising by a dilatation of portions of some of the lactiferous tubes," or by Mr. Birkett as "Cystoid formations, distinctly referable to the dilatation of a duct, or to a connection with one, and containing growths which appear to spring from their walls." These growths are strictly analogous in their structure to the pedunculated or floating bodies which have been observed in the other forms of adenocele; containing, like them, a structure allied to the breast-tissue, and being composed of more or less distinct cæcal terminations of newly developed ducts, with variable quantities of true connective tissue.

How, then, is such a tumour to be made out? and what are the special symptoms which characterise it from the other forms of mammary tumour?

First of all, it is to be looked upon as an innocent tumour, and, consequently, it will be found to affect the patient precisely in the same way as all other adenoid tumours, in a purely local manner. It is to be regarded as a local disease, which at no period of its growth and in no way affects the patient otherwise than through local causes; it is never associated with secondary glandular enlargements or with secondary deposits in other tissue, as in the cancerous tumour, and we must therefore look to local symptoms to guide us in the formation of a correct diagnosis.

It is to be remembered that it is a *genuine cystic disease*. The tumour is always made up of cysts, *possessing* solid contents in different degrees; but

the existence of cysts is uniform and characteristic; *it is also a disease of the gland itself.* Unlike the other forms of adenocele, whether cystic or solid, it is a new growth, developed and growing independently of the mammary gland, although in some instances having a slight connection with it; but it is a true cystic adenoid disease of the mammary gland, dependent upon the dilatation of its ducts; and has, therefore, in the majority, if not in all cases, some communication through the nipple with the external surface. As a result, this condition gives rise to a symptom which, when present, must always materially tend to confirm the impression which may have been formed by the careful observation of the case, and by the presence of those conditions to which attention has just been drawn, and that is, the power the surgeon has to evacuate some of the contents of the cyst or obstructed ducts through the nipple of the organ by gradual pressure.

A cystic tumour of the breast-gland, in a healthy woman, unattended by any other than local symptoms, and accompanied with the discharge of a clear or coloured viscid secretion from the nipple, which can be induced or materially increased by pressure, may with considerable confidence be set down as the form of disease we are now considering, and may with justice be described as the *true cystic adenocele* of the mammary gland, in contradistinction to the false adenoceles or other tumours, which have but little, if any, connection with the true gland-tissue, but which are new growths partaking of the nature of the breast-gland, according to the pathological law, which appears universal, that all new growths partake of the nature and peculiarities of the structure in which they are developed.

On the Open, Ulcerating, and Discharging Tumour of the Breast.—There is a period in the growth of any tumour situated in the mammary gland, or in its neighbourhood, when the integuments become so involved as to ulcerate or give away, and when a discharging surface or cavity exists which presents an aspect differing according to the innocency or malignancy of the growth with which it is connected. In the cancerous tumour it is almost needless to add that the open surface or discharging orifice will be distinctly cancerous; the integument itself or the margins of the wound will be found infiltrated with the cancerous material, presenting the thickened, indurated, and everted margin so characteristic of the cancerous ulcer, and which, when once seen and appreciated, can hardly be mistaken. Not so, however, with the innocent tumours, which are also liable to be connected at some period of their growth with an open wound and discharging cavity, for in these cases a very different condition presents itself to our observation; and to understand this difference it is essential to recall one or two points of difference which have already been mentioned in the nature of the innocent and malignant tumours.

It has been stated that it is the peculiar nature of the innocent tumour to affect the part in which it is developed purely mechanically; it may separate or displace tissues, but it never involves them in any other way.

On the other hand, it has been stated that it is the peculiar nature of all cancerous or malignant tumours to infiltrate and involve every tissue with which it comes in contact.

Applying, therefore, these two opposite features of the innocent and malignant tumours to those cases of both diseases in which the integument is materially involved, we shall readily understand how two very different local appearances will be produced; for in the cancerous, as just explained, the wound or surface will be characterised by all the peculiarities of the cancerous ulcer, whilst in the innocent tumour the integument will have given way purely from over-distension, and, as a consequence, the margin of the wound or discharging cavity will look healthy, free from all appearances of infiltration, and rather as if cut or punched out mechanically than ulcerated.

This great difference between the two affections is most important and is very palpable, and often enables the surgeon to form a correct diagnosis in a case where otherwise a difficulty might be experienced.

In the cystic adenoceles this healthiness of the margin of the wound is very *marked*, for it is not uncommon to find a sprouting and discharging intra-

cystic growth protruding from the wound through the ruptured integument, and presenting a very doubtful and sometimes cancerous aspect; but if found projecting through an orifice of the integument which is uninfiltrated and apparently healthy, such as we have already described, the innocent nature of the tumour may with some confidence be declared.

On the value of the Retracted Nipple as a Symptom in Tumours of the Breast.—There can be but little doubt that the importance of this symptom of the retracted nipple has been considerably overrated, and that as a positive indication of cancerous disease it has been over-estimated. It may co-exist with a cancer in the breast, as it may with some simple or innocent affection; but, on the other hand, a cancer of the organ may be present unconnected with any such morbid condition.

For a retracted nipple may be described as an accidental symptom in the development of a tumour; it is the product of mechanical causes, and its presence is determined by the manner in which the gland is involved in the disease rather than in the nature of the affection itself. Should any tumour, simple or malignant—should any abscess, chronic or acute—attack the centre of the mammary gland, a retracted nipple, in all probability, will be produced; for as the disease so placed will necessarily cause material separation of the gland ducts, their extremities—terminating in the nipple—must be drawn upon, and, as a consequence, a retracted nipple will be the result.

In the early stage of an infiltrating cancer of the organ this symptom is one of frequent occurrence, the nipple being always drawn towards the side of the gland which may be involved; at a later stage, however, of the affection, when the infiltration is more complete, the nipple may again project. In a central chronic abscess of the breast the retracted nipple is equally common, and in the true cystic adenocele it may be also present. In the ordinary adenocele, whether cystic or otherwise, it is rarely present, for very plain reasons, as these are not diseases of the breast-gland itself, but only situated in its neighbourhood; in rare cases, however, such an association may exist; in one case only have I ever observed it, and in that some blow or injury had preceded the development of the adenoid tumour, and it is open to a doubt whether the retracted nipple might not have been brought on by some chronic inflammatory condition, such a cause being well able to produce it.

On the Value of a Discharge from the Nipple for Diagnostic Purposes.—It is very questionable whether this symptom has really any material value for diagnostic purposes; and when the discharge is slight or of a bloody nature, it certainly does not indicate any special affection. It is well known that in *cancerous* affections a discharge from the nipple is not infrequent, the fluid having the appearance of blood-coloured serum, but it is never profuse, there being rarely more than a few drops.

In the *ordinary adenoceles* this symptom is seldom present.

In the *true cystic adenoceles* this symptom appears to be of considerable value, for in all the cases which have passed under my observation, and in the majority of the recorded examples, this discharge from the nipple was a prominent symptom, the fluid being generally of a mucoid nature; and more or less blood-stained; and although at times it occurred spontaneously, and with relief to the patient, at others it could readily be induced by some slight pressure upon the parts.

It exists, therefore, as a symptom in the true disease of the breast structure, whether cancerous or adenoid; it is but slight and uncertain in the former, and more general and copious in the latter, and as a means of diagnosis becomes of some value.

On the importance of Enlargement of the Absorbent Glands as a Diagnostic Symptom.—When these are present with a tumour of the breast, they bear important testimony to the malignant nature of the disease; for in all the simple adenoceles no such complications exist, although in the inflammatory affections of the organ they may be produced.

Still in some cases of cancer of the breast many months may pass away without the appearance of these enlarged glands, and in one case of which

I have the record a year expired without giving rise to such a symptom. Nevertheless, when they are present they are of positive value, and point towards the malignant rather than the innocent nature of the disease.

On the Value of the Tubercular and General Infiltration of the Integument over the Breast.—If there be one symptom which affords more positive evidence than another of the cancerous nature of the tumour in the breast, it is the one we are now considering, for no such symptom is ever present in any inflammatory or simple disease of the mammary gland. It is a genuine tubercular or general infiltration of the integument with the cancerous product, and, as such, is of special value. It may be slight, from the mere shot or pea-like affection of the skin, to its more general infiltration; but in all stages it is equally characteristic, and speaks in positive language of the cancerous nature of the mammary growth, one tubercle telling as plain a tale as many.

On the Excision of the Breast.—There is little or no danger attending the excision of the breast, beyond that which accompanies any or even the smallest operation.

It is true that patients occasionally sink after having passed through the operation, from pyæmia, erysipelas, or other causes, contingencies which attend any practice, but from the excision itself there is little danger to life. Of the 133 cases of cancer which I have noted in which excision was carried out, nine cases died, or about 6·7 per cent., death taking place in each from the following causes:—

One from pyæmia, on the thirty-fifth day; one from erysipelas, contracted several months after the operation, when the wound had healed; two from acute bronchitis, three weeks and a month respectively after the excision; one from profuse diarrhœa, on the eighth day, probably pyæmic; one from hæmoptysis, in the third week; two from exhaustion after a return of the growth, in three and six months; and one from actual sinking after the operation on the third day. The three cases which died from pyæmia, and from diarrhœa, and which was also probably pyæmic, and the one which sank on the third day, may, perhaps, with justice be directly assigned to the operation, but the fatal termination in the remaining six examples had no reference whatever to the excision.

In the operations for the innocent tumour of the breast there was no fatal instance.

In operating for cancer it is unquestionably the wisest course to excise the whole gland, and it is as well not to be overanxious about preserving too much integument, for if any doubt exists as to its perfect healthiness, the suspected portion had better be excised.

It is always important, when dissecting out the tumour, to keep clear of all diseased tissues, and in fatty subjects to leave a fair covering between the incision and the tumour itself, for there is good reason to believe that an early return of the affection is too often to be explained by want of attention to the practice to which I have just alluded.

In the operation for "*adenocele*," it is quite exceptional for the removal of the breast to be necessary, and in the majority of instances such a practice would be clearly unjustifiable. As a rule, the tumour is readily removed on making a clean section through its cyst-wall, and the breast itself is rarely injured, even by the operation. In exceptional examples, however, of this affection, it is absolutely necessary that the breast-gland should be excised, that is, where a large tumour is closely connected with it; and in the genuine or true cystic adenocele of the mammary gland—in such examples as I have already quoted, and in those alone. In the removal of a small tumour not involving the breast the best practice is to make the incision in a line radiating from the nipple, and to manipulate the parts as little as possible; the surest plan being to cut well into the tumour, having previously raised and made it prominent by grasping it with the thumb and finger of the opposite hand quite at its base.

In case of complete excision the best line of incision appears to be that *which corresponds* with the course of the fibres of the pectoral muscle.

After both operations steady and moderate pressure, by the application of a firm pad to either margin of the wound, possesses many advantages, and a more rapid union appears to follow such a practice than any other.—*Guy's Hospital Reports, Vol.* x. 1864, *p.* 99.

112.—ON POLYPI OF THE RECTUM IN CHILDREN.

By M. Guersant.

This is far from being a rare disease, for M. Guersant meets in private or hospital practice from six to eight cases every year. Generally the polypi are found just above the sphincter, but at some autopsies they have been met with higher up, and in one case even in the cæcum. Scarcely ever exceeding a small nut in size, they are usually found solitary and pediculated. The pedicle is more voluminous in recent polypus, and is composed of the mucous membrane which covers the hypertrophied mucous follicle constituting the substance of the polypus. Generally soft and bleeding, the polypus is sometimes tolerably firm and resistant. The symptoms are significative, the child passing blood at stool, and especially at the end of the effort, and there are frequent desires to go to stool without any dejection taking place. When the fæcal matters voided are hard, they may be found grooved as a consequence of the pressure exerted by the polypus during their passage. If the amount of blood lost be not excessive, the health may not suffer for a long time; but when this is great, the child becomes pale and chlorotic. Generally, if the child be examined after going to stool, a red tumour is observed at the orifice of the anus, which returns after the evacuation, and this is usually mistaken by the friends for prolapsus ani or for a hemorrhoid, which M. Guersant has never met with in these young subjects. The form of the extruded tumour and its pediculated condition distinguish it easily from the prolapsus, especially if it be followed into the rectum by the finger, when it is usually found fixed to the posterior side of the rectum, and on pressing it, it slips from under the finger like a cherry-stone. The affection is often mistaken, the child being sometimes treated for dysentery; and when the pedicle is thin, the polypus may be separated during the passage of the fæcal matter, and discharged, a spontaneous cure being thus produced. Ordinarily, however, the pedicle is firmer, and the polypus continues to reappear after each stool. Although the prognosis is very favourable, surgical interference should always be resorted to, and the more promptly in proportion to the amount of blood lost. The operation is very simple. An enema is given, and the tumour usually appears when the last portion of this has been returned. It is seized with a forceps, and a silk ligature passed around its pedicle. It is frequently discharged at once; but if not, the ligature is allowed to pass into the rectum with the polypus, which then comes away in the evening or next day. Having met with considerable hemorrhage from the use of the scissors, M. Guersant regards the ligature as the preferable means. Sometimes the polypus has to be brought down to the anus by means of the finger, and when this is rendered difficult by its slipperiness, a small polypus forceps should be passed in by the finger, and the tumour brought down by their aid, or, what is better, submitted to torsion without bringing down. Any hemorrhage that follows may be arrested by cold-water injections or rhatany enemata.—*British and Foreign Medico-Chirurgical Review, July*, 1864, *p.* 266.

113.—ON THE TREATMENT OF ALBUMINURIA IN CHILDREN.

By Dr. Wm. H. Dickinson, Assistant-Physician to the Hospital for Sick Children.

[The only state of disease to which the substance of the kidney is liable during childhood is that which gives increase of bulk with a smooth mottled exterior. The fundamental change in all such cases is an inflammatory state of the tubules, in consequence of which they become choked up by an excess of their own epithelial growth.]

The mechanical obstruction of the tubes is the only event to be feared. If the complaint proves fatal, it is in consequence of this occurrence. It is this which interferes with the formation of urine, occasions the accumulation of its components in the blood, and is the source of all the evils to which the victims of the disease are liable. It must be the aim of treatment to keep the tubes clear; this done, the disorder will right itself. The increased vascularity will subside when free secretion is possible; and, if nothing is done to keep up the irritation of the gland, the catarrh will soon be at an end.

We must seek then to increase the quantity of fluid which washes the tubes, without doing anything which can produce local irritation. In short, we must pass as much *water* as possible through the organ. This fluid is necessarily devoid of irritating qualities. It probably makes no demand upon the true secreting power of the gland, but passes by filtration from the Malpighian bodies.

I have adopted, since the year 1860, a plan of treatment founded upon these principles; and when applied to children, the results have been such that I have determined to bring them before the Profession.

The tendency of medical observers to find success in methods of treatment devised by themselves, is an influence probably more constant in its operation than any remedy to which suffering humanity is subjected. The consciousness of this source of error has made me very cautious in drawing conclusions upon this question, and has induced me to resort to what means I could of testing the results.

The treatment of the disease in grown persons does not enter into consideration here. The tardiness with which the adult kidneys respond to stimuli, makes a modification of the system necessary.

With children the method has been mainly hydropathic. The way it has been carried out, and the results which have been attained, will appear from the following details.

Every case of albuminuria in children which I have had to treat since October, 1860, has been subjected to the same regimen, though in some cases the dropsy was so great as to seem to prohibit fluids. Notes have been kept of twenty-six cases, most of which were severe. Many others of less import, have come and gone among out-patients, and left no record. All have been restricted to a fluid diet, which has been of a nutritious character. Besides this, a certain quantity of spring water, varying from two to four pints, has been administered in the twenty-four hours. In three of the cases no other remedy was used. In the remainder the action of the water has been seconded by small doses of infusion of digitalis, or more rarely of acetate of potass. Lastly, when the active symptoms have disappeared, iron has been given, either as sesquichloride or acetate.

The results may be stated generally. Of the twenty-six cases thus treated, twenty-two were known to have recovered and to have got rid of every trace of albumen. Three improved greatly and had little evidence of disease excepting slightly albuminous urine, when they ceased to attend the hospital and were lost sight of. The remaining case ceased to be my patient, and, as I learned, eventually died. Thus, out of twenty-six cases, twenty-five are believed to have recovered, which result is known to have been completed in twenty-two. Many of the cases were of the greatest severity, such that before the adoption of this system of treatment no *expectation would have* been held of their recovery.

These results do not suffer by comparison with those attained by other methods of treatment.

In the Hospital for Sick Children, I have been able to collect the particulars of thirty-nine cases which were admitted as in-patients between the years 1852 and 1861. Of these twenty are known to have recovered. Eleven died, while eight were more or less relieved. The remedies for the most part were aperients and diaphoretics, the latter including vapour baths and antimony. It must be considered, however, that from the inadequate size of this institution, none but very severe cases can be admitted; and, therefore, no equal comparison can be made between these results and those derived from more mixed cases.

Dr. Miller, in his work on Scarlatinal dropsy, states, that of sixty-nine cases afforded by dispensary practice, eight died. His treatment was that in vogue ten or fifteen years ago, by bleeding, purging, and sweating.

It was found that with the cases under consideration the number of days under treatment, before the patient was restored to apparent health, varied from ninety-seven to four; the average of the whole number of cases being thirty. A certain time was then required to get rid of the last traces of albumen. The recovery was thus delayed to between 179 and seven days, giving an average of forty-five days from the commencement of treatment to the total disappearance of the albumen.

It was found that the use of the water when given under the circumstances stated, never increased the dropsy, but the contrary. It was usual, however, when the œdema was excessive, to let the digitalis set up a certain amount of diuresis before the full quantity of water was ordered. Many children take it eagerly, particularly in the early stages of the disease. The urine was often observed to undergo a notable increase in the quantity of renal epithelium which it deposited, and the albumen usually quickly lessened in greater proportion than could be explained by the increase in the quantity of the urine.

It was supposed, in one or two cases, not under my own care, that the treatment had a tendency to encourage hæmaturia; and although I have not observed this myself, yet it must be admitted that such an occurrence is not unlikely. If the kidney be congested the escape of blood is a natural means of relief. This cannot take place if the tubules are obstructed through which the blood must come. When, therefore, the plugs are washed out it is conceivable that an evacuation should take place which before was hindered.

The general use of iron during convalescence may be held to weaken the argument in favour of the other measures adopted. It may, however, be fairly presumed that it does not influence the original disorder, though it corrects the anæmia which results from it. As my object has been rather to benefit my patients than to make neat experiments, I have departed from simplicity in this respect.

Brief notes of one or two cases may be given as examples.

Case 1.—Eliza S., fifteen years of age, was admitted into St. George's Hospital under the care of Dr. Bence Jones, 10th October, 1860. A fortnight previously she had caught cold from dabbling about a street pump. The face and all the limbs swelled, and were much so when she came under observation. The complexion was pallid. She sat upright, with the shoulders supported by the arms; and it appeared from auscultation that two-thirds of each pleural cavity were full of fluid. The breathing was rapid. She coughed a good deal, and spat up bronchial mucus. She complained of pain in the loins, and micturition was frequent. The urine was bloody and highly albuminous. Specific gravity 1019. The use of the microscope showed that it contained abundant pus cells, but no casts. The only treatment adopted was the administration of four pints of distilled water daily. By the 4th of November the albumen was reduced to a mere trace, and the colour of the urine was natural, excepting that it was less bright than usual. All the superficial dropsy had gone, and the pleural cavities gave evidence of diminished distension. But two days later she was unwisely permitted

to go into the garden, and blood reappeared in the urine. Coarse granular casts were now discovered, which consisted of broken-down epithelium. The relapse, however, was temporary, and by the end of the month she was practically well. The albumen was so diminished that its existence became a question. On the 10th of December, she was in perfect health. The face was rosy, the pleuræ fully resonant, the appetite great, and the urine absolutely natural. She now left the hospital, but returned to show herself occasionally. She remained in perfect health.

Case 2.—George T., three years of age, had an attack of scarlet fever which was followed within three weeks by swelling of the face, legs, scrotum, and belly. Diarrhœa came on, and the swelling subsided. The bowels remained loose after the swelling had entirely disappeared. He then had vomiting, which was frequent through the whole of one night, and in the morning three well-marked epileptic fits occurred in succession. He was now brought to the Hospital for Sick Children, and came under my care as an out-patient. He was visited at his own home. He was extremely pallid, but there was no trace of dropsy. The head was hot, the tongue coated, the pulse rapid—160. The urine was scanty, it was the colour of dark sherry, and when boiled the clot of albumen occupied half the bulk of the fluid. He was ordered to live entirely on slops, to drink two pints of spring water in the day, and to take as medicine a drachm of the infusion of digitalis, with half a minim of laudanum three times a day. He had no more fits. The urine increased in quantity, and gave a copious deposit of epithelium and epithelial casts. Three days later all chance of head symptoms appeared to have passed away, and the tincture of sesquichloride of iron was ordered in the place of the opium Next day the albumen was reduced to a hardly perceptible trace, and on no subsequent examination could any be discovered. Within a fortnight of his coming to the hospital he was in perfect health.

This example illustrates the treatment adopted on the occurrence of cerebral complications. Cupping, purging, and blistering are believed to have disastrous effects. An anæmic state of brain, of which dilatation of the pupil is a sign, is constantly found after death from anæmic convulsions; and such measures as these appear likely to increase the condition. Opium, on the other hand, is its direct antagonist. It fills, instead of emptying, the cerebral vessels. It here seems to have been used successfully as an adjunct to means more directly bearing upon the renal affection. Experience shows that convulsive attacks are peculiarly apt to come on after the exhaustion of diarrhœa or vomiting, by which the cerebral and other vessels are drained of their contents.

The same measures which succeeded in this case have been used in others with the same result.

Case 3.—Edward L., six years of age, became an in-patient at the Children's Hospital, 11th September, 1862. In the absence of Dr. Hillier he fell to my charge. A month previously he had had the rash of scarlatina, but the disease was slight, and in eight days he was going about as usual. Four days before his admission his face swelled slightly, and his urine was observed to be scanty. When seen the œdema was excessive over the whole surface, and the skin had a transparent waxy appearance. The albumen was not large in amount, but the urine was very scanty. The child was depressed, the tongue coated, and the case apparently one of great severity. He was ordered to be fed upon fluids, including plenty of broth, to drink three pints of water in the day, and to take a drachm of the infusion of digitalis every four hours. Next day he was attacked with pain in the side, and a friction sound was heard over the lower part of the left pleura. No change, however, was made in the treatment, except that two leeches were put upon the place. The pleurisy passed away without the effusion of fluid, the urine became more copious, and the dropsy slowly diminished. On the 17th, it was necessary to stop the digitalis in consequence of faintness. The œdema now was scarcely perceptible. A mixture of the acetates of iron and potass, in the proportion *of two* grains to ten, was given three times a-day, and under its use all the

remains of œdema disappeared, the child's aspect became ruddy, and all traces of albumen vanished. When he left the hospital, on the 17th of October, he was in perfect health, and could not have been recognised as the same child who had been admitted a few weeks before.—*Edinburgh Medical Journal, Sept.* 1864, *p.* 195.

114.—ON INCONTINENCE OF URINE IN BOYS.

By James Adams, Esq., St. Helen's Place, London.

[Some cases of incontinence of urine in boys are extremely difficult to relieve, but by the following simple plan a cure may generally be rapidly effected. Cases owing to stone in the bladder, or to worms in the small or large intestine, are of course only curable by treatment suitable to the cause. The treatment recommended by Mr. Adams is intended only for cases which can be traced to no assignable cause, which are considered to depend on an irritable state of the bladder, and which are often attributed to weakness of constitution and struma, and for which steel, quinine, and strychnia are frequently ineffectually administered.]

The plan is one of great simplicity, and consists in encircling the root of the penis in a common elastic ring made of vulcanised india-rubber, such, in fact, as is used for stationery purposes. Let the bladder be emptied the last thing at night; the ring is then to be applied, and must be retained during the night, and removed early in the morning. It may be also used during the day if the bladder is irritable, which is not uncommonly the case.

In cases which have come under my notice, I have administered the carbonate of iron and an alkali, with an occasional purge of calomel and scammony; but I do not attribute much importance to the medical treatment, as I have always found that similar medicines have been ineffectually given before the ring was employed.

There is nothing new, after all, in retaining the urine in the bladder of children, as formerly it was the invariable custom to clap on the *jugum penis*, for five or six hours before the operation of lithotomy; but I am not sure that the plan I now mention has been used for incontinence of urine before. I can recommend it for its extreme simplicity and its unquestionable efficacy.—*Lancet, May* 28, 1864, *p.* 621.

A SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE PRECEDING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

AFFECTIONS OF THE SYSTEM GENERALLY.

ACUTE RHEUMATISM.—*Treatment by Blisters.*—Place blisters of considerable size around the limbs, in close proximity to the inflamed joints, with the object mainly of affording through the serous discharge from the blistered surface, a ready means of exit for the poison or materies morbi. Six or seven blisters may be applied simultaneously, and linseed-meal poultices subsequently applied will favour a sufficient flow of serum. The most important results observed in the cases thus locally treated are, the rapid diminution in the force and frequency of the pulse, and the immunity of the heart from inflammatory mischief. The usual alkaline plan of treatment produces great depression of general power. This treatment is of no use in gout, for the crystalline urate of soda cannot be expelled like the fluid poison of acute rheumatism, but is deposited as a solid in the inflamed joint. (Dr. Davies, *Clinical Lectures and Reports of London Hospital*, 1864, *p.* 293.)

CANCER.—*Galium Aperinum.*—In the same manner as a similar remedy acts in scurvy, the fresh juice of the galium aperinum seems to act in cancer, altering and improving the unhealthy condition of blood. It has been long employed as a popular remedy in cancerous affections. A solid extract is a convenient form of preparation, of which a drachm and a half may be given twice a day dissolved in a wineglassful of water. (Mr. F. A. Bulley, p. 108.)

DELIRIUM OF FEVER.—*Sulphuric Ether and Ammonia.*—In cases of violent delirium of fever, with hot burning skin and quick pulse, wine is quite inadmissible, notwithstanding the depression and sordes, as it increases the delirium and excitement. A mixture of sulphuric ether and carbonate of ammonia will be found to act beautifully, particularly where there is subsultus tendinum, or muscular spasm of the limbs. The addition of ipecacuanha wine in doses of three or five drops materially assists in removing the sordes and promoting perspiration. (Dr. H. Osborn, p. 35.)

DROPSY.—*Stork's-bill.*—The Erodium cicutarium or common stork's-bill, an indigenous plant belonging to the natural order Geraniaceæ, is a remedy of great use in dropsies, as it possesses considerable diuretic properties. It grows abundantly by the sea-side, in sandy situations. It should be used as a decoction, two ounces of the dried plant in three pints of boiling water. An interesting case is related in which it acted very beneficially, after the failure of digitalis and other diuretics, and also of hydragogue cathartics. (Dr. W. Abbotts Smith, p. 79.)

FEVER.—*Stimulants and Food.*—Keep the patient cool, sponge his skin over three or four times a day, give him plenty of liquids to support the renal respiration, and plenty of fresh air to support the pulmonary respiration. The plan of giving enormous doses of brandy is bad; if the patient is *likely to* die, no amount of brandy will save him, and the plan of giving him

nourishment every half hour may be carried to an injurious excess, for it is very doubtful whether any of the nourishment given in severe cases is assimilated. (Dr. Gull, p. 24.)

INTERMITTENT FEVER.—In recent cases there is frequently considerable gastric derangement. In such cases, quinine must never be given until the tongue is tolerably clean, and the stomach free from irritability. The minimum dose of quinine required to prevent a paroxysm is ten grains, and it must be given three hours before the expected attack. Where, however, the paroxysms are irregular in their character, five grains of quinine given every four hours generally answers thoroughly. Out of 109 recent cases, 102 yielded at once, either to ten grains, or to five grains, every four hours, and in but seven was a repetition or continuance of large doses required. To guard against relapse, quinine was generally continued in doses of two grains three times a day, for a week or two. It is quite impossible to check a paroxysm with the liq. potass. arsenitis, however administered, in the same manner as it is with quinine. (Dr. S. H. Ward, p. 28.)

Salicine.—It has long been known that salicine, the alkaloid obtained from the common white willow, possesses febrifuge properties, but it has not been employed in sufficiently large doses. It may be given simply as a decoction of the bark, associated with other bitter and aromatic vegetable extracts. An ounce to a pint of water is the proper strength for a decoction. M. Cazin, of Calais, usually combines it with camomile and wormwood. (p. 36.)

RHEUMATIC FEVER.—After the acute feverish symptoms have been allayed by the use of calomel and opium, followed by a purgative, it is a great element in effecting a rapid cure to commence at once with the administration of quinine or bark, or a combination of both. The calomel and opium are far preferable to Dover's powder, as they do not cause and keep up profuse perspiration. No other application is necessary, or is so grateful to the affected joints as warm dry cotton. (Mr. J. Roberton, p. 30.)

ZYMOTIC DISEASES.—*Sulphites.*—Not only sulphurous acid, but the sulphites of the alkalies and earths possess the property of arresting fermentation, and neutralizing catalytic action. Hence, in catalytic (zymotic) diseases, the salts of sulphurous acid, the sulphites, hyposulphites, and bisulphites, are remedies of the greatest value, arresting the action of the ferment in the blood. The administration of these salts has been very extensively tried by Dr. Polli, of Milan, and by the author, "and in every case their administration seemed to be of decided advantage." The bisulphites of soda may be given in doses of twenty grains every hour in severe cases of scarlatina, poisoned wound, measles, or fever, and in smaller doses to children. It is quite unproductive of any injurious effects. (Dr. de Ricci, p. 31.)

AFFECTIONS OF THE NERVOUS SYSTEM.

EPILEPSY.—*Belladonna.*—A case of epilepsy in a young man is related, which remained unamenable to apparently judicious treatment, until belladonna was administered in doses of one-sixth of a grain of the extract three times a day. It was an ordinary case of epilepsy, the only noteworthy thing being that the attacks invariably followed the ingestion of alcohol in any form. (Dr. J. C. Thorowgood, p. 40.)

FACIAL NEURALGIA.—*Acupuncture.*—The cause of facial neuralgia is probably a superabundance of nervous fluid in the nerve affected. Nervous and electrical power are identical, hence it follows that if a conductor be presented to a nerve so overcharged, the superabundant fluid will escape and the pain cease. This is practically found to be the case. Let two or three sewing needles be inserted near to where the supra-orbital nerve issues above the eye, and neuralgic pain in the forehead will cease. This plan does not answer in all cases, but in many it proves of almost magical efficacy. (Mr. W. Craig, p. 41.)

FACIAL PARALYSIS.—*Local Injection of Strychnia.*—Inject a few drops of

a solution of strychnia along the course of the facial nerve, between its exit at the stylo-mastoid foramen and its passage to the neck of the condyle of the lower jaw. The injection must be repeated every two or three days. In three cases in which this has been tried, a perfect cure resulted in from three to six injections. (M. Courty, p. 44.)

NEURALGIA AND MUSCULAR RHEUMATISM.—Idiopathic neuralgia and muscular rheumatism are two varieties of the same disease, the nerves of common sensation being affected in both, and the causes of both being the same. A piece of linen more or less wetted with chloroform must be laid on the painful part, and covered with the palm of the hand. This plan generally relieves very rapidly. (Dr. Dupuy, p. 44.)

PARALYSIS OF THE HAND AND FOREARM FROM EXCESSIVE SMOKING.—A case of "dropped hand" is related, which the author considers arose indubitably from the excessive use of tobacco. It certainly rapidly passed off on discontinuing the habit, and no other cause whatever could be assigned for it. (Dr. A. J. H. Banks, p. 45.)

RHEUMATIC MUSCULAR PAIN.—Apply the following ointment with a feather over the seat of pain, and then cover the parts with cotton wool and a waterproof cloth. ℞. Tinct. aconiti, 3j.; adipis, 3ij.; chloroformi, 3j.; morphiæ muriatis, gr. xv. Mix the tincture with the lard, and add the chloroform and morphia in a glass mortar; keep in a wide-necked and well-stopped phial. (Dr. Dupuy, p. 44.)

TETANUS.—*Ice to the Spine.*—In an interesting case of recovery from tetanus great relief seemed to follow the application of ice along the whole course of the spine. (Mr. Adams, p. 39.)

Local Use of Tobacco.—In cases in which it is thought advisable to administer tobacco internally, it is well to apply it at the same time to the wound which originates the mischief. When it is desirable to make a decided impression quickly, a solution of nicotine of known strength may be used. The influence of the drug may be obtained by local use only, much of the depression attending its internal administration is thus avoided. (Mr. H. J. Tyrrell, p. 37.)

AFFECTIONS OF THE CIRCULATORY ORGANS.

FATTY HEART.—*Diagnosis by the Pulse.*—In many cases of fatty heart the pulse beats exactly at the healthy standard as to frequency, but even in these there is a peculiarity, and one diagnostic of the state of heart. The organ being enlarged, so to speak, the pulse is so likewise, at the same time that the beat to the finger does not convey the idea of strength. It is large and compressible, "passing sedately under the finger." It is action without power. (Dr. H. Kennedy, p. 45.)

Fatty disease of the Heart induced by Tobacco.—The habit of smoking is in many cases the predisposing cause of the disease. Tobacco is a depressor of the nervous system, and its habitual use engenders a state of health which is very apt to be followed by a fatty heart. (Dr. H. Kennedy, p. 47.)

HEART.—*Effect of Tobacco on the.*—It very frequently happens that inveterate smokers have an intermittent pulse, quite unexplainable by organic lesion of the heart. The normal action of the heart will in a large proportion of such cases be restored on the abandonment of the habit. (M. Decaisne, p. 49.)

HEMORRHAGE.—*Wire Compress.*—The following plan of arresting hemorrhage after operations has some advantages which acupressure has not. Thread a piece of very fine soft passive iron wire with a needle at each end, and, having seized the bleeding point with forceps (merely to accurately indicate its position), pass the two needles one at either side of, and close to the artery through the flap, and let them emerge on the cuticular surface *about* half-an-inch apart. Remove the needles, place a little piece of cork *on the skin* between the emerging wires, and twist the latter together until

the hemorrhage is arrested. The process is then complete. To remove the wire, cut one end close to the skin, remove the cork, and place one finger firmly in the position previously occupied by the cork: gradual, firm, but gentle traction of the other end will readily remove the wire. The process is easy, effectual, and admits of accurate adjustment of the cut surfaces and edges. (Mr. J. Dix, p. 90.)

HEMORRHOIDS.—*Persulphate of Iron Externally.*—Bathe the piles freely with lead water, and on retiring to bed at night apply ferri persulphas, 3ss., cerate simplex, ℥j., well rubbed together. In ulcerated hemorrhoids, and when the constitution is debilitated from diarrhœa or excessive fatigue, this application gives great relief, soothing pain and cauterising the part. (Dr. G. S. Cartwright, p. 118.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

HOOPING COUGH.—*Bromide of Ammonium.*—After freely trying the bromide of ammonium in hooping cough, the author observes, "In nearly all the cases there was marked relief to the hoop after a few doses had been given." Dr. Gibb, of London, who first introduced the drug, gives two or three grains thrice daily to infants, and four, eight, or even ten grains, as frequently, to older children. (Dr. R. P. Ritchie, p. 54.)

PHTHISIS.—*Living in the Open Air.*—Dr. Blake, San Francisco, California, has published in one of the American journals a plan of treating phthisis perhaps more applicable to California than to England. He directs his patients to live entirely in the open air during the summer months, changing their habitat in winter to a part having the same winter temperature as had their summer residence. The patients are directed not even to sleep in tents, but out under the trees, and to live on plain camp fare. Dr. Blake believes that, except in those cases in which the destruction of lung tissue is too extensive, there are few which cannot be cured by patients availing themselves of the summer climate of California and of the winter climate of Mexico. (Dr. J. Blake, p. 53.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

ABDOMINAL TUMOURS.—*A New Means of Diagnosis.*—Tumours in the abdomen connected with a part which moves with the respiratory act, such as the stomach, transverse colon, and liver, of course are affected by this action along with the organs to which they are attached. This forms a valuable means of diagnosis from tumours springing from immovable parts, as the aorta (aneurism), the kidneys, or the abdominal walls themselves. This motion, synchronous with respiration, is not to be confounded with mobility of tumours by the hand of the surgeon, and it must be remembered that a tumour springing from an organ movable by respiration, may by peritoneal adhesions become immovable. (Dr. H. Kennedy, p. 56.)

Tumours from the Colon being Dilated with Hard Fœculent Matter.—There is usually more than one such hardened mass perceptible on careful manipulation. A very valuable aid in the diagnosis of such tumours, is the fact that they are impressed, as it were, with a certain amount of movement in themselves, owing to the slow peristaltic action of the bowel. It will frequently be observed that the tumours have altered their position slightly, but still so as to leave no doubt on the point. (Dr. H. Kennedy, p. 57.)

ACUTE HEPATITIS IN THE EAST.—No time must be lost, as suppuration will take place in a severe case in the course of a few days, or even hours. General bleeding must be carried to the extent of relieving pain when such is present, or the dull tensive weight and uneasiness in the right side complained of in other cases, and the operation should give full freedom of respiration, and relaxation of the surface of the body. After some hours, should these indications of relief prove lasting, then in a constitution otherwise

sound, another free abstraction of blood must be made, followed by leeches to the side, repeating these local abstractions until all the serious symptoms shall have vanished. Mercury may be used as an adjuvant, "its exclusion from the treatment of acute hepatitis in Bengal is unsafe and unwarranted." After the subsidence of all the more acute sequelæ the persevering use of the nitro-muriatic acid will go further towards restoring the healthy condition of the liver than any other means. (Sir J. Ranald Martin, p. 65.)

Dyspepsia from Zinc in Water.—Some spring water contains free hydrochloric acid. If this water is passed through pipes coated with zinc (which is frequently the case) the water will become impregnated with a salt of zinc. Dyspepsia and other symptoms arise from this. (Dr. H. Osborn, *Med. Times and Gazette, May* 28, 1864, p. 586.)

Enlarged Tonsils.—The practice of applying caustics, alteratives, or absorbents for the reduction of hypertrophied tonsils is inefficient; but a positive effect has been accomplished by the use of escharotics. The "London paste" employed by the author is composed of caustic soda and lime moistened with a little absolute alcohol. This paste penetrates rather than spreads. It must not remain in contact with the hypertrophied tonsil for more than five seconds. A little instrument has been contrived, and will be found figured at p. 110, intended to prevent the accidental contact with parts not meant to be touched. (Dr. M. Mackenzie, p. 109.)

Hare-Lip.—*Operation for.*—The earlier the operation is performed the better; it should decidedly be done before teething, and usually within the first month, and even a few hours after birth. If, however, the child is weakly, and the gap large and complicated with cleft palate, the operation should be delayed for some months until additional strength is acquired, and also that the parts may be pushed closer in apposition by means of an instrument adapted for that purpose; and the same instrument must be employed after the operation, instead of strapping with plaister, in order to keep the parts together (this instrument will be understood at once by reference to the woodcut at p. 102). If a notch or irregularity is left in the lip, it arises generally from too little having been cut away from the margins of the fissure, and to ensure a good and easy approximation of surfaces there must be a free separation of each side of the fissure from the areolus. (Prof. Fergusson, p. 102.)

Intussusception.—*Insufflation.*—Introduce the nozzle of a small pair of bellows into the anus, and inject air gradually, but to a considerable extent. The invaginated part will sometimes pass up with a distinct crack. Water is not borne like air, indeed in some cases very little can be got up. Air may be injected until the body shows signs of considerable distension. The symptoms of intussusception are briefly, the sudden seizure, the obstinate vomiting and constipation, the pain, the hard tumour in the abdomen, but chiefly the passage of blood per anum. (Dr. D. Greig, p. 61.)

Larynx.—*Glycerine as a Vehicle for Local Applications.*—One of the best vehicles for local applications to the larynx is glycerine. Two drachms of tannic acid dissolved in an ounce of glycerine is frequently of much use. A moderately strong solution of morphia in glycerine is a most soothing application in cases of irritable larynx. Being viscid, it adheres to the surface of the mucous membrane, and retains there the astringent or the sedative which it holds in solution. It may be mixed with a solution of nitrate of silver, or with one of perchloride of iron, or it may be made the vehicle for alum. It must always be applied by means of a brush. (Dr. G. Johnson, p. 112.)

Strangulated Hernia.—*Extra-peritoneal Operation.*—All that is necessary in practice is to relieve the tension of Gimbernat's ligament in femoral hernia, of the conjoined tendon or fibrous aperture in inguinal hernia, and of the linea alba in umbilical and the more common forms of ventral hernia. These structures may be reached and relaxed, the cutaneous tissues only being divided, prior to the use of the hernia knife. Make a short incision through the skin and superficial fasciæ, near the neck of the tumour, and sufficiently large to admit the end of the finger, to such an extent as to enable the finger *nail to detect the* interstice or boundary line between the firm unyielding

apertural margin and the more movable and yielding tissues of the hernia. Insinuate the hernia knife along the finger nail (it should be blunt-pointed but cutting to the point) close to the constricting margin. It must be used very sparingly and must not go beyond the finger nail. The incision must be followed by pressure with the nail and tip of the finger, in the same direction. The hernia may now be returned. (Mr. F. Jordan, p. 113.)

In cases of strangulated hernia it is quite unnecessary even to expose the sac. Make a superficial incision on one side of, but about an inch below the neck of the hernia. This incision should be transverse to the deeper one, which must be carefully made through the connective tissue covering the pectineus muscle in femoral hernia until the finger can be carried, covered by some connective tissue, outwards, upwards, and backwards, to the edge of Gimbernat's ligament. The hernia knife must now be carried along the finger nail, and a line or two (according to the amount of connective tissue) beyond; the edge is turned to Gimbernat's ligament, and a very sparing incision made transversely inwards. The tip of the finger now being firmly pressed towards the spine of the pubes, and the thigh flexed and rotated inwards, the hernia may be readily returned. In inguinal hernia the careful division of the superficial fasciæ will enable the finger to pass along the neck of the tumour, to the seat of strangulation. This plan of operation is especially applicable to umbilical hernia. (Mr. F. Jordan, p. 116.)

AFFECTIONS OF THE URINARY ORGANS.

Acute Orchitis.—*Puncturing the Testicle.*—The pain from this disease is frequently almost unbearable, and is but slowly relieved by the ordinary treatment, viz., leeches, and ice applied to the part, followed by strapping with plaster. Instead of this take a small sharp bistoury and puncture the inflamed organ; blood and serum will escape with very great relief to the pain. The ordinary practice at the Hôpital du Midi, in Paris, is to puncture in several places with a lancet; the instrument is not carried into the body of the testicle, but simply through the tunica albuginea, to tension of which the pain is chiefly owing. (Mr. Henry Smith, p. 136.)

Albumen in the Urine.—*Detection of.*—The following is the most sensitive way of using nitric acid for the detection of albumen in the urine:—Pour not less than half a drachm of fuming nitric acid into a test tube; incline it, and then let a like quantity of the suspected urine trickle down very slowly to the acid. If albumen be present, a milk-white, sharply-defined, tolerably tenacious film will form at the exact point of junction of the two fluids. Sometimes this film is so thin as only to become visible by reflected light when the test tube is inclined. If, in immediate contact with the acid, a ruby or violet ring is developed, uroxanthine is present; and bile also, if in addition to a red or violet there is formed likewise a green coloured ring, which remains for some time. (Dr. A. Clark, p. 127.)

Deposits of Urates in the Urine.—A frequently recurring deposit of pink urates, without any assignable cause, in nine cases out of ten, depends on some chronic affection of the heart, liver, or spleen, associated with a tendency to gravel. In such cases alkaline tonics should be given, or the acid salts of vegetable acids, as citrates, tartrates, lactates, or acetates, which, in the system, are converted into alkaline carbonates. (Dr. G. Harley, p. 69.)

Lithic Acid Diathesis.—In all cases of lithic acid diathesis the carbonate of ammonia should be avoided, on account of the salt which it forms being much less soluble than that formed with lithic acid by potash, soda, or lithia. In cases of uric acid deposit it is unnecessary to render the urine more than neutral. Indeed, there is a danger in making it alkaline, from the circumstance that phosphates have a great tendency to form round uric acid calculi. (Dr. G. Harley, p. 70.)

Lithotomy.—*A cause of failure in the Operation*.—Most of the cases

hitherto related as instances where the incisions for lithotomy have been made, and a stone has not been found, have been examples where the surgeon has failed to reach the bladder. This occurs more frequently in children than in adults. For the perineum is much deeper in proportion in the child than in the adult, and, in addition, all the material is loose, lax, and loaded with fat. The circumstance that the bladder is more abdominal than pelvic in early life, has been greatly overlooked. To avoid this accident more care than is usually given should be devoted to the operation, and as the surgeon cuts into the membranous portion of the urethra and neck of the bladder, he should never push the point of his forefinger onwards, unless he feels certain that he has it between the staff and the wound. (Prof. Fergusson, p. 119.)

A New Lithotomy Tube.—In cases of lithotomy in adults (in whom the wound is long), and in children when the operation has been prolonged, and the cellular tissue considerably disturbed, the introduction of a tube through the wound, and its retention in the bladder for several days afterwards, is a measure of the greatest value. A wood cut and description of a tube, intended for this purpose, will be found at p. 126. The tube is curved, and of as large a bore as possible, and a piece of sponge is passed through it, projecting at each end. This sponge is intended to take up the urine by capillary attraction, and convey it through the tube. Over the end of the tube an india-rubber receptacle for the urine may be tied. (Mr. J. Hutchinson, p. 126.)

Lithotomy in the Female.—Dilatation of the urethra in the female may be effected very easily and safely, by passing into the urethra a number of pieces of catgut bougie, about four inches long, as many as can be introduced conveniently. They may be secured in position by a thread tied round them, and attached to a bandage which, passing over the os externum, has a slit in it to admit the bougies, and is tied at either end to another bandage round the waist. The bougies becoming swelled, dilate the passage in the gentlest and most gradual manner. Additional bougies may be added, in such numbers and at such intervals as is found to be desirable. The fresh bougies must be oiled, and inserted between those that are already there. The dilatation usually takes between twenty-four and forty-eight hours. (Dr. G. M. Humphrey, p. 128.)

LITHOTRITY.—As a first-step a lithotrite of considerable size should be introduced, equal to a No. 10 or 11 bougie, and the stone be broken into various fragments. Next take a very small lithotrite, and crush the fragments, and then use the small scoop with the object of removing some of these fragments. In a few days use the small crusher and scoop again, but particularly the latter, with which fragments may be extracted singly, or two or three together. This is a much better plan than waiting for the spontaneous escape of the fragments. Very large lithotrites are by no means necessary, and are grasped too closely by the urethra to admit of ready manipulation. Moreover, a fragment of considerable size may remain betwixt the blades of a small lithotrite, and yet the united size or diameter may readily pass or be drawn along the urethra. (Prof. Fergusson, p. 119.)

STRICTURE OF THE URETHRA.—*Holt's Dilator.*—The author says that the rapid dilatation of strictures by means of his dilators may be performed with as great safety as any operation in surgery, providing that the surgeon will only ensure the dilator being passed fairly through the stricture and not through the side of urethra, as has been done. (Mr. B. Holt, p. 132.)

UROHÆMATIN.—*Its Pathological Significance.*—The colour of the urine as passed is a most treacherous guide as to the healthiness of the secretion. Many specimens of pale urine, when boiled with a quarter their bulk of strong nitric acid, develop a deep mahogany colour; this is from the presence of an excessive amount of urohæmatin, indicating great destruction of blood corpuscles, the debris of which are thus eliminated. An ignorance of this frequently leads to the belief that there is nothing materially wrong, when a grave lesion is making rapid strides towards a fatal termination. The presence of this blood debris does not materially affect the specific gravity of the urine. This state of urine is found in many cases of obscure disease,

usually considered to be simply hysteria. In some cases the excess of uro-hæmatin in the urine is so great, that after it has been set free by an acid, and taken up with ether, the ether, after standing, solidifies into a red currant jelly-like mass, and may actually in some cases be cut with a knife. This symptom is met with in various diseases, especially, however, in some cases of chlorosis, and is then of grave import. Our efforts must be directed to restore to the blood as much as possible of the material which is being drained from it. This is best supplied by syrup of phosphate of iron. The preparations of zinc are also useful astringent tonics. (Dr. G. Harley, p. 74.)

Vesical Sounds.—The shank of a vesical sound should be of smaller diameter by several sizes than the curved end, to admit of more ready manipulation; moreover, the comparatively greater weight of the end gives more certainty to both hand and ear. (Prof. Fergusson, p. 123.)

AFFECTIONS OF THE BONES, JOINTS, ETC.

Injuries of the Skull.—*Trephining.*—To elevate a depressed piece of bone is desirable, but so many cases recover where the bone remains depressed, and so many die where it is elevated by trephining, that the remedy has proved, in such cases, hardly an assistance to nature. Out of 35 cases of trephining in the Federal army in six months, twenty-eight died, two remained undecided, and but five had recovered. It cannot be said that the trephine should never be used, but the cases are very exceptional which require it. (Dr. J. Ashhurst, Jun., p. 100.)

Wire Ligatures cut Short and left in Wounds.—When, in cases of amputation, the vessels are secured by wire ligatures, it is unnecessary to remove them at all, if cut off close. They will produce no irritation, and the rapid healing of the wound is very much favoured by the avoidance of any disturbance, and of the pain caused by removing ligatures, which is occasionally very severe. The same principle may be applied to ligature of the veins in cases of varicocele; and to ligature of arteries in cases of aneurism, where pressure has failed, but where it is desirable that a modified current of blood should be permitted to pass through the artery, by partially compressing the main artery by tying the wire loosely and cutting both ends short. (Mr. B. Holt, p. 98.)

AFFECTIONS OF THE SKIN.

Acne Rosacea.—*Armoricia.*—Let an infusion of the root of armoricia (horse-radish) be used frequently as a lotion to the parts affected, and let the compound infusion be taken as an internal remedy. Two ounces may be taken as a draught, night and morning, with a little compound spirit of ammonia and syrup of ginger. This treatment frequently proves of great service. (Mr. T. F. Ker, p. 144.)

Alopecia.—In this disease, when not senile, the hair follicles are merely in a state of atony and not destroyed. There is frequently some removable cause. When the skin has not had free power of secretion, from the too abundant use of pomades, &c., the head should be well washed with the yolk of egg, or borax in almond emulsion, afterwards carefully rinsing with warm water, and to apply the following lotion: Spirit. ammon. aromat., tinct. canth., glycerine, aa. ℥jss., aquæ rosæ, ℥vi. M. In *syphilitic* alopecia the best internal treatment is the biniodide of mercury. In cases arising from chronic skin disease our first effort must be to cure the local disease, afterwards using stimulating applications if necessary. (Dr. J. M. Frodsham, p. 138.)

Alopecia Areata.—Carbolic Acid.—In a case related by Dr. Watson of Edinburgh, carbolic acid dissolved in glycerine, in the proportion of one drachm of the acid to three ounces of glycerine, was used with very good effect. All crusts were carefully removed prior to its use. The lotion was

applied night and morning, and, to prevent it evaporating, a cap made of oil-silk was constantly worn. The head was washed night and morning before the lotion was applied, with black soap and water. Carbolic acid may be dissolved in acetic acid, and then forms an excellent application in some of these skin affections. (Dr. J. Watson, p. 139.)

HOSPITAL GANGRENE.—Nitric acid must be immediately applied to a wound having a tendency to hospital gangrene, or water must be used very copiously; the first to destroy the whole secreting surface, in the hope that on the separation of the eschar a healthy wound will result, the latter that the secretion from the poisoned wound may be diluted and quickly removed. The acid must be applied very thoroughly, and if the integument is undermined at any point, the acid must be there inserted. Chloroform may be administered. The second plan of treatment may be carried out by placing the part in a warm bath, the water of which should be often changed, and should be medicated with some disinfecting fluid. In all cases, where practicable, a change of air is the grand desideratum; removal from one room to another, or from a town to the country. The actual cautery has been applied to a gangrenous stump most successfully by Mr. Curling. (Mr. C. F. Maunder, p. 153.)

ITCH.—Of 37,429 persons treated in the following way at the Hospital St. Louis, only 535 required a second application. For half-an-hour rub the whole body except the head with black soap, and let the patient continue the process for half-an-hour afterwards in a tepid bath. On leaving the bath, he must be submitted to a rapid and general friction with the following ointment: Lard 64, sulphur 20, subcarbonate of potass, and water of each 8 parts. The patient must then dress himself without wiping off the ointment, as the contact of this is necessary for the destruction of any remaining acari, and of any that might remain in the garments. (Dr. Hardy, p. 142.)

Fumigation by sulphur has been much employed in Paris lately, and with great success. Throw half-an-ounce of sulphur, mixed with two drachms of nitre, into a warming pan of hot coals, which must be employed in the usual manner of warming a bed. The patient must then strip naked and get under the clothes, which are to be closely tucked round his neck and shoulders, so as to prevent as much as possible the escape of gas. The worst cases may be cured in this way without any inconvenience to the patient. (Dr. Gale, p. 143.)

PORRIGO.—The most successful plan of treatment is to give cod-liver oil and iodide of potassium internally, and to apply externally sulphur ointment. This latter is the "basis" of successful treatment. All scales must be carefully removed before the ointment is employed. The disease occurs among the lower orders, and especially in delicate and strumous subjects, and is eminently contagious. (Dr. W. Tilbury Fox, p. 140.)

PRURIGO.—*Its Diagnosis from Itch.*—To distinguish prurigo from scabies, remember that the former is papular, and frequents the back of the limbs and posterior surface of the trunk; the latter is vesicular, and frequent the anterior surfaces. In chronic prurigo, some papules may present at their summit a *small* vesicle; but this must not mislead, nor must the fact that the vesicles of itch rest sometimes on a papuli-form induration. (Dr. Duchesne-Duparc, p. 141.)

The best topical application is tar, in suspension or incorporated with lard and united with opium. The following is a good formula: Tar 60 grains, gummy extract of opium 15 grains, lard one ounce. M. Cazenave, considering prurigo as a neuralgia of the papillæ of the skin, suggested the usefulness of aconite, and it is found to be of great service in allaying the itching. It may be applied externally and administered internally. (Dr. Duchesne-Duparc, p. 141.)

[We ourselves always give alkalies freely internally, and find no difficulty in curing recent cases by this treatment alone; if of long standing, we also employ alkaline baths and friction with some alkaline ointment. We cannot *recollect* any case in which this treatment has failed.—EDITORS.]

VENEREAL AFFECTIONS.

BUBO.—When pus has formed in a bubo, the best plan is to make a small but dependent opening, the moment its presence is detected. The practice of waiting until more complete suppuration has occurred, as well as that of free incisions, are both bad, for in the former case the skin and fascia overlying the gland are usually destroyed and slough away, producing a deep and tedious sore, and in the latter case the incisions do not heal, and leave unsightly and indelible scars. A real chancrous bubo, showing no tendency to heal, should be dressed with a strong solution of nitrate of silver, sulphate of copper, black-wash, or some similar application, or they should be injected into the interior of the bubo. (Dr. Veale, p. 197.)

CHANCRE.—In the treatment of soft chancre the three following rules may be regarded as absolute :—1st. That every non-indurated chancre should be thoroughly cauterised if the ulceration be progressive or stationary. 2nd. That the escharotic employed should never be of less strength than the fuming nitric acid. 3rd. That every suspicious excoriation should be treated as a chancre. (Dr. Veale, p. 197.)

Indurated Chancre.—Excision.—Until an indurated chancre has been followed by enlargement of the lymphatics in the neighbourhood, it is both justifiable and expedient to obtain the destruction of the part affected by it, for which purpose excision is especially applicable when the sore happens to be seated on any part which, like the prepuce, admits of ablation without detriment or disfigurement to the patient. (Dr. H. Veale, p. 198.)

SYPHILIS.—*Mercurial Vapour Bath.*—The way to cure or eradicate syphilis, is by remedies applied to the skin, and not taken by the mouth. There can be no doubt that the treatment by moist mercurial vapour is, in a vast majority of instances, the method to be preferred before all others. The calomel and bisulphuret are the most suitable preparations for moist fumigations. (Mr. E. W. Pollard, Mr. Langston Parker, pp. 201, 204.)

AFFECTIONS OF THE EYE.

CORNEITIS, AND INFLAMMATION OF THE ANTERIOR PART OF THE EYE.—Evacuation of the aqueous humour is a most valuable remedy in the treatment of corneitis, and such anterior internal inflammations as involve the iris and cornea. The irritation resulting from the intraocular pressure occasioned by the increased secretion of aqueous fluid, opposes the beneficial operation of the medicines employed. Often a single performance of the operation is sufficient; sometimes the operation may require to be repeated two or three times. (Mr. T. Wharton Jones, p. 184.)

EXTRACTION OF CATARACT.—*Critchett's Vectis Spoon.*—The author advocates the employment of a very small and peculiarly-shaped spoon (figured at p. 176) for the extraction of cataract. It will without resistance glide between the posterior surface of the cataract and the hyaloid membrane, and will accurately adapt itself to the posterior surface of the lens. On its withdrawal it will bring away the cataract, or the firm nuclear portion of it, unbroken. The extremity of the spoon is very sharply recurved so as to form a fine hook, which offers no impediment to the introduction of the spoon, and by which the cataract may be drawn easily and surely through the corneal opening. (Mr. G. Critchett, p. 175.)

Extraction of Soft Cataract by Suction.—Teale's Suction-Curette.—All cases of soft cataract in which the contents are fluid or semifluid, are much more easily and safely cured by withdrawing the soft contents by suction than by the method of treatment now in vogue, known as "linear extraction." The pupil having been dilated and the speculum applied, freely tear the anterior capsule of the lens by two needles passed through the cornea on opposite sides; so free must the laceration be that the anterior capsule may

curl back and be lodged behind the iris. The posterior capsule must be uninjured. Now withdraw one needle, the other being left to steady the eye with, and introduce the suction-curette (procurable from Weiss, see woodcut at p. 173), first puncturing the cornea with a needle the exact size of the curette. The mouth of the operator must then be applied to the small india-rubber tube connected with the instrument, and the whole of the contents of the cataract may be withdrawn from the eye. In the majority of cases recovery is most speedy and perfect, the operation being followed by little or no irritation of the eye. (Mr. T. Pridgin Teale, Junr., p. 171.)

GLAUCOMA.—Great caution must be exercised in applying belladonna or atropia to the eye in glaucoma, for the peculiar effect of the drug on the arterial capillaries of the eye deteriorates the visual powers considerably. 'Belladonna diminishes the calibre of the arteries by producing contraction of the middle or muscular coat. The fundamental morbid condition of the eye in glaucoma appears to be great venous congestion, with diminished calibre of the arterial capillaries. (Mr. T. Wharton Jones, p. 187.)

GRANULAR CONJUNCTIVA AND PANNUS.—*Inoculation with Pus.*—In cases of granular conjunctiva and pannus the treatment by means of inoculation with purulent matter is still pursued with success. The quality of pus varies very much, some producing a mild inflammation, others a very violent form. The pus from the eye of a child suffering from purulent ophthalmia of a mild type, is frequently most suitable. When inflammation has resulted from the inoculation, the discharge must be washed away from the eyelids every hour with tepid water, if the pain is very severe, but only twice a day if there is very little pain. The result in severe cases of pannus or granulations is unsatisfactory if the inflammation has not been sufficiently severe, or has been checked too soon, for it is a *sine qua non* that the granulations be entirely destroyed and the conjunctiva smooth. Minute red granulations, giving the palpebral conjunctiva a red velvety appearance, must not be treated by inoculation. The large red, or gray red, or gray or yellow and opaque granulations, are those which will disappear when inoculated. (Mr. C. Bader, p. 160.)

The greatest success results in cases where the lids are not only severely granular, but the whole cornea is completely vascular, semi-opaque, and the pupil scarcely visible, where in fact there is little to lose and all to gain. Where the cornea has become a pannus, and but little more than perception of light remains, this plan of treatment is especially applicable. (Mr. G. Lawson, p. 165.)

IRITIS.—We must, from the very first, keep the pupil as much as possible out of harm's way. The fiftieth part of a grain of a very finely levigated powder of sulphate of atropine applied to the conjunctiva of the lower lid will usually effect this at once, and if occasionally repeated will maintain this condition. When this is effected our constitutional remedies are of use, but not until then. A solution of atropine of the strength of two grains to the ounce is usually sufficient for dilatation of the pupil, if a solution of the alkaloid is preferred to the powder. (Mr. Jabez Hogg, p. 169.)

NUCLEAR CATARACT.—*Iridesis.*—In cases of congenital cataract where the nucleus only of the lens is opaque, the margin being perfectly transparent, the most successful practice that can be adopted is to make an artificial pupil by iridesis, for the healthy part of the lens still remains, and consequently sight is pretty good. There must, of course, be sufficient clear margin of transparent lens to ensure an artificial pupil at least equal in its area to the average size of the natural pupil. By performing two operations on each eye in such a way that by the second operation the pupil assumes a crescentic form with the two cornua cut off, the pupil is made of the proper size to admit the light freely. (Mr. G. Critchett, p. 177.)

STRABISMUS.—Squint may be cured without any operation at all, by inducing by optical means an increased action of the enfeebled rectus externus. A prismatically-ground spectacle glass, with the base turned in the direction opposite to that of the squint, restores binocular vision, the eye however remaining squinting; by, however, making the prism a little too weak, the

external rectus is induced to contract, to avoid the slightly-double image. At first the effort cannot be long sustained, but by using the glasses at intervals only during the day, and gradually lengthening the period of exercise, the patient will soon be able to wear them much longer at a time, and fuse the images more thoroughly and with less effort. As the muscle becomes stronger, the prism may be decreased in strength, and the squint gradually entirely removed. In a case related the cure occupied eight weeks. (Mr. Ernest Hart, p. 158.)

Moorfields Operation.—The steps of the operation for strabismus as practised at Moorfields are as follows: The spring speculum having been applied, let an assistant seize the conjunctiva with a pair of rats-toothed forceps close to the outer margin of the cornea, and turn the cornea outwards. Let the operator seize a fold of the conjunctiva with a similar forceps, nearly midway between the inner edge of the cornea and the caruncle, just over the lower border of the rectus internus, and snip it through with slender blunt-pointed scissors, so as to make a small horizontal incision. The facia which this incision exposes, is in turn snipped through. Next a blunt hook, with rather a large curve, is passed through the wound beneath the tendon, which is put on the stretch by slightly raising the hook, and divided with small snips of the scissors between the hook and the sclerotic. This operation is slightly modified by Von Graefe, see page 157. (Mr. J. W. Hulke, p. 156.)

Critchett's Operation.—Divide the conjunctiva near the inner border of the cornea, and dissect it back in a flap, together with the fascia and the tendon of the rectus internus, which must be divided from its acquired faulty place of insertion. Close the wound at the inner side of the eye with fine silk stitches, after removing so much of the free edge of the conjunctival flap that the closure of the wound draws the sunken caruncle forwards, and gives the cornea a slight convergence. The divided tendon which has been brought forward with the fascia and conjunctiva, unites itself to the globe at an anterior point, nearer the situation of its normal insertion. Slight convergence is always desirable at first, for the cicatrix always yields a little. (Mr. J. W. Hulke, Mr. Critchett, p. 157.)

Strumous Ophthalmia.—The disease known as strumous ophthalmia is not really an essentially scrofulous disease, but is an affection of simple character, depending upon disorder of the digestive organs. There is usually more or less remittent fever, worse towards evening, and the organs of digestion and the secretions are very unhealthy. Tonics, and the local application of nitrate of silver, are not followed by much success. Tartarised antimony should be given in minute doses at frequent intervals, until free vomiting is excited; and this in all cases, however attenuated or however stout the child may appear, the object being to relieve the stomach of its offensive contents, and to render its secretions more natural, as well as those of the other secreting organs. Four grains should be dissolved in eight ounces of water, and for a child under three years of age, two teaspoonfuls should be given every ten minutes until vomiting ensues. This treatment should be repeated daily until the intolerance of light begins to diminish, which usually happens in two or three days. The emetic may then be discontinued, and a powder of calomel and rhubarb given every night or every alternate night, but if there is great debility it is better to omit the calomel. (Mr. H. Hancock, Dr. W. Price, pp. 162, 165.)

MIDWIFERY, AND THE DISEASES OF WOMEN, ETC.

Albuminuria in Children.—The only form of disease of the kidney which occurs during childhood is increased vascularity or catarrh of the kidney, leading to increased secretion of epithelium, to such an extent as to choke up the tubes, and produce a marked mottled appearance on the exterior of the organ. The aim of treatment must be to keep the tubes clear; this done, the disorder will generally right itself. We must pass as much *water* as possible through the organ, which will wash the tubes without

producing irritation. The diet must be fluid, but nutritious, and from two to four pints of spring water administered in the twenty-four hours. When the active symptoms have disappeared iron is required. (Dr. W. H. Dickson, p. 268.)

Amenorrhœa and Dysmenorrhœa.—*Apiol*.—Apiol, the active principle of parsley, is a nervine tonic, and is therefore of use in those cases only characterised by depressed nervous power. In cases where there is some derangement of a vital element, or where there is plethora or anæmia, it is useless, and treatment must be directed to remove these conditions. It is peculiarly of use in uterine affections. As a neurotonic it supplies to the nervous system the energy it has lost, and is the best emmenagogue for cases of amenorrhœa depending on this loss of nerve power. It must be administered in doses of two capsules daily. (Dr. Corlieu, p. 258.)

Deficient Action of the Uterus.—*Borax*.—Two scruples of borax, dissolved in a little water, when the uterus is disposed to act, produces constant and unremitting contraction of that organ, which speedily puts an end to the labour. It has the advantage over ergot of seldom producing sickness. (Mr. E. H. Moore, p. 227.)

Diagnosis of Uterine and Ovarian Tumours.—It is very difficult, perhaps impossible, to distinguish between a multilocular ovarian cyst and a fibro-cystic uterine tumour, when the cysts are large and the connection with the rest of the uterus elongated, the peduncles in these uterine tumours being often two to four inches in length. There is usually more or less hemorrhage in the case of uterine tumour in the early part of the case, whilst in ovarian disease menstruation is, as a rule, scanty, and hemorrhage rare. Of course, when there is no peduncle to a uterine tumour, the uterus will move along with the tumour. Not a few of these tumours have been removed by abdominal section, some in mistake for ovarian cysts, others after a correct diagnosis. But such tumours are better not to be interfered with by operation, seeing that they differ most essentially from ovarian tumours in the prospect of life to the patient, for they usually cease to cause hemorrhage and to grow. (Mr. Spencer Wells, and Dr. W. T. Gairdner, p. 240.)

Diet of Child-Bed.—The system of low diet usually pursued is entirely wrong. There is great nervous exhaustion, and frequently considerable loss of blood. The rational treatment is a restorative one, it involves rest, and if possible, sleep. Animal food is especially called for, combined or not according to circumstances, with liquid containing alcohol. (Dr. Graily Hewitt, p. 228.)

Distended and Painful Breasts.—In cases of distended and painful breasts, a good application is the following, spread on linen: Cer. flav., ol. olivæ aa. ℥ij.; glycerine ℥ij.; emp. resinæ ℥ss.; emp. belladonnæ ℥ss. Misce. ft. emplast. (Dr. Hardy, p. 257.)

Fibrous Tumour of the Uterus.—*Incision of the Os and Cervix*.—To destroy these growths by breaking up their structure proceed as follows. Divide the mouth and neck of the uterus up to the inner mouth by the hysterotome; and when the cut surfaces have healed gouge the tumour so as to break up the capsule. Hemorrhage invariably ceases after the mouth and neck of the uterus is incised. To incise the mouth and neck of the uterus, press the perineum down with the bent speculum, then seize the superior lip of the os uteri, and hold it firm, an assistant steadying the speculum. In this way a perfect view of the os and cervix may be obtained. Having determined to what extent to incise, introduce Simpson's hysterotome within the os and cervix, and cut first on one side and then on the other. Then plug the cervix and the vagina most thoroughly and firmly with lint soaked in sweet oil, fixing the whole with cotton wool, so that hemorrhage is prevented, and air excluded from the cut surfaces and the uterus. (Mr. I. Baker Brown, p. 222.)

Enucleation of.—The tumour must be well pressed down into the pelvis by an assistant. The first incisions must be very free and pass deeply down into the tumour, thus not only completely dividing the capsule, but facilita-

ting its bisection should that afterwards be found requisite. The opened capsule must be separated by the fingers, or, if needful, by blunt-pointed scissors, the finger being used as a director. Strong and large vulsella, with midwifery forceps, should be at hand, to be used for traction if necessary. The grand object is to draw down, after separation of the tumour, the uterus inverted with the tumour to the external parts, or as near as possible to them, which facilitates the operation. After separating the final attachments of the uterine tumour, carefully avoiding cutting through an inverted pouch of peritoneum, the uterus must be returned. (Mr. Hutchinson, p. 238.)

Enucleation by the induction of Gangrene.—It is now well established that free incisions in these tumours suffice for their cure. Sloughing and disintegration of the tumour ensue, it diminishes in size, and finally disappears. When death ensues from this treatment it is always from absorption of the putrid matter, hence the absolute necessity of very frequent injections and washings with disinfectants, and obviating the production and long contigaity with ulcerated surfaces of effete and putrid matters. (Dr. C. H. F. Routh, p. 238.)

Inversion of the uterus certainly renders the operation easier, but it also increases the fatality of it by inducing inflammation in and about the peritoneum around the organ. In some cases fatal from peritonitis the peritoneum was found quite uninjured, and traction alone can be assigned as the cause of the inflammation. It should therefore be avoided if possible. (Dr. C. H. F. Routh, p. 236.)

Uterine Hemorrhage from Fibrous Tumour.—In cases of severe hemorrhage from fibrous tumour of the uterus, if the os and cervix be freely incised at each side the bleeding will cease. This is far more effectual than any other treatment which can be adopted. (Mr. I. Baker Brown, p. 222.)

Incontinence of Urine in Boys.—Let the bladder be emptied the last thing at night, and then a common elastic india-rubber ring applied on the root of the penis. This may be removed in the morning. If the bladder is irritable during the day, it may be kept applied continually. This is, of course, useless in cases depending on stone in the bladder, or on worms in the intestines. (Mr. J. Adams, p. 271.)

Mammary Abscess.—*Iodide of Potassium.*—The external application of belladonna is not always so successful as an antilactescent as might be wished. Iodide of potassium in doses of three grains every four hours, with a little Dover's powder at bed-time, will frequently succeed in arresting the secretion of milk in cases of threatened mammary abscess, better than belladonna does. (Dr. F. H. Morris, p. 231.)

Nervous Headaches.—*Turpentine.*—Nervous headaches in women must not be confounded with neuralgia, or even with hemicrania. It may last for months without intermission. One of the best remedies is essence of turpentine. It should be given in capsules, at meal times, each capsule containing eight drops of the essence. (Prof. Teissier, p. 260.)

Ovarian Dropsy and Ascites.—*Diagnosis of.*—In ascites the flanks and sides protrude most, whilst in ovarian disease the protrusion is generally most evident in front, less so at the sides, and often more so at one side than the other. As the fluid of ascites gravitates much more readily than a cyst can move, the form of the abdomen is much more affected by change of position in ascites than in ovarian disease. The umbilicus, though it may be raised to a level with the skin of the abdomen, is never prominent in ovarian disease, as it very often is in ascites, or when ascitic fluid surrounds an ovarian tumour. In ascites the greatest circular measurement is at the level of the umbilicus; in ovarian dropsy it is often some inches below that level. The other means of diagnosis, more evident even than these, are well known. (Mr. T. Spencer Wells, p. 253.)

Injection of Iodine.—There is not nearly so much danger to be apprehended from tapping and injecting ovarian cysts with tincture of iodine as is generally supposed. The danger is not increased by using large quantities of the tincture. Four ounces of the strongest tincture of iodine of the Edinburgh pharmacopœia is the average quantity to be used, and after the parietes of

the abdomen have been pressed to secure the contact with the fluid of every portion of the interior part of the sac, the mouth of the canula should be opened, and as much of the fluid as will, allowed to escape. Many cases have proved fatal from the trocar having been allowed to slip out of the walls of the sac, and the iodine tincture having escaped into the peritoneal cavity. The danger from absorption of iodine can only arise from carelessness by the slipping of the canula out of the sac walls, for when confined to the interior of the sac there is no absorption into the system. Injection of iodine is only suitable for cases where there is positive evidence of one large cyst, entirely free from any solid deposits; and in proper cases when carefully selected it may succeed, and is at any rate well worth trial, as extirpation may thereby be avoided. (Dr. C. Clay, p. 249.)

OVARIOTOMY.—It is much better in tying the pedicle to pierce it and pass ligatures round each half, and tie them separately; the ligatures are thus thrown off much sooner, and the recovery hastened. The system does not rally well towards improvement until the suppuration of the pedicle has ceased, and the ligature is thrown off. (Dr. C. Clay, p. 249.)

Sutures best adapted to Ovariotomy.—After trying extensively every kind of suture, the author has come to the conclusion that nothing answers so well as fine, strong, pure silk. It is easily applied, easily removed, and, if not drawn too tight nor left too long, causes no sloughing of the enclosed circle of tissue. The sutures should not be left more than forty-eight hours. He considers them preferable to any kind of wire suture. (Mr. T. Spencer Wells, p. 256.)

POLYPUS OF THE RECTUM IN CHILDREN.—There is always hemorrhage, more or less, when the child has a stool, and a red tumour will then be observable at the orifice of the anus, which returns after the evacuation. It is usually mistaken by the friends for prolapsus, or for a hemorrhoid (which never occurs in infants.) If followed by the finger into the rectum there cannot be any mistake, for the pedicle will be easily felt. It must be seized with forceps, when protruded during an evacuation, and ligatured around the peduncle, the thread being allowed to return into the rectum with the tumour. It will come away in a day or two. (M. Guersant, p. 267.)

PUERPERAL DISEASES.—Puerperal peritonitis, puerperal fever, and phlegmasia dolens, are not owing, as is thought by many, to inflammation in the neighbourhood of the uterus, but to the introduction of sceptic matter into the uterine veins. This is usually owing to a relaxed state of the uterus. Perfect contraction of the womb is almost a complete safeguard against these diseases. None of these diseases require an antiphlogistic plan of treatment. (Dr. Graily Hewitt, p. 228.)

RETROFLEXION OF THE UTERUS.—*Treatment by Operation.*—The muscular tissue of the uterus is not passive in retroflexion, there is probably active contraction at the point of flexion, and this is proved by the success attending the division of the os and cervix. Having fixed the uterus, introduce the hysterotome within the os and cervix, and divide the parts freely up to, but not through the internal os. Without adopting any other means, excepting rest, the uterus will resume its normal form. The incisions and vagina must be well plugged with oiled lint to prevent hemorrhage. The ordinary treatment for retroflexion, viz., rest and the use of the stem pessary, is very unsatisfactory, and does not *cure* the case, being only a palliative. (Mr. I. Baker Brown, p. 246.)

SEA-TANGLE TENTS.—The opinion of Professor Simpson on these tents, is that they fulfil the indications of a tent so admirably, that he believes they will ultimately replace the sponge and other substances still employed. (p. 247.)

These tents are very liable to slip out, but may be easily held in position by a piece of sponge placed below them, or by a piece of worsted wrapped round the point, until they begin to dilate. (Dr. A. R. Simpson, p. 247.)

TURNING *versus* FORCEPS.—*Disproportion between the Fœtal Head and the Pelvis.*—Supposing equal skill and dexterity be brought to the application of the long forceps as usually is to operation of turning (which is seldom

the case), the use of the forceps is the safer and more successful plan. The pressure of the forceps does not appear to exert any injurious effect on the child's head; whereas in extraction after turning, the head and neck are often so twisted in their passage through the pelvis, that life is destroyed. The pressure of the head on the funis is probably often the cause of death, but in many cases the death of the child is more probably dependent on injury to the neck. Taking the general run of cases, however, delivery can be effected more easily by turning than by the application of the forceps, for, apart from the frequently contracted state of the os, there is often great difficulty experienced in applying them, and there is also liability to laceration of the uterus. Turning is an operation comparatively safe in itself to the mother. (Dr. Graily Hewitt, p. 209.)

Uterine Sound with Movable Head.—In order to obviate the constant bending, which is necessary in the immovable sound to make it tally with the varying obliquity of the uterus in different individuals, the author has introduced a sound with movable head. (See woodcut). The movable part is exactly two and a half inches in length, and can be set at any angle by a screw in the handle. The same instrument may be of use in cases of anteversion or retroversion, and to bring on labour in some cases of retroversion of the pregnant uterus. (Dr. J. Lumley Earle, p. 226.)

MISCELLANEA.

Action of some Spring Water on Lead.—It happens occasionally that spring water contains hydrochloric acid. This can only be detected by evaporating at a low temperature. The free acid acts readily on lead, and the water produces symptoms of lead poisoning when drunk. This acid may exist in spite of the presence of earthy carbonates in the soil, and in very considerable amount. (Dr. Henry Osborn, *Medical Times and Gazette*, *May* 28, 1864, p. 586.)

Action of Bromide of Potassium in Inducing Sleep.—In cases of loss of sleep attended by, or caused by over mental excitement, bromide of potassium is a remedy worth trying. In one such case, after opium and other sedatives had completely failed, twenty-five grains of bromide of potassium, taken three times a day, so quieted the state of nervous excitement that sleep was induced. It should be taken before meals. Other such cases are related, in all of which the nervous element preponderated in relative disorders, in fact just such cases as are usually made worse instead of better by opiates. (Dr. Henry Behrend, *Lancet*, *May* 28, 1864, p. 607.)

Chlorodyne.—*To Prepare.*—Dissolve eight grains of acetate of morphia in ℥ss. chloroform, by the aid of a gentle heat; then mix this with ℥i. of treacle, with the aid of ℥i. of gum water (not adding the whole of the chloroform at first). Lastly add ℥ii. sulphuric ether, ♏xxiv. Scheele's prussic acid, and ♏iv. ol. menth. pip. The whole will make rather more than an ounce of chlorodyne. (Mr. J. Ashburner, *Lancet*, *June* 11, 1864, p. 688.)

Chloroform.—Free admixture of air with this anæsthetic is of the first importance, and guaranteeing this, any apparatus may be employed. Chloroform should invariably be given slowly, and sudden increase of the strength of the anæsthetic is most dangerous. The occurrence during its administration of sudden pallor or of lividity indicates danger; also flickering of the pulse or feeble or shallow respiration. If symptoms are urgent, pull forward the tongue, clear the mouth and fauces, dash cold water on the face and chest, and aid the respiratory movements by rhythmical compression of the thorax. In the most threatening cases artificial respiration must be commenced instantly. In cases of fatty, weak, or dilated heart, great caution is required, but valvular disease is of less importance. It should not be used in operations on the eye, but in most other cases, even operations on the mouth, it may be used. (Chloroform Committee of Medico-Chirurgical Society, *Lancet*, *July* 16, 1864, p. 70.)

POISONING FROM BELLADONNA.—After emptying the stomach the best plan of treatment is by small doses of opium combined with stimulants, if much tendency to syncope exists. In a case related, ℥xx. of laudanum were given in brandy. Calabar bean should be used to remove the dilatation of pupil. (Dr. Fraser, *Medical Times and Gazette, Oct.* 8, 1864, p. 286.)

SYRUP OF CHLOROFORM.—The following is an excellent form for an anodyne containing chloroform, which will be found to remain combined and to mix readily with either spirit or water. Chloroform, 3iv.; ether, 3iss.; oil of peppermint, gtt. viij.; resin of cannabis, gr. xvj.; capsicum, gr. ij. Macerate for two or three days and then filter. Then take of muriate of morphia, gr. xvj.; hydrocyanic acid (Sch.,) ℥xcvj.; perchloric acid, water, aa. 3ss.; syrup of treacle (or honey) to make in all four ounces. Dissolve the muriate of morphia in about an ounce of syrup, to which has been added the perchloric acid and water, assisting solution by a water bath, and when cold, add the prussic acid. (Mr. T. B. Groves, *Dublin Medical Press, July* 8, 1864, p. 612.)

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THE

RETROSPECT

OF

PRACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M. D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.,

AND

JAMES BRAITHWAITE, M. D., LOND.

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PRACTICAL MEDICINE,

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DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON TYPHUS AND TYPHOID FEVERS.

By Dr. WILKS, Physician to Guy's Hospital, &c.

[Dr. Wilks gives us 12 cases of fever which occurred in Guy's Hospital as illustrative of his treatment of typhus and typhoid without stimulants. As the treatment of fevers, pneumonia, and other diseases, by large quantities of stimulants, has lately become popular with many members of our profession, we advise them to read with attention the following remarks by Dr. Wilks, the Editor of the Lancet, and Dr. Gairdner, of Glasgow.]

With respect to the treatment of fevers (observes Dr. Wilks) I adhere to the old practice of the hospital—that which was laid down by Drs. Bright, Addison, and Barlow, in their lectures, and which they adopted for so many years with success. These teachings of these professors was to the effect that a large number of cases ran their course without any other treatment than careful watching and feeding; that they required no other medicine than a simple saline; but that some needed a stimulant during the progress of the disease, and others required it from the commencement. The question, therefore, with regard to the administration of stimulus, was *when* to give it, and in what quantity. At the present time there are advocates for a universal method in favor of alcohol in all cases of fever, just as there are those who indiscriminately administer ammonia in scarlet fever, and who, when failing to prove its value in all cases, fall back upon the explanation that if alcohol or ammonia be of real service in a bad case of fever or scarlatina, and if these remedies do no harm in the milder forms, it is a good rule to administer them universally. Such a method is not only unscientific, but I believe positively injurious; for in many cases of typhus fever in young people, where the brain has been involved, I have a very strong opinion that the brandy which I have seen given in such cases has been positively hurtful. In my intercourse with medical men I judge that very many are scarcely alive to the fact that typhus fever is very rarely fatal in young persons—the prognosis, indeed, only becomes serious as years are added to the age of the patient,—and therefore that they are too apt to attribute recovery to their remedies. Young persons always do well if left alone. Of this fact I could now quote a large number of cases in proof, and, on the contrary, the few instances which I have seen end fatally have been those in which a large amount of stimulus was given from the commencement of the disease; and, what perhaps is even more to the point, the withdrawal of stimulus in some cases where it was adopted as the method of treatment, has been attended with the most decided advantage.

Although the subject of fever and its treatment may appear exhausted, yet this difference of opinion respecting the use of alcohol shows that this therapeutic struggle must continue for some time longer; for whilst we are witnessing the free use of stimulants in fever, we are witnessing that it was Dr. Graves's plan to administer antimony in the same disease. When deliberating on the merits of such contradictory treatment, many escape the dilemma by believing *that different diseases have* been the subject of treatment, and that

fever has changed its type: they assume, without even a sceptical thought passing through their minds, that the favourable issue was due to the remedy, and therefore their conclusion is a logical one. Such persons, however, have no right to frame this or any other opinion as to treatment unless they are conversant with the natural history of the disease; for, did they quite realize to themselves the fact that in young persons typhus fever is rarely fatal, they might with great advantage state the argument in another form: as, for instance, Will a few doses of antimony, or a few daily ounces of wine, or the abstraction of a few ounces of blood, be sufficient to kill a patient suffering from a disease the tendency of which is to subside spontaneously in the course of a few days? Surely, too, every medical man must have seen cases either under his own charge or his neighbour's, where, from a mistaken diagnosis of the nature of the case, a remedy has been given which above all others he would have discountenanced had his opinion been otherwise: he must, for instance, have seen several grains of opium administered daily on the supposition that the case was one of delirium tremens instead of fever, and yet the patient has done well. If he saw many such instances, he might consider that he had good reason to believe in another change of type. Without, therefore, denying that fever may annually change in character as to some minor features, yet I believe that the argument of its great alteration founded upon the administration of remedies to be a most fallacious one. It is for this reason that I offer a few cases out of a large number which have recovered without the use of stimulants, also cases where patients were cupped and bled and yet had a favourable issue. Instances where a good recovery has taken place in patients who have lost much blood by the nose or bowels must be familiar to every practitioner.

I would not wish to dictate rules of treatment to any of my medical brethren; but my own opinion is that expressed in the first few sentences of these observations. I believe that support and a moderate amount of wine is the best treatment; but I assert that alcohol is not an antagonist to the fever, which runs its course in spite of the administration of the stimulant. I would not say that in many cases of typhus in young people a little wine may not be useful; but the fact still remains, which cannot be gainsaid, that such cases would do well without any stimulant whatever, When the ground is thus cleared we shall know better what we are doing. I might mention that the only two cases which I have seen fatal of late have been those of two students, to whom a large amount of stimulant was given, and who had the care of the most assiduous nurses both night and day. In one of these there were constant convulsive movements during the last five days of life, with coma vigil, and other symptoms resembling those in which the late Dr. Graves would have administered antimony. In this case there was no albumen in the urine, nor in another, which recovered, where convulsions were most violent. Albumen has, however, been very frequently present in other instances.

I will offer one or two instances as examples of several where the patients have made a good recovery without the use of stimulants. These have been mostly cases of typhus, this being the form of fever principally prevailing; but the same remarks are applicable to typhoid. With regard to the use of stimulants, it is important to know how much is needed even if it be agreed that the extreme effect is required. It is said by some that in such a case too much cannot be supplied. But of this I have great doubt; for should it be conceded that alcohol is antidotal to the fever poison, or that it is directly supporting the patient, it may be still possible that a surplus amount may be harmful. I am under the impression that a patient who requires the alcohol treatment may be kept under the influence of the stimulant by taking a tablespoonful of brandy every hour, which amounts to twelve ounces in the twenty-four hours. I have grave doubts about more than this being of service, although I have of course witnessed recovery where larger quantities have been taken.

The other cases of typhoid I give merely to show that loss of blood is not *necessarily* fatal, nor is sufficient to make us regard those patients in whom *it occurs* as having a different type of fever.

[The following commentary on these cases, and on the general practice of using so much alcohol in fevers, by the Editor of the 'Lancet,' is so good that we reprint it entire, and also the experience of Dr. Gairdner, as detailed in our next article. We think the profession will have enough to guide it on this matter, and will hesitate to continue the practice of excessive stimulation which has lately appeared to gain ground.]

In the 'Lancet' of January 28th last, our readers will have read and been interested in reading a series of cases of typhus fever with remarks thereupon, by Dr. Wilks, of Guy's Hospital. The remarkable feature in the treatment of these cases was a negative one—the absence of stimulants. We take credit to ourselves for having brought before the profession from time to time the therapeutical fashion of stimulation which has been so prevalent of late years, and for having suggested doubts as to whether it was a fashion based on grounds which would bear scrutiny.

There are few things more remarkable in the recent history of medicine than the extent to which alcohol has been introduced, and the importance which has been attached to it in the treatment of disease. Coincidently with a greatly reduced use of it in society has occurred a greatly increased use of it in medicine. Judging from the London practice of the last few years, a cursory and casual observer might think that alcohol was a remedy of specific power, the triumphant introduction of which was to make Dr. Todd as famous as vaccination made Jenner, or chloroform Simpson. This use of alcohol has coincided pretty closely with the prevalence of a theory of disease which is enticing from its very simplicity. All disease, according to this theory, is a form of debility. A semi-solid exudation in the tissue of the lung, a layer of lymph on the interior of the pericardium, quick pulses and slow ones, are so many different forms of this universal disease of weakness. All semblance of strength in disease is a mere deception; a burning skin, a bounding pulse, are only weakness in a disguised form. Of course weakness is a somewhat vague thing, defying the definition of the physiologist and the analysis of the chemist; but it is a popular term which every man is unthinkingly apt to think that he understands. This is a great recommendation of a theory—that it be short and comprehensive, and have an easy look; and therefore it has had an extensive reception accorded to it at the hands of busy men who have not much time to spend over the choice of their theories. The only thing more remarkable than the extensive reception of this one notion of disease is the extensive spread of a belief that alcohol in some form or other is the panacea for it. We are not questioning just now the soundness of this discovery. We are simply pointing out that alcohol, as a matter of fact, has become, or let us say had become, under this double theory more important in the weaponry of medicine than almost any other remedy, though there is not a tittle of evidence that it can "antagonize" any one disease in the sense in which quinine antagonizes ague, or mercury syphilis, or (may we say?) alkalies rheumatic fever. We have always had misgivings as to the soundness of this theory. One great discovery has been made of late years—that there is a method in diseased processes; that there is a good "nisus" in the worst of them; that when disease overtakes the body, it finds the body not unprepared to meet it, and make the best of it, but with many provisions for conducting it to a kindly and harmless issue; and that the height of medical wisdom is to recognize these provisions, to place the body in the best natural circumstances, and gently assist all the natural processes of cure. Old medicine erred in treating disease and nature too roughly. This new treatment by alcohol, pushed as it was a few years ago, was scarcely less "perturbing" than the old one. Thirty-six ounces of brandy in twenty-four hours to a girl fourteen years old, with fever and a diseased heart, was apt to be a disease of itself, and was surely calculated to give more work than help to the heart and the excreting organs, to say nothing of the brain, on the efficiency of which we know recovery from disease so much depends. Accordingly we are not surprised to learn of the failure of this plan. It is excusable only as a wild and random rebound from an opposite error, and we should despair of medical philosophy altogether if we did not receive *intimation from time* to time that the days of this fashion are num-

bered, and that a more moderate, a more physiological method of treating disease is destined to replace this.

Dr. Gairdner did good service, in the course of last year, in publishing in the 'Lancet' a paper on the treatment of typhus on a plan according to which the majority of the patients (as many as nine-tenths) got no stimulants at all. We are not unmindful that exception has very recently been taken to this paper by another respected contributor to this journal, and this in our own columns. Dr. Henry Kennedy, of Dublin, after waiting nine months, and finding that no physician challenged the views advanced by Dr. Gairdner, has done so in an able, if not also a somewhat warm, communication, which will be found in the 'Lancet' of Jan. 7th. We apprehend that the simple explanation of Dr. Gairdner's views going so long unchallenged is, that they commended themselves to the profession as being neither unreasonable in themselves nor unsupported by the facts and figures adduced by Dr. Gairdner. We cannot entirely agree with Dr. Kennedy in his sweeping objection to the application of statistics to medical questions. There is room for the application of statistics in medicine. The efficacy of ventilation in hospitals, of lemon-juice in scurvy, of vaccination in preventing small-pox, are great facts which admit of statistical statement, and the statistical statement of them is widening every year the extent of human belief in them, and the application of them. And we fancy that, if the introduction of large quantities of alcohol into the treatment of disease were the boon which the late Dr. Todd thought it to be, the fact would admit at this time of far less statistical disproof than is accumulating against it. There is really in matters of detail very little serious difference between Dr. Kennedy and Dr. Gairdner. They both say that there are many cases of fever which require no alcohol; whereas Dr. Todd's administration of it was well-nigh universal. They both say that death from fever in young persons is exceedingly rare: Dr. Kennedy is not sure that he ever saw one. And yet Dr. Todd found reason to give twenty or thirty ounces of brandy a day to young patients. Dr. Kennedy objects to Dr. Gairdner dividing the ounces of stimulant by the number of patients; But Dr. Gairdner pointed out distinctly that this was no indication of the amount actually given to each patient, for, whereas most patients got actually no stimulant, these who got any generally got considerable quantities. Dr. Kennedy objects, almost with feeling, to any disapproving allusion to Dr. Todd's practice on the principle *de mortuis nil nisi bonum;* but surely it is a stretch of this rule to ask us to concede infallibility to the dead, and respecting the dead at the expense of the living, to allow any such principle to interfere with the settlement of an urgent medical question. Dr. Kennedy objects to Dr. Gairdner's figures as conveying no detailed information; and so far reasonably enough. The figures are valuable as the calculations of an observer known to be reliable, and who repeatedly says that he bases his opinions not only upon figures, but details which it is impossible to represent in figures. Dr. Kennedy objects, lastly, to Dr. Gairdner's comparing the results of his own practice with that of others in other places, asking, "Is Dr. Gairdner sure of the identity of the disease? Is he certain that disease is the same in London as in Edinburgh or Glasgow?" Well, Glasgow has no great sanitary reputation; so that, *à priori*, any difference between it and other places might, without violence, have been presumed to be against it. But we are in possession of evidence of a still more satisfactory percentage of recoveries in cases of fever than that of Dr. Gairdner, and this happens to be in the case of a London hospital and a London physician. Dr. Chambers has published various lectures on fever, and gives a table of 121 cases, of which only 4, or 2½ per cent., died. And the administration of large quantities of stimulants is not a feature of Dr. Chambers's practice. He agrees with Dr. Gairdner in the essential point of supporting the patient, not by stimulants, but by nourishing food. Other leading physicians in London are acting upon the same principles. Dr. Gull was lately reported to be treating many cases of fever without alcohol; and Dr. Wilks's remarks and cases above referred to well deserve the attention of the profession.—*Lancet, Jan. 28, and Feb. 11, 1865, pp. 91, 153.*

2.—TWO MONTHS OF FEVER DUTY IN THE GLASGOW ROYAL INFIRMARY.

By Dr. W. T. Gairdner, Professor of the Practice of Physic in the University of Glasgow.

[The cases admitted into the male wards during the period of Dr. Gairdner's attendance were 68 in number: of these, however, 61 only were typhus or typhoid fevers.]

The treatment, where not otherwise specified, has been conducted mainly on the following principles:—To afford a bland and nutritious aliment throughout the fever, chiefly consisting of milk and water, buttermilk, rice and milk, arrow-root, and beef-tea, sometimes with a little bread, where it was relished. The choice of the special form of diet was to some extent determined by the feelings of the patient, and to a like extent left to the discretion of the nurses, but milk diet in all instances formed the staple nourishment. Farther, it has been considered expedient to employ no medicine *as matter of routine*, so that where no statement to the contrary is made, it may be presumed that no medicine at all, unless it may have been an ordinary laxative, or a little common cough mixture or the like, was administered throughout the disease. A similar remark applies to the administration of alcoholic stimulants, except that I have thought it right, in view of certain questions recently under discussion, to state distinctly, as from the hospital wine and spirit roll, which is kept with very great accuracy, the whole amount of stimulants given in each case; and where none were given, to make an express statement to that effect.

[Dr. Gairdner then describes a long series of the most interesting cases, and concludes a long paper by the following hints on treatment.]

The first lesson that has to be learned in dealing with fever, is at the same time to many minds the most difficult—to let well alone. And those who naturally or by education incline to a more or less active routine practice, are not, I believe, likely to be convinced by statistics, or by any form of abstract reasoning, that such practice is either unnecessary or injurious in such a disease as typhus fever. The argument, to many an irresistible one, will always recur—What! are we to stand by and to do nothing at all, when the pulse is 120, the tongue dry, the brain oppressed and delirious? and it may even be admitted that the question is an extremely embarrassing one for all who are not prepared to answer it out of a matured and carefully studied experience. But in view of facts which have become clearer and clearer in proportion as our knowledge of typhus fever has been defined and rendered more exact, it has become a duty with teachers of medicine, not indeed to acquiesce in a blind expectancy, any more than in any other unreasoning routine, but to take a firm hold of principles based upon experience, and to place these in the light of such practical illustrations as their opportunities, derived from hospital practice, enable them to command.

I would therefore invite the attention of all physicians who admit any doubt as to the proper practice in typhus fever, and especially of all who have still to attain practical experience on a large scale, to the cases here recorded, with the view of showing:—

1. That at least two-thirds, more probably three-fourths, perhaps even a greater proportion, of the cases of typhus fever in the present epidemic, may be successfully treated without either medicine or stimulants; or, at least, with only such slight accidental doses of either as cannot be supposed to influence considerably the result of the fever, as such.

2. That in the cases of typhus fever recorded in this paper the treatment, whether by stimulants, or medicines, or both, has been directed entirely to emergencies and casualties, and not to the subduing of the febrile excitement, or the restraining of the typhoid delirium, or the reduction of the frequency, or increasing the strength, of the pulse; on the principle that fever, with rapid weak pulse and a certain amount of typhoid delirium, are to be regarded as the normal facts of the disease, always observed within certain limits of time, and not to be avoided, or *cut short*, by any known specific treatment; from which it *follows, that remedies, especially* very active remedies, are to be *reserved chiefly for manifest variations* from this normal course; as when tartar

emetic has been given, in small doses with wine, in numerous cases of bronchitic complication, or wine and whisky in obvious tendency to collapse; or to take a less extreme instance, a purgative in accidental constipation, an emetic in gastric irritability, lime-water in diarrhœa, or an opiate in case of protracted sleeplessness in the early stage.

3. That under a practice so regulated, the mortality of typhus fever has been not more than 9·3 per cent. in the cases here presented, which have been treated absolutely without selection, and without excluding a single fatal case on any ground whatever.

4. That in children and young persons, probably up to the age of twenty, or at least fifteen, typhus fever, so treated, is a disease of hardly any danger.

5. That in all cases where a regular daily note of the pulse shows a gradual climax, ending in a maximum rate on the 8th, 9th, 10th, 11th, or 12th days, or even later, and afterwards a gradual decline in the rate, there is no occasion for alarm after the crisis of the pulse has been reached, even although delirium should go on, hardly abated, for several days; and that no sudden crisis is to be expected, or wished for, in such circumstances.

6. That nourishment, of the kinds indicated at the beginning of this paper, is to be carefully administered throughout the course of typhus fever. That free ventilation is also indispensable.

7. That good nursing, and constant watchfulness against casualties; especially watching of the chest, the evacuations, and the skin of the back, are the real "physic" needed by all fever patients; and more particularly by the old, the intemperate, and the subjects of chronic disease, in whom the danger is incomparably greater than in the young, healthy, and temperate.

[The following remarks, from a lecture of Dr. Gairdner on the same subject, are too good for us to omit.]

Milk or buttermilk is with me the staple food in typhus; and I will even say that I know no other food that can be depended on. Yet I see, and always see with a new surprise, descriptions of the treatment and dietetics of fever in which not a word is said about milk, and a great deal about beef-tea, wine, whisky, brandy, and all manner of things supposed to be more strengthening, or stimulating, than milk diet. Now, I tell you frankly that treating fever patients without plenty of milk is a thing that I do not understand at all; for I suppose I have not treated a single case of fever of any kind for the last fifteen years (I cannot make precise statements beyond that date) without milk, and I always proceed on the understanding that milk in fever is the one thing needful as diet—always to be given, and given liberally, whether specially ordered or not. To give wine, whisky, and beef-tea, while withholding milk, is simply, in my opinion, to destroy your patient; and the more wine or whisky you give, while withholding milk, the more sure you will be to destroy your patient soon, because you are thereby superseding the natural appetite (or what remains of it) for a nourishing and wholesome diet, by a diet—if it can be so called—which so poisons the blood and checks the secretions, and alters for the worse the whole tone of the nervous system and of the digestion and assimilation. I believe that infinite mischief has been done in typhus fever, and in all fevers, by giving wine, and withholding or not giving milk. Under a false theory of administering alcoholic food, it has resulted, not only that natural and genuine food has been withheld, but that the small remaining amount of appetite for such food has been obliterated, and not unfrequently, even at an early stage of the disease, the patient has been practically disabled from taking any proper nourishment at all. I know, unhappily, as a fact, that not only doctors, but nurses and patients and patients' friends are readily brought under the influence of this fatal delusion, that alcoholic liquors can, in fevers, take the place of natural food. But it is none the less a fatal delusion, and I warn you solemnly against it; all the more that the patient is absolutely in your hands, and you can obtain no guidance from his natural instincts, if you begin by overwhelming them with large doses of alcoholic stimulants. I have been very careful, at least for fifteen years past, to avoid this error, and I believe *that any success* I may have had in managing fever has been more due to this *than to any other* cause. But I allude to the matter now simply to say that if *you would* observe the natural or normal course of typhus fever, with respect

to the crisis (as proposed at present), you must absolutely make up your mind to feed your patient naturally, and not to stimulate him. An opposite course leads to consequences which I may consider more in detail on another occasion; but what I am anxious to say now is, that it tends to disturb the course of the fever, and to retard the crisis.

Further, you must, if you would observe the natural course of typhus fever, give your patient plenty of fresh air. This is now so very general an admission that I need hardly have alluded to it, were it not that adequate ventilation is still apt to be forgotten in treating fevers in the houses of the poor or in badly constructed hospitals. With adequate ventilation, you must, of course, especially in winter, have the means of warmth; and great attention must be paid to cleanliness, so far as this is possible in bedridden and exhausted patients.

These, I think, are all the precautions necessary, I do not say for the proper treatment of typhus fever, but for the proper observation of its natural course in normal cases.

Now I venture to assert—and I make this assertion not by way of theory or opinion, much less prediction, but as a statement of facts, founded on a rather extensive experience ever since 1855 or 1857—I venture to put it forward as a law, or generalization, with respect to typhus fever: *That in a large proportion of cases typhus fever, left to its natural course, and treated with abundant milk diet and without drugs or stimulants, will have its natural crisis before the twelfth day.* Of course you will understand that I use the word *crisis* in a modified sense; for I have just been explaining that there very rarely is in typhus a true crisis, in the ancient sense of the word. What I mean is, that most commonly before the twelfth day, very frequently as early as the ninth or tenth day, sometimes, though rather rarely, even before the ninth day, indications are to be perceived that the disease is about to terminate favourably; and that those indications are to be trusted, even when some of the most formidable-looking symptoms continue present to a later period. And this I confidently assert to be a very general law, or *norm* (to use a word borrowed from the Latin, but yet not naturalized in English, except in the adjective form *normal*), of typhus fever as observed in Edinburgh and Glasgow of late years. Observe, I do not venture to assert that it has always been so, or will always be so; on the contrary, I have myself remarked the tendency to a delayed crisis as an epidemic fact at certain seasons; but in a large proportion of cases, and excluding certain exceptional groups of cases, it has been so, in the main, in every year from 1857 to 1862 in Edinburgh, and during a large portion of the epidemic of the last two years in Glasgow.

If you wish to have the satisfaction of getting the earliest information of this early crisis in typhus, *watch carefully and constantly, from day to day, or several times a day if need be, the rate of the pulse.* For this is, speaking practically, the guiding clinical fact in the natural history of typhus fever. Making allowances for certain obvious, and a few not so obvious, causes of accidental disturbance, you will find that the pulse pursues a very regular course of rising frequently up to the acmé, and declining frequently beyond the acmé, of the fever; and that although the other symptoms very surely follow the pulse in uncomplicated and regular cases, yet they often follow at some distance behind; so that the crisis can be ascertained for practical purposes, and particularly for the great purpose of guiding treatment, more accurately and effectually by frequent noting of the pulse-rate than by any other method. I have ample proof of this in the cases of the last two months, for which I will refer you to the hospital case-books, and to the abstract in the 'Glasgow Medical Journal' for January, 1863.

This accurate and frequent noting of the pulse-rate in typhus, in connexion with the day of evolution of the disease, is of immense importance as regards the prognosis; for not only are you able to predict the recovery with greater confidence, and at an earlier period than you could do otherwise, but you have often the first intimation of complications, either during the fever or during the convalescence, through some unaccountable departure from the norm, or regular *gradual increment* and decrement, of the pulse. In fatal cases, *where the death is from pure* typhus, the pulse usually becomes more

and more rapid, and at the same time weaker and smaller, up to the very hour of death. In complicated cases, where the complication is not very dangerous, the pulse may be merely quickened for a day or two, or the crisis may be protracted, or the pulse may begin to rise again, after a quite distinct critical subsidence. In very dangerous complications the pulse is out of all bounds, and no rule can be assigned for its irregularities. But in regular and normal cases, being uncomplicated—i. e., not having any positive organic or inflammatory complication,—the decline of the pulse, even by a few beats, before, at, or about the twelfth day, carries as an after consequence the decline of the fever; and, on the other hand, a delay of the crisis very much beyond this is to be regarded with suspicion, though not by any means necessarily of fatal tendency.

But the most curious inference which I have been able to draw from the cases of typhus which we have been observing for the last two months is this:—If, in a particular case, you observe that the pulse keeps steadily to the *norm* or law of uncomplicated typhus, as I have endeavoured to exhibit it; if the rate of the pulse increases steadily and gradually up to the ninth, tenth, eleventh, or twelfth day, and then, at any period between the eleventh and fourteenth day, is observed to be undergoing a correspondingly regular decrease, not rapid, but regular and gradual; *in such a case of regular crisis indicated by the pulse-rate, it is almost insignificant, as regards the prognosis, that the other symptoms continue formidable-looking for days;* for in all such cases that I have observed, without exception, the patient has got well in the end, and that without further treatment. Thus I can tell you from experience, that after a regular crisis of this kind, the tongue may be as black as pitch, and so dry that it feels like a bit of roasted leather, delirium may go on, the patient may be out of his bed many times in a night, the skin may be hot and dry, the eruption brilliant or livid for days together; still, if you have the favourable state of the pulse, and if there is no complication, the *patient will get well.* The pulse is your criterion. Whenever it is fairly turned, and declining in rate, especially if it is at the same time gaining in strength and volume, you may be at your ease, unless there is reason to suspect a complication; and even then you may be at your ease, if the complication is not severe. But observe, *only if you do not interfere.* The proper course in such a case is to take confidence from the pulse, and avoid interference. *You can afford to wait.* And only one more word to-day, but it is a very startling one, and yet true according to my experience: even if the delirium should become worse after the crisis, as indicated by the pulse; nay, more, even if the patient has had little or no delirium before, and *begins* to be delirious for the first time after the pulse has begun to show symptoms of crisis; still it is better to wait, for in all probability he will be quite well in a few days, provided you do not disturb the course of the fever.—*Glasgow Medical Journal, Jan.* 1865, *p.* 385, *and Lancet, Jan.* 21, 1865, *p.* 58.

3.—ON TYPHOID FEVER IN PENICUIK.

By Dr. George Thin.

[Dr. Thin's observation of fever in Penicuik extends over a period of six years, and with the single exception of last summer he has always seen more or less of the fever every year. The village of Penicuik lies ten miles south of Edinburgh at the foot of the Pentland Hills, in the valley of North Esk, at a very considerable height above the sea. The last epidemic which occurred in the village was in the months of July, August, and September, 1863. The first case that occurred was that of a boy aged nine years, who lived in a cottage situated in the outskirts of Valleyfield, in a family of characteristically clean and orderly habits. He took ill in the beginning of April, and died in June. No member of the family had been away from home, nor had any *visitor been living* in the house; in short, no trace of possible infection could *be discovered.* The cottage is provided with neither privy nor water closet.

From this case the fever spread to the family where his linen was washed, and then to many other houses. Upwards of sixty cases came under Dr. Thin's treatment during the three months named.]

I have two things to account for: first, why Penicuik should be notorious for fever at all; and, secondly, the varying conditions that regulate its local distribution. I shall consider these two questions in their order.

I have previously stated that I consider the natural climate of Penicuik to be, with a trifling exception, salubrious. The inhabitants are well and constantly employed, and are consequently comfortably housed, fed, and clad. In general comfort, indeed, they contrast favourably with those of any other Scottish village with which I am acquainted. Such being the case, we are compelled to look for the predisposing causes of disease in the adventitious circumstances specially incident to the conditions of life affecting them.

Avoiding any statement open to controversy, I take it for granted that no intelligent physician will dispute, that one of the ascertained causes concurring in the production of many cases of typhoid fever is exposure to the effluvia of decomposing fæces. Any one who has read the mass of evidence collected on this point by Dr. Murchison and Dr. Budd would probably grant more than this, but so much as this even Dr. Christison grants. In the address delivered by him at the meeting of the Social Science Association in 1863, after exhausting the arguments that can be urged against the connexion between sewer gases and fever, he says, "foul air merely favours its invasion." Why, that is as much as any one ever can say of a supposed cause of an unknown impalpable blood-poison. We all know that other conditions than that of "foul air" are also necessary, but whoever will take the trouble to examine the evidence of facts will find that to be one of the most constant.

In nearly every village of Scotland there is scarcely a cottage which has not attached to it a potato garden, in some corner of which (beside the pigstye, if there is one) is the dunghill or "midden." All the filth of the family is here accumulated for the benefit of the next year's crop, and generally becoming incorporated with earth and ashes, is thus rendered harmless to health. This primitive and natural state of things has long ceased to be the rule in Penicuik. The success of a large paper manufactory has so conduced to the prosperity of the village, that land became too valuable to admit of the old cottage system; large houses of two and three stories, with several families in each flat, having been built to accommodate the increased population. Only one public privy exists in the village; and I speak to what is strictly within my own knowledge when I say, that the private privies have been too few in number, and generally kept in a filthy condition. A pail, with the accumulated excrements of the females and children during twenty-four hours, is an utensil in daily use in very many families.

I believe this state of matters to have been the determining cause of many of the epidemics of fever in Penicuik.

That the point at which this fever breaks out is not determined by the permanent qualities of any particular soil or situation is well illustrated by the topographical descriptions I have given above. During the previous years of my experience, Valleyfield had been singularly free, while the street *b*, which in 1863 had only a few solitary cases on one side, in former years deservedly bore the reputation of being the chief centre of fever. The first, and for six weeks the only case, occurred, as I have previously stated, on the outskirts of Valleyfield. Why the disease should have originated in that particular cottage I am unable to say. It was situated, as regards cleanliness, &c., neither better nor worse than those adjacent. It can be only attributable to a high degree of that susceptibility in the patient which must exist in every case before the poison can take effect.

The second I consider to be a clear case of infection by fomites. All the fæcal discharge from the first patient was emptied in a wood opposite the cottage, and quite out of the way of the second family. Between the first patient's house and that in which his clothes were washed there are two rows of cottages, in none of which there was as yet any fever, so that it is not difficult to believe that the disease was thus conveyed.

The next cases that occurred were numerous and simultaneous in their appearance, and can, I think, be satisfactorily accounted for.

No amount of fæcal accumulation will produce fever without the co-operation of certain conditions of the atmosphere. What these conditions may be I do not pretend to say, but heat and stagnation are certainly parts of them. There is no reason, for instance, to believe that the sanitary arrangements of Penicuik are any better this year than they were last year, and yet up to the time I left in October, there had not been one case of fever in the village. This is easily attributed to the weather, which, although dry there as elsewhere, was never, with the exception of three days in May, close or sultry; strikingly contrasting, in this respect, with the stagnant heat that preceded the outbreak in July, 1863.

It was immediately after the heat that the cases occurred. The cottages were badly accommodated as to privies, and I ascertained, by personal inquiry, that the excreta of the adult women, and of young children, were thrown at night into the bed of the Esk. During the months of June and July, the volume of water in the river was so small that it was all carried away by the mill-lade at a point higher up, the bed of the river presenting simply a succession of shallow, stagnant pools. The fæcal matter not being carried away by the stream, became decomposed by the heat of the sun, and the resulting effluvia must have been inhaled more or less by all the residents of the cottages.

The square block of buildings which I have described as being about sixty yards farther down the stream, and in which three out of the four families inhabiting it suffered severely, is provided with water-closets. These discharge by a single pipe into the bed of the river immediately in front of the dwellings. At this point also the river was stagnant.

A very similar case to the above is recorded by Dr. Ogier Ward, as having occurred at Birmingham in 1838. A small stream, used by those living in its neighbourhood as a common sewer, had dried up. About fifty persons took fever in consequence.

I believe that typhoid fever is contagious, but its course at the time of which I am speaking was due to the causes of quite a different nature. There was nothing in the way of personal communication which could in any way explain its being localized in special centres. The cottages in Valleyfield had daily communication with the shops at Penicuik, and a great number of the workers in the Valleyfield paper-mills live in Penicuik, and pass and repass several times daily; but this intercourse did not affect the distribution of the disease. It is worthy of remark, that though the mills are in the immediate vicinity of the infected houses, the workers who lived in other than the localities indicated, as a rule escaped.

The source of the poison that produced the cases in the street marked *a* was not far to seek. Immediately behind the last of the two-storied houses, several horses, cows, and pigs are kept in a very limited space; and connected with these and some large families of human beings inhabiting the house, there was a constant accumulation of filth. The liquid part of this, after being conveyed in a close drain for a few yards, flows in a small open ditch through a piece of sloping boggy ground, in order to be conveyed to the river. The smell from this drain was abominable. During the summer I heard many complaints on that score from those living near it. It is situated immediately below the fronts of the two concealed cottages; the workshop and the two-storied houses open to it from behind, and all the inhabitants of these houses were necessarily exposed to its effluvia. It is necessary to add, there was no possibility of the cases in *a* being produced by the sewage from the cases in Valleyfield, as there is no link by which to trace any connexion between the two centres of disease. The cases in *a* occurred at the extremity farthest removed from Valleyfield. Besides, as I have before remarked, the cases began simultaneously in both.

I have said that six cases occurred, in six different houses, in the street spoken of as *b*. In three of the houses I could discover nothing special in *the drainage*. It was different with the other three. Two of them were con-

tiguous, and between them there was an offensive drain. This was immediately rectified, and no more cases followed at that point.

The remaining case is interesting. I had been in long attendance on the mother of the family for an ailment of a chronic nature. Every time I entered the house, I perceived a very offensive odour proceeding from the window sink, the drain from which was very short, and had no communication with the drainage of any other house, and I repeatedly prophesied, that if it was not rectified it would produce fever. The odour was not fæcal, and no fæces had ever been thrown down. Shortly afterwards one of the children took fever.

The offending sink remained in use, and within two months the only two children in the house took diphtheria,—the first cases of that disease in Penicuik for at least twenty years.

I think the facts I have just briefly related afford useful evidence to any one anxious to form an opinion on the disputed points connected with this fever. These points, as far as I know, relate chiefly to its origin and contagious properties. The controversy in regard to the first of these has, I think, been chiefly due to different observers attaching too much importance each to the facts of the cases specially observed by himself, and on the strength of these proceeding to too hasty a generalization. Whether or not there be a ponderable but impalpable essence which must in every case of typhoid fever be present as the proximate cause, I think we can never know; but even granting the existence of such an agent, there is no evidence to show that the secondary causes co-operating to its origin are uniform.

That certain effects of atmospheric agency or decomposing sewage have, in many instances, been the chief factors in the production of this fever has been proved, over and over again, on circumstantial evidence as conclusive as any ever brought before a jury. But we all know that identical conditions of filth and weather existed in Scotland for years without a case of this fever ever being heard of. Clearly, then, there must be present an unknown something, a "change of the type of disease" if you like, though the phrase is apt to mislead. It is very important to bear in mind this *tertium quid*, and bring it prominently forward, if we wish to carry conviction in pressing sanitary reforms. Dr. Murchison, Dr. Budd, and other writers, have done an immense service in showing that fæcal accumulations can in certain conditions produce fever; but by exalting the relative importance due to these theoretically, inquirers of a speculative turn of mind have rebounded into the mischievous extreme of denying that they have any specific action at all. Dr. Christison, in the address alluded to above, shys, "I suspect, then, it must be allowed of this disease, as in respect to most other epidemic diseases, that we do not yet know its cause—that foul air merely favours its invasion; but that its true cause is something more specific—some ζύμη or ferment which has hitherto eluded our search. In that case, while we shall do well to encourage better drains, more and better water-closets, and better ventilation of dwellings, still we must not count upon thus extirpating enteric fever."

The unimportant part which Dr. Christison here assigns to defective drainage is in direct variance with facts open to examination by any one who chooses to take the trouble. It might be said, with almost equal force, that the true cause of ague is some ζύμη or ferment, that fens and marshes merely favour its invasion, and that, drain and cultivate them as we may, we must not count upon thus extirpating it.

In classifying typhoid fever with other epidemics, Dr. Christison is guilty of a very palpable fallacy. There are no epidemic diseases common in this country which have the same evidence to connect them with special local conditions. It does not at all affect the question to point to individual instances of the disease occurring in houses whose ventilation and drainage are perfect. These instances, exceptional even in number, do not in any way invalidate conclusions drawn from a large number of facts, and may easily be explained on the ground of unusual susceptibility on the part of the patient, who may *have been exposed to some* other source of the poison without his knowledge. *To found any* argument on a few solitary cases like those ad-

duced, would be as reasonable as it would be to deny the connexion between tetanus and wounds, because a few cases of tetanus had occurred in which no wounds could be observed.

Dr. W. Budd of Bristol has observed and recorded a very interesting series of cases, in which he supposes a number of individuals to have been infected by the alvine discharges from a patient suffering from fever. Reasoning from these, he has advocated the opinion that sewage can produce fever only when it contains discharges from those previously diseased.

In the Lancet, Nov. 9, he assigns two reasons for this belief, both of them very inconclusive. First, he gives instances in which cloacæ, after having existed harmlessly for years, have suddenly become sources of disease, concluding therefrom that they must have received some new noxious element. Dr. Budd's views are so materialistic that he forgets to assign any part to the *tertium quid*, of which I have spoken as always co-operating to produce fever. Dr. Christison can see nothing else than his ζύμη, but Dr. Budd cannot see it at all. This is the real new element which has rendered so many cloacæ fruitful. His second reason is stated thus: "That this fever is caused by something far more specific than the putrescent compounds which exhale from common sewage, seems to be further plain from the significant, not to say decisive fact, that of the two kinds of fever, the intestinal is the only one that originates in sewer influence."

The only fact that that shows is simply what is stated, that typhoid fever has a relation to sewage which typhus has not, It is not even the shadow of a reason for or against the point at issue. It would be easy to multiply instances which are quite inexplicable by Dr. Budd's theory. Of these the following is an example:—During the summer of 1862, only two cases of fever came under my care. The first of the two was that of a young married lady, who occupied a house from which I had removed the preceding May. There was a bedroom in the ground flat of the house, separated from the water-closet by a stone wall. Some defect in the construction of the sewer permitted a communication with the flooring of the bedroom. The consequence was a decided odour, which in damp weather became very strong. For this reason I had, while living in the house, caused the furniture to be removed from this room, and kept it shut up. I warned my successors of the danger of using it, but, on account of its other conveniences, my remonstrances were disregarded. In six weeks the lady took typhoid fever, and swam for her life through a long illness.

No possible source of infection could be traced. No others of the family took ill, the greatest precautions being afterwards used to avoid the source of danger. The house was a new one, and as I happen to know all of the tenants who have occupied it, I am able to state that there had never been, up till that time, a case of fever in it. When this lady took ill there was no case of fever in or near Penicuik.

I shall relate one other instance. In 1863, simultaneously with the outbreak of fever in Penicuik, a number of cases occurred in the Wellington Reformatory Institution, situated on rising ground about two miles south from Penicuik. At that time the buildings consisted of an old two-storied house in the centre, inhabited by the superintendent and his family, and a new building joining it at right angles, in which were two dormitories for about ninety boys. Adjoining, and in a line with the other gable, was a cottage in which the grieve or farm superintendent and his family dwelt. The first case that occurred was in the grieve's family. His daughter, a girl of sixteen, was a domestic servant in a family in Penicuik, but visited her father's house regularly. She was quite out of the way of any infection while in Penicuik. While on one of her visits to her father she took ill. *At the same time* several of the boys belonging to the Institution began to droop; and within six days more, sixteen of the boys, two others of the grieve's family, the superintendent's wife, and two children, had taken ill. There were no deaths.

There were two distinct sources of disease, each of which was sufficient to *account for the* outbreak. The superintendent's wife informed me after her *recovery that she* had been for months conscious of a bad smell about the

house, which was always worse in damp weather. Accordingly, the flooring was lifted, and it was discovered that in and around the house there were old cesspools which had been full for the period of many years during which the house had been uninhabited.

The other source was the cesspools belonging to the boys' privies. These overflowed into a ditch, between which and the Institution there was only the breadth of the highroad. In passing along this road, the smell in hot weather was quite perceptible. There was no possibility of infection from the fæces of a fever patient in this case.

That alvine discharges from a fever patient should be as active, or even more so, in originating the fever poison as ordinary sewage, it is very reasonable to suppose. But facts are quite against the supposition that they are always or even frequently the source. Were such the case, an epidemic of typhoid fever should become more virulent the longer it lasted, because the source of the poison would daily become more abundant. Whereas, at least in my experience, the contrary is the case; a number of cases always breaking out at the same time, the subsequent cases being less numerous and often milder. Besides, the theory breaks down altogether when applied to the numerous instances in which persons take this fever who have never been in any possible way exposed to such a source. Though the exciting cause of fever is, in very many instances, connected with sewage, I think those who limit it to that are in error. It is highly probable that other kinds of organic matter in a state of decomposition can originate the poison, though not so readily. The case of a child who took fever from the foul air of a window-sink, is, I think, in point. Neither urine nor fæces were ever put down, the smell which enabled me to prophesy fever probably arising from the decomposed food which was carried into it by the dish-water.

I think the case of the stinking drain which infected so many families in the street *a* bears in the same way. The filth of which that drain was the bearer consisted chiefly of the excrementitious discharges of cows and pigs. Yet I consider the probability of that drain being the cause of the fever in its neighbourhood is almost as near certainty as such a thing can be. It was the only distinguishing sanitary condition between the affected houses and others in their immediate vicinity. Compare them, for instance, with those on the other side of the street. The inhabitants drank the same water, bought their provisions in the same shops, and were as well housed and clad.

It was not due to infection, for there was quite as much inter-communication with the opposite side of the street as there was with the houses adjacent. None of them had water-closets, so that it could not be a choked sewer or a poisoned drain. The circumstances tie us down to discover a local cause, and no other than the one I have given can be found at all proportionate to the effect.

Two things seem to me to have a strong analogy to typhoid fever.

Students and others who are much in dissecting rooms are liable, especially if from any cause they become weak, to a slight feverish attack, which generally passes off in a few days. The symptoms are depression of spirits, loss of appetite, a slight amount of fever, and diarrhœa, with considerable flatulence. The gas passed by the bowels is fœtid, and can often be distinguished as having the special odour of the subject which the student has been dissecting. Clearly, in this case, the gaseous results of decomposition, after circulating in the blood, are thrown off by the bowels, exciting general and local irritation in their passage. It is highly probable that typhoid fever owes its local symptoms to the inflammation of the glands, caused by the passage of some similar but more virulent poison.

Epidemic cholera is another instance in which a poison produces local bowel symptoms. The causes of cholera and typhoid fever are identical, the different results being the special influence of the climate. The epidemics of cholera, which have ravaged Shanghae for several summers, were first produced by the influx of country people, who fled for protection from the Taepings. They arrived in multitudes which far exceeded the house accommodation of the native part of the settlement. The houses, which they rented by the month, were *each packed by* about a hundred persons, and when they were

turned out at the month's end, the floors were often covered with from one to two feet of excrement.

Excluding cases in which the disease may be conveyed by the patient's stools, the question as to whether it can be communicated by exposure to influences arising from contact or proximity to the persons of the sick, is still undecided. That it is not so contagious as small-pox or typhus, is abundantly evident from the way in which it selects certain houses, or sets of houses, and passes over others. That it can originate without contagion can be shown in many instances in which solitary cases of the disease spring up when there is not another within many miles. But that it is infectious in a certain degree, and especially in certain conditions, I firmly believe. The circumstances of the case render this very difficult of proof. It is evident that before any person can be exposed to contagion from the person of a fever patient, he must be for a longer or shorter space of time in the house in which the patient resides. If fever follows the visit, you cannot prove to your opponent that it was a case of infection, for he immediately replies that the same original sanitary defect that produced the first case produced the second. The possibility of this objection being urged, prevents my quoting many instances which I believe to have been due to personal contact with the sick; but three cases which seem to me to exclude it are, I think, worthy of being recorded.

The first is the case whose history I have given above; the second that occurred in 1863. The husband, who was the first to take ill in that family, had never been exposed to any source of the disease as far as was known, except the bed-clothes brought by his wife from the cottage in which the boy lay who first took ill. There is a considerable distance between the two cottages, and the stools from the boy were emptied on a dunghill close to the house.

The second case was that of a young man who went several times to see his sweetheart who was lying ill, and caught the disease from her. She had been sent home from her master's house, where she had been seized, along with one of his children. As there were neither privy nor water-closet in the cottage in which she lived, and as there was no fever where the young man lodged, I have no doubt that the infection which seized him was due to the ordinary exhalations from her person. His case was slight.

The third instance which I shall give is a very singular one.

In 1861, there lived near Penicuik, in a large new house, with perfect drainage, a boy aged about twelve years, whom, for the sake of reference, I shall name A. A. had a cousin of about the same age, who had come from Edinburgh to spend a few days. The children of an officer in Greenlaw Barracks, which are near the house, were sent to spend the day with A. and his cousin. Three soldiers were sent to erect a small tent for the amusement of the boys. The following day A. was indisposed, and in two days afterwards took to bed ill of a fever, from which he died, after a long illness. A.'s cousin took ill in Edinburgh some days afterwards, and recovered, after passing through a very decided course of the disease. The three soldiers took ill, and one of them died.

There were no previous cases of fever among the soldiers at Greenlaw. The nearest case of fever at the time was in Penicuik, where it was very prevalent. A. had been several times in Penicuik, and the only possible explanation I can give of so many people catching fever at one fortuitous interview is, that A., who was the first to take ill, had the infection about his person. The turf on which the tent was pitched was removed from the influence of drainage of any sort.

A young lady who called at the house on the first day that A. was in bed, finding no one at home at the time, spent an hour in his bedroom. She took ill of fever that day week. There was nothing wrong with the drainage of the house in which she lived.

These and similar cases have confirmed me in the belief that whether we are quite certain of the personal contagion of this disease or not, the only *safety, practically*, consists in acting rigidly as if we had decided in the *affirmative. I think* it highly probable, that persons living in conditions

which favour the access of this fever, are much more apt than others to become infected by contagion, while, conversely, those living in a pure air, while more able to resist contagion, are more amenable to the influence of the poisonous gases. This opinion is, I grant, very speculative, and I apply it exclusively to young people, who are the most frequent subjects of the disease.

It seems to me to be certain, that typhoid fever is the product of several very distinct causes.

First, there must be a susceptibility on the part of the patient. The way some persons withstand constant exposure to the disease, without taking it once, shows this very clearly. This susceptibility scarcely exists after the age of thirty.

Secondly, a cause which is most tangible, and which has therefore been most talked about, is the emanations from decomposing organic matter, nearly always sewage. This has been shown over and over again. To deny it shows either ignorance or prejudice.

Thirdly, heat and stagnation of the atmosphere nearly always exist, though not necessarily.

Fourthly, a thing or things unknown, probably due to a condition of the atmosphere. To this cause must be attributed the varying cycles of the disease, the preceding conditions being much more constant than the prevalence of fever has been.

It would be interesting, but fruitless, to speculate as to how many of these causes must co-operate in its production; whether, if some of them exist in a high degree of force, a less degree of the other or others suffices; or whether, in a still more exaggerated state of energy of one or more of them, the others may, in exceptional cases, be absent. It will be productive of more benefit if medical men will set themselves to the removal of the source of disease which is within their power—filth in all its forms.—*Edinburgh Medical Journal, Jan.* 1865, *p.* 599.

4.—ON THE USE OF STIMULANTS IN FEVER.

By Harry Leach, Esq., Resident Medical Officer to the Hospital Ship Dreadnought.

[Typhoid is the prevailing form of fever received on board the Dreadnought Hospital Ship.]

Of the sixty cases admitted during the past year, wine or brandy was prescribed for thirty-five patients, whereas the remaining twenty-five received no stimulants whatever. Statistics in medicine are so often fallacious, that these figures merely go to prove that the latter were mild, and the former severe cases of typhoid. But the principle of waiting and watching for "the time when" to give stimulants in fever appears to be superlatively worthy of attention. Thus, none of these patients commenced wine or brandy before the eighth or tenth day of the disease, and, in more than three-fourths of these cases, until the fourteenth or fifteenth day. The quantity given in twenty-four hours averaged six ounces, and never exceeded twelve ounces, this latter quantity being sometimes judged necessary for a period of from forty-eight to seventy-two hours at about the eighteenth or twentieth day of the fever.

Four deaths occurred—severally from perforation, double pneumonia, exhaustion, and exhaustion with extensive ulceration of the larynx.—*Lancet, Feb.* 4, 1865, *p.* 124.

5.—ON THE TREATMENT OF MALARIOUS FEVER.

By E. Hare, Esq., Deputy-Inspector-General of Hospitals, Bengal.

My arrangements for the experiment on fever were to give quinine a full trial as an *antidote*. *I gave it*, therefore, irrespective of all variations of

symptoms, except that the more violent and more dangerous these symptoms appeared, the larger, earlier, and more frequent was the dose of quinine given. My assistant was directed to mix a large bottleful of quinine, rubbed up in plain thick mucilage, ʒj. to the ounce. I would not give it in its more soluble form with diluted sulphuric acid, because I feared that in the frequent and large doses I proposed giving, the quantity of sulphuric acid required to dissolve the quinine being considerable, it might injure the stomach; and besides, being an experiment, which I wished to be simple, and free from all objection, I preferred giving the quinine pure. One dose of a purgative, viz., 3j. of compound jalap powder was allowed to be given in some cases, but latterly, especially during the rains, the purgative was very rarely given. All the worst cases, in fact, had no purgative, for I was too anxious to give as much quinine as I could, and as early as possible, and was afraid of losing time by nauseating the patient with other medicines. Moreover, I believe the purgatives delay and prevent the action of quinine, and remove it from the system too rapidly. Sydenham and many other practitioners have observed "that we are in danger of bringing back intermittent fevers if we employ purgative medicines too soon after we have stopped them by bark," and we have the same observation in De Haen ('Cullen,' vol. i., p. 642).

The patients came in at uncertain hours, and I could not be always in the wards. I therefore gave a standing order to my assistants that the moment a patient was admitted he was to have ʒj. of quinine. I saw him myself always a short time afterwards, and gave him another ʒj. or ʒss., according to the urgency of the symptoms. In visiting the patients an assistant accompanied me, carrying the large bottle of quinine described above and a two-ounce measure glass, and each patient had an ounce = ʒj. of quinine given to him, and it was swallowed before me. Ordinary cases took this dose three times a day, but if the fever was dangerous I gave ℥jss. of the solution, or ℥ss. of quinine at once, and ʒj. every three or four hours afterwards, according as I found the stomach would bear it without vomiting. It was very rarely necessary to continue this treatment beyond the second day after admission. The patient was almost invariably convalescent on the third day, when gr. v. doses three or four times a day for about two days more, with good food, completed his cure. Throughout the treatment, if the patient had had his fever long, and thus, being without appetite, had taken no food for some days, I gave him moderate quantities of fluid nourishment without delay—as arrow-root and milk, and soup of moderate strength—and if he were a man of free, or especially of intemperate habits, I allowed him, if he had been ill a few days and without his usual stimulant, a small quantity of brandy or wine.

I may safely remark that in the annals of Medicine such an experiment as the above, in simplicity and decisive results, has never before been recorded. If the table of Mortality No. 3 of Non-Military General Hospital be compared with the Military (No. 4 Table), which forms a part of the same range of buildings, the difference will be seen to be very great. In the non-military Hospital, during the twenty previous years, there was 1 death in every 11¼ admissions for fever, whereas in the military the mortality was only 1 in 30. and the cause of the difference will be at once perceived, from the very different description of cases admitted in each. The military patients are simply the soldiers of the garrison of Fort William, who are sent to hospital on the first appearance of sickness, whereas the cases received into the non-military wards are from the unemployed sailors wandering about Calcutta—in fact, any European who is sick and has no other home to shelter him. Sailors and others are often thus picked up from the roads of Calcutta in an insensible state and sent to the Hospital. They are, therefore, the most hopeless class of cases that can be treated in the deadly climate of Bengal, and their treatment having been so long delayed, becomes most difficult. The large mortality—1 in every 11 cases of admission for fever—for the twenty years before my experiment proves how severe the cases must have been which I treated there. The mortality also for dysentery—1 death *in every 4¼ cases—during* the same period (see the Table No. 3) is another *instance of the enormous mort* -ich had been habitual from the above

cause in the non-military Hospital. My treatment, therefore, comprised the young, robust soldier, treated immediately after his sickness appeared, and the debauched, drunken outcast from the streets and ditches round Calcutta, where they were frequently found by the police after lying there all night.

I will now, having explained what the experiment was, state the deductions which I think must be drawn from the result.

In the first place, no blood was drawn either by lancet or leeches. Bleeding, therefore, is not necessary, and the disease not inflammatory. No opium; no purgatives to bring away bad secretions; no drug of any kind is required, except quinine, for the successful treatment of malarious fever.

Quinine may also be given in the largest doses, whether there are head symptoms, delirium, coma, or pain in the liver. Whether it be in the hot stage or cold, quinine is not only safe for all forms of malarious fever, but its certain cure; and in cases where there is danger to life, the earlier and the larger the doses of quinine which can be given to the patient the better. Vomiting and full cinchonism were my only checks for continuing it in full doses. I mean by cinchonism the peculiar head symptoms which large doses of quinine produce, and when they were fully obtained I considered my patient safe, and that only moderate doses were then required. I always waited, too, till the cinchonism had lessened before I continued even small doses—gr. iv. to v. three to four times a day for about two days longer.

Quinine, therefore, may with reason be pronounced as a direct antidote to the poison of malaria, and not simply as an anti-periodic and adapted only to stop periodicity, for it always cured equally well those fevers in which there were no periods, but which continued without the slightest remission during the twenty-four hours.

I must here also explain a deduction from my experiment which I had long meditated before I went to Calcutta. It had constantly struck me that all the fevers I met with in India were caused by malaria. All infectious fevers, as typhus and typhoid, are unknown in the plains of India among the European part of the population. I have met with only two cases of typhoid fever, with ulceration of Peyer's glands, during my practice in India, and they were both recent arrivals from on board ship, and these cases I saw after my Calcutta experiment. I therefore had come to the conclusion that, with rare exceptions, all fevers in India, whether called ardent, continued, congestive, inflammatory congestive, remittent or intermittent, were all caused by one only cause, viz., the blood-poison malaria. It was my conviction of this truth which justified my indiscriminate and determined dosing with quinine every fever patient admitted into my wards, and I was guided a good deal in my plans by the following passage from Dr. Holland's "Notes and Reflections," p. 150:—

"And as bark by curing associates together many intermittent affections, under the presumption of a common cause, so may colchicum furnish similar inference regarding affections seemingly remote in situation and symptoms. This mode of inference, hitherto very limited, may hereafter be extended much further. It is one of singular value, as closely connecting the treatment with the theory of the disease, and rendering them mutually corrective of each other. It at once enlarges and simplifies our views in every part of pathology; each fact well ascertained is fruitful of results, far beyond those which directly appear to the view."

Again, p. 348, he enumerates these as proofs of a disease having a common origin with ague, viz., malaria:—

"1st. By their intermissions.

"2nd. By their succession to, or alternation with, intermittent fever.

"3d. By the influence of the same specific remedy in removing or relieving such disorders."

He says the applications of these proofs might be largely made.

Again, p. 354:—"The influence of certain medicines, and particularly bark, in curing even the most anomalous varieties of these intermittent disorders, is a fact of great interest. Like the use of mercury in obscure syphilitic affections, or colchicum, *in the most* irregular forms of gout, it enables us to denote *and class together symptoms* apparently the most remote in kind.

but which presumably could not thus be relieved unless depending on some common cause. We have, therefore, in the specific nature of the remedy a sort of practical test of the character of the disease, often of great importance to the consistency and success of our treatment, and related, through the principle of the inquiry, to some of the most interesting questions in all pathology. This it is, as was noticed when speaking elsewhere of colchicum, which gives peculiar value to all which illustrates the action of these remedies. They are the interpreters of facts, far beyond their momentary effects, and of connections between morbid states, which are in no way so definitely made known to us."

The success of this method is acknowledged in establishing the gouty nature of many diseases of the joints, eye, heart, &c., by colchicum; by mercury, in testing the syphilitic nature of many diseases of the skin, iritis, &c.; and by quinine and arsenic, in proving many neuralgias and other very numerous chronic diseases, described by MacCulloch, to arise from malaria. But I believe that I am the first who ever thought of applying the principle, connected as it is with some of the most interesting questions in all pathology to acute disease, viz., all the fevers of India. And let me remark that colchicum and mercury being purgative, and acting freely on all the secretions, may act thus beneficially on all diseases apart from any specific effect; but this cannot be said of quinine, which acts neither on the bowels nor any of the secretions. This reasoning, therefore, applied to quinine may be received with greater certainty than to the others; and yet remarkable and valuable results have been obtained from experiments on the latter. It may, therefore, be received with the greatest confidence that the fact that 421 fever cases, indiscriminately admitted during all the seasons of the year, were, I may say, saturated within two days with quinine, with only three deaths, in two Hospitals in Calcutta, where the usual mortality had been, for twenty years previously, one death in eleven cases, and one death in thirty, proves beyond dispute, to my mind, that all these fevers were of a malarious origin. For, mark, it was not only that quinine was given with impunity—that is, that it did not increase the average mortality, which had been, before its use, enormous.

In reviewing the results of quinine in the treatment of malarious fever, a necessary question arises, How does quinine act? It was introduced to the world as a tonic, and it produces very remarkable head symptoms. So long, therefore, as the congestions of fever were considered as inflammatory, and remittent fever to require active depletion, we need scarcely wonder that it should have been so carefully excluded, especially when the fever caused head symptoms; and it was this prejudice in particular which I especially went to Calcutta to disprove, for I knew that these latter cases were the most fatal, and therefore those requiring the earliest and largest doses of quinine, and the head cases were those which, in my experiment, I especially attacked vigorously with my remedy; and it was soon evident to me, as it will be to any one who tries the large doses, that, so far from exciting, quinine produces the most deadly coldness of the skin, feeble pulse, sometimes intermitting, complete deafness and amaurosis, evidently from its powerfully sedative, almost paralysing, action on the brain. The delirium also it causes is not active, but a low muttering wandering. It is, in fact, to me quite certain that no one had ever tried quinine in acute fever except in small doses, when it does produce often a peculiar excitement, especially of the head; but in large full doses it is the most powerful and alarming sedative. I have during my practice often been frightened with my own results. I recollect one case which made a strong impression on me. It was in Burmah. A young officer of my regiment, Lieutenant M., was sent in from an outpost, having had remittent fever for some days, and he was reported to me as having been delirious the night before, and in great danger. I saw him first about 3 p. m., and found him in the most hopeless state. The accession was just coming on, and I knew that he must die in the night if it was not checked. There was therefore no time for delay.

I instantly gave him 3ss.; at 5 p. m. he took ℈j., and at 7 p. m. ℈j. more, *or seventy* grains of quinine in four hours, and I sat down by his bedside to

watch the result. No paroxysm appeared—it was quenched by quinine as water does fire. His skin became deadly cold, his pulse fluttering, with complete deafness and muttering delirium. He was very restless, complaining that he saw devils and things floating before his eyes. The pulse alarmed me greatly, and I gave him repeatedly a glass of port wine. In the course, however, of about four hours these dangerous symptoms subsided. He fell asleep, and the next day was not employed in giving him medicine, but in giving him food. His fever left him from that night, and he was soon well, and is now strong and robust, and an ornament to his profession. Major S., of the same regiment, was almost a similar case, and I could enumerate numerous others. No one, in fact, living, has given quinine more largely than I have, and though I have as yet had no fatal result from it, I have often dreaded such a misfortune, in cases where I felt justified in giving it in very large doses; and the dangerous symptoms have always been, not excitement, but deadly coldness, arrest of the pulse, and paralysis of the heart. These, however, are the effects of large doses; given in small quantities it produces, in some cases, a peculiar kind of excitement.

I will now close my account of the Calcutta experiment. Its results were published to my service in India by a copy of the report on it being sent by the Medical Board to every Medical officer. I was, at my own request, posted to an European regiment, and I determined quietly to allow the seed I had sown to produce its fruit; and as for myself, with the noble opportunity which the Surgeon of a European regiment has, especially on service in Burmah, I resolved quietly, instead of perpetually writing, to wait my time, collect my results, and see what further experience would teach me.

I have since treated 6982 fever patients, with only one death in every 211¼ cases, and I have to describe but one change in my treatment, which, however, I consider a most valuable and important improvement. I have, in fact, gone backward, it may be thought by some, in my antispoliative treatment, and resort now, I am certain in some cases with great advantage, to mercury.

I have not, however, lost any of my old confidence in quinine. I maintain still that given early and in sufficient doses, it is a perfect antidote; and I would cheerfully go back again to Calcutta and try my experiment again with quinine *only*, and a syringe to wash out the bowels with plain water. With water to wash out the bowels and apply to the head, and plenty of quinine, I want no other medicine for the treatment of the worst forms of tropical fever. Leeches, bleeding, purgatives, emetics, are, if not useless, at any rate unnecessary. You can cure all your patients without one of them. Why, then, do I now use calomel? I will explain.

Quinine, as is shown above, when given in the large doses which are occasionally necessary to save life, in cases where the fever has been of some days' continuance, produces very dangerous and distressing symptoms; though I assert again that I have never yet had a case where the results were fatal, nor do I know any case of mine in which even the patient complained of any lasting bad result from it, and in a regiment I was with so many years I must have known these results, if there had been any. Yet I can easily conceive of such accidents occurring, where very large doses, as ℥j. were given at once, but with only one ℥ss. dose to commence with, and ℈j. doses given at intervals of never less than three hours (and I have rarely, indeed, found more necessary), the symptoms of cinchonism will soon warn you to stop the medicine before sufficient can have been given to poison.

However, besides this, any one who treats many bad forms of fever will often find himself puzzled how to act when the patient, as he often will, vomits his medicine. It may be written down, as an axiom in all cases true, give plenty of quinine to a case of malarious fever and it will be cured; but this is not always so easy in practice as it appears on paper, for if you cannot give the quinine because the patient vomits it, what are you to do? You are reduced to the same perplexity as Dr. Johnson was in his first trial of bark. With the very bad and long-neglected cases in the non-military General Hospital this was the bane of all my pleasure, for I really would have enjoyed my practice there but for the anxiety these vomiting patients sometimes caused me. When I *went into the ward* and asked how Smith or Jones was, the

apothecary would tell me he was very bad, and had brought up all his medicine. There was no time for delay. The patient, if he could not retain the quinine, would probably die in the night; and I would go to his bedside, and persuade him, perhaps with difficulty, to swallow another dose, but before I left the ward, to my disgust, I heard the sound of vomiting again, till sometimes I was much perplexed. However, by not allowing the patient to drink anything, and by giving the quinine in pills, in port wine, in soup, or, if the patient were a spirit drinker, in brandy, I always contrived to give him sufficient to check the fever, and then his stomach would bear more; but still it was only by very careful management that I did so, and always with great discomfort and distress to the patient. In the General Hospital I had no choice. I was obliged there to continue the simple quinine or give up my experiment; but since then, while I was in Burmah with my regiment, and was trying there calomel and quinine in large doses in the treatment of malarious dysentery, I soon became convinced how much one drug assists the other in producing its peculiar constitutional effects. There are many cases of fever which I described above which cannot be salivated with the largest doses of calomel aided by mercurial frictions, but if you add quinine to these doses you will find that salivation quickly appears.

Dr. Morehead notices the same fact, p. 205. He says:—"I can state from my own observation that it is by no means an unusual occurrence in the course of remittent fevers, which in their early stages have required the exhibition of several full doses of calomel, to observe, after the recurrence of the fever has been prevented by quinine, a slight mercurial action to appear on the second and third day, though not more than a few grains of calomel or blue pill, in combination with quinine, had been given during those days." Dr. Watson also, and many other writers on the treatment of intermittent fever, notice how much better and more quickly quinine acts the next day if a full dose of calomel be given overnight,

Again, when I left my Hospital, and had to treat children for fever, I found the little things sometimes obstinately refuse to take large doses of the bitter medicine, and often if they did take it, it was vomited; and I confess that I rarely now treat a young child for bad fever without mixing calomel with the quinine. Its effects on them, without producing any visible constitutional effects, are marvellous, and the quantity of quinine you are obliged to give is greatly reduced. My rule, therefore, now is, if a patient can take quinine in the quantity I think necessary for his safety, I give it only; but if he vomits it often, I give him a full dose or two of calomel, and I prefer the old ℈j. dose to any other from its inimitable effects in checking vomiting. I never have had occasion to exceed two doses: and I find I can then continue the quinine without any trouble to myself or patient. The quinine also need not be stopped, but given with the calomel; the calomel put dry on the tongue, and the quinine given in pills.

To a child I give one large dose of quinine to start with, and try and repeat it, but if the little thing is sick and refuses it, I drop two or three grains of calomel dry on its tongue, and mixing three to five grains of quinine in jelly, I make it into a pill, and put it on the end of my finger into the child's throat. This cannot so easily be done with ten grains, which without the calomel would be required. Very soon, however, you will find that this treatment will check the fever, and you can then stop the calomel, and continue only the quinine. There is no necessity to salivate any one, or give a grain of calomel more than just sufficient to assist the quinine, and in many cases it will be found a great comfort to both patient and physician to give it in this way. I believe myself that as quinine is an antidote so is calomel, and that the two in combination have more powerful and immediate effect than separately. Were we to be so unfortunate as to lose cinchona, which to us residents in the tropics would be as sad a loss as the fire which Prometheus restored, we must resort, I think, to the calomel system again, for bleeding alone and purgatives will not check the paroxysms of remittent fever. It *requires*, as Johnson soon found, something more to propel the stagnant congestion, and the salivation system only began to yield as quinine was gradually introduced. In the chronic forms of malarious disease, where we have

time to produce the constitutional effects of arsenic by small doses, we could use it, for the results of arsenic are often in ague far more permanent than quinine.

In conclusion, also, I must, in self-defence, explain other details in my practice. My published treatment may induce my readers to suppose that I treat every case of fever with 3ss. and ℈j. doses of quinine, and push it to the utmost; but it must be recollected that my published cases, and the treatment I have recommended, is only for *extreme* cases, and where there is urgent danger of life. Also I must be pardoned, in performing an experiment on quinine in the General Hospital, for pushing my treatment to extremes. Half the value of the results of my experiment would have been lost, if I had not done so, for it is only the extremes of any practice which show its really hurtful tendencies. And I confess that I might have saved much of the quinine which I gave to my patients during my trial; but I might have lost some of them had I tried to adopt this economy. I never witnessed any harm from giving over-doses; and I thought it right, to settle finally the disputes about the danger of quinine in head symptoms and local congestions, to push it to extremes; and no one, I think, can be sorry that I did so—not even my patients; but I do not do so now. I do not, moreover, push quinine through the paroxysm, if I have time, and can safely avoid it; and prefer the sweating stage to any other for my largest dose, for the febrile disturbance of the accession is unfavourable for the absorption of all medicines—quinine, calomel, and purgatives included. All I say is, if there is danger, do so. Only a few cases also require heroic doses, but these being more particularly described, assume the most prominent position in every description of systems of treatment, and are too apt to be considered as resorted to in every case. I frequently give fever cases only ten-grain doses, oftener still only five grains three or four times a-day, and to a few none at all, but treat them with a purgative, low diet, and rest in bed. In justice, however, to your patients, you ought to give them a little quinine, for why let them be sick, when they may be well so much sooner? But when I see a patient with dull eye, cold clammy skin, tremulous tongue and oppresed pulse; or another, with acute head symptoms, delirium, and impending coma; or a child, with spasmodic contraction of the thumbs, squinting, or actual convulsions, in a paroxysm, I do not lose time by preparing such cases for quinine with purgatives, &c. I know the danger impending, and I give them (at least the adults) 3ss. of quinine at once, and follow it up with ℈j. doses every three or four hours, according to the symptoms it produces, till they are fairly cinchonised, and if they vomit the quinine I add ℈j. or ℈ij. of calomel. I never give more than 3ss. of quinine in one dose, and prefer frequently repeated ℈j. doses, because you can watch better the symptoms it produces, and stop it before the cinchonism becomes extreme.

Also, with respect to diet, I do not offer or prescribe food for a young soldier just attacked with fever; I would give little or nothing for the first day or two; but if I have a patient sent me who has had fever for a week or more, I feel sure that he is starving, although he may have no appetite—perhaps, even he may loathe food, but I persistently make him swallow small quantities of soup, arrowroot, and milk, and if I know him to be a spirit drinker, or even a free liver, I give him wine or spirit. This is all I wish to impress on my readers: that malarious fever is not an inflammatory disease, but the direct contrary, and requires no depletion by bleeding or purgatives, and that the congestion of the liver, &c., which appear in fever, and the morbid secretions thence arising, are best cured by early and large doses of quinine, under the use of which the congestions disperse, and with them the morbid secretions. Also that we can support the strength of the patient without danger by proper food and nourishment. Let a patient be therefore the first day or two of his fever with but little food, but afterwards give it to him in moderation, and especially do not starve children.—*Medical Times and Gazette, Nov.* 26, *and Dec.* 17, 1864, *pp.* 563, 647.

6.—REMARKS ON SCARLATINA.

By Dr. William J. Cummins, Cork.

[Dr Cummins has recently had many opportunities of testing the treatment of scarlatina by sulphites, so much advocated by Prof. Polli of Milan, and Dr. de Ricci (see *Retrospect*, Part L., p. 31). The salt used was the bisulphite of soda in ten-grain doses every one, two, or three hours according to the case. It will not produce any marked effects when the rash is already well out, for the catalytic action has then already taken place.]

What, it may be asked, is the use of administering such a medicine? I answer, because experience has taught us that the chief danger in the malignant variety of scarlatina (into which any of the others may lapse) is the tendency to secondary inoculation of the blood, a monster evil, which the sulphurous acid, set free when a bi-sulphite is decomposed by the acid of the stomach, tends to prevent. But, apart from the use of this medicine during the course of scarlatina, it is valuable as a prophylactic.

I have already mentioned that, before I was able to procure a supply of bi-sulphite of soda from Dublin, thirteen members of four families were attacked by scarlatina, and that three died (one more died a month after attack, from asthenia, caused by an abscess in the neck); it has also been stated that, when subsequently using the medicine, fifteen cases occurred in fourteen families, with no death directly due to the disease in any, except the two cases already detailed, in which the medicine was tried as an experiment *late in the disease*, when the patients were at the point of death. But I have yet to mention that the other members of these fourteen families, as well as of one other which had lost a child in twenty-four hours, without medical aid—in all fifteen families, numbering thirty-seven individuals—took, by my orders, ten grains of bi-sulphite of soda twice a-day.* This accounts for the comparative immunity which they enjoyed from the effects of contagion; for, with the exception of three who were attacked by the precursory symptoms of scarlatina—such as vomiting, sore throat, and transient eruption, which, on pressing the medicine every three hours, subsided entirely by the following day—and three others who went through a brief and modified attack of scarlatina, they all escaped the effects of contagion to which they were exposed in close unventilated houses. The results of the trial which I have given the bi-sulphite theory, tabulated, are as follows:—

Taking the medicine late in the disease by cases at the point of death (in one only for a few hours), 2; died 2.

Without the medicine.			Taking the medicine prescribed early in the attack.		
Families	Attacked	Died	Families	Attacked	Died
4	13	3	14	15	0
Besides one death indirectly caused by scarlatina a month after attack.			One death occurred indirectly from scarlatina a month after attack.		

Using the medicine as a prophylactic, by persons exposed to contagion in their own houses.

Individuals	Attacked		Cut short	Modified	Escaped entirely
37	6	Result, . . . No death.	3	3	31

The very extreme of the importance of results such as these tends to cast

* The use of bi-sulphite of soda as a prophylactic for zymotic diseases has not been limited to *scarlatina*, the author having prescribed it during the last few weeks for several families exposed to the contagion of typhus, and so far with success.

a doubt on their reality; and, while I can assert that I have not overstated them in any particular, I must add that I accept the conclusions to be drawn from them with reserve, and look anxiously for their confirmation in any trial which others may be induced to give the medicine on my recommendation.

Another medicine which has been used for its direct effect upon the blood in scarlatina is the chlorate of potash, and I can speak most favourably of its use. Given in doses of from five to fifteen or twenty grains, according to age, it produces a manifest effect upon the condition of the throat and the colour of the rash, which is perhaps due to the oxygen it contains; but the chlorine itself may act as a disinfectant, or when urea is detained and decomposed in the blood into carbonate of ammonia, by combining with the latter and forming a chloride of ammonium. This medicine has often failed, however, and occasionally seems to produce no effect whatever. I cannot speak from experience of the sesquicarbonate of ammonia, of chlorine water, or of other medicines which have been used for their direct effect upon the blood in scarlatina; but the muriated tincture of iron is a remedy of great value in the malignant form, especially when there is diphtheritic exudation, or a decided erysipelatous tendency.—*Dublin Quarterly Journal, Feb.* 1865, *p.* 23.

7.—THE BLISTER TREATMENT OF RHEUMATIC FEVER.

Cases under the care of Dr. JEAFFRESON, at St. Bartholomew's Hospital.

[See last volume of the *Retrospect*, Synopsis, page 273, for a brief epitome of this plan of treatment].

The safety of the heart is undoubtedly the main point at which every treatment must be directed; and in this particular more especially does the Blister Treatment exhibit its peculiar value. In a communication read Wednesday, March 22, by Dr. Davies at the Hunterian Society, he stated that of 50 cases which had been admitted under his care at the London Hospital 27 had hearts already damaged by recent or old inflammatory mischief, and 23 were free from cardiac complication. The results of the blister treatment in these fifty cases showed that as many as twenty-five, when discharged from the Hospital, were totally free from any endo- or peri-cardiac disease; or, in other words, that while every heart was saved which came in sound, two recent cases of endo-carditis were apparently cured by the alteration effected, as he believed, in the alkalinity of the blood by the free discharge of serum from the neighbourhood of the inflamed joints. Dr. Davies also states that those cases answer best to the treatment in which a great number of joints are simultaneously affected, and when, by setting up a large amount of discharging surface in the proximity of the inflamed parts, a large proportion of the *materias morbi* may be evacuated at one *coup*. Cases where the poison would appear to crop up to the surface by instalments, attacking the various joints at intervals of days, do not afford such striking examples of the efficacy of the treatment. The first case, where an unexampled amount of blister was applied in an extremely acute case, and where the patient was discharged, cured in thirteen days, will well illustrate Dr. Davies' position. That this treatment is not simply local in its action is also shown in the alteration produced in the urine in the majority of the cases cited; for in 11 the urine remained acid, but generally diminished in acidity during the whole period of the case; in 22 it became neutral shortly after the serum was discharged; in 10 it exhibited an absolute alkaline reaction; while in 7 no notes were taken.

Case 1.—William S., aged thirty-two, a working silversmith, and exposed to great variations of temperature, was admitted into the Hospital on December 2, the seventh day of his illness, and was discharged cured on December 15, thirteen days after he came under treatment. Eleven blisters, amounting to 482 square inches, were applied simultaneously, and with almost immediate relief. As the patient said, "the rheumatic pains left me as soon

as the blisters drew;" and on the third day from admission all pain had disappeared. The pulse fell from 105 to 95 per minute; the temperature from 101·4 to 99·6 and 98·8; no cardiac mischief was developed. The urine, scanty and acid on admission, was rendered slightly albuminous from the presence in it of a small quantity of blood. The slight strangury and albumen, however, disappeared in forty-eight hours. He had slept very badly from the commencement of his illness, but as soon as the poultices were applied to the blistered surfaces sleep returned, and was "good" every night during the time he remained in the Hospital. His appetite, bad on admission, was good on the third day; and his thirst, which was slight when he came under treatment, was not increased by the blisters, is reported to be absent on the fourth day. The heart was sound when he came under treatment, and free from disease when he left the Hospital.

Case 2.—Wm. P., aged twenty-two, bootmaker, was attacked, for the first time, with rheumatic fever on November 29. He was admitted into the Hospital on December 6, the seventh day of his illness, and was discharged on January 2, 1865. Seven blisters, equal to 133½ square inches, were applied on December 9, and the pain, which was severe previous to their application, had disappeared as soon as the surfaces had been dressed. The following day the clinical report states:—"There is no pain anywhere, and with the exception of a little uneasiness on the right shoulder on December 17, he was quite easy and comfortable until the day of his discharge." The pulse fell from 110 on admission to 90 per minute on the third day. The temperature from 101·4 to 98·6. The heart presented a systolic murmur, distinct at admission over base and apex, and was unchanged at the time of the patient's leaving the Hospital. His sleep, which was stated to be bad before the blister application, was improved on the second, good on the third night, and remained satisfactory until he was discharged. The urine presented a slight trace of albumen as the result of the treatment, but was normal and free from that substance forty-eight hours after the blisters had been applied. The appetite, which was indifferent on admission, is reported to have been "good" on the fourth day, while the thirst was absent at first, and continued so, unaffected by the local irritation set up by the blisters.

Case 3.—John B., aged nineteen, was admitted on December 8, 1864, the seventh day of his disease, and discharged January 30, 1865. He had, however, been up and dressed from January 11, 1865. Eight blisters were applied on December 9, three on the 12th, one on the 13th, one on the 14th, one on the 21st, making in all 603½ square inches of empl. lyttæ. The day succeeding the application of the eight blisters the report states that all pain had disappeared. On the fourth day from that date, the patient has "now a little pain in the right shoulder, and says he wants another blister for it," which is applied and followed by great relief. The clinical account says:—"December 14.—The patient has no pain anywhere. December 18.—The same. December 19.—Had pain in the left knee yesterday; I think he caught cold from going to the closet yesterday. And December 20.—The left ankle and knee were swollen. The application of the blister removed all pain and swelling, and never returned during the time he continued under treatment. The pulse fell from 108 to 86, and varied from time to time, with the appearance of the affection in different joints. The temperature fell from 100·8 to 99·4, and also varied with the pulse. The heart presented before the application a systolic murmur at the apex, which remained unaltered at the time of his leaving the ward. His sleep, which has been bad since December 2, was pretty good on the night after the blisters had been dressed; moderate on the fourth; good on the fifth; disturbed considerably by a blister on the 6th; good on the following and every succeeding night. The urine.—Its reaction before the blister application was not noted, but the treatment induced some strangury and traces of albumen, and its condition varied from time to time with the variable condition of the joints. Appetite is stated to have been bad on admission; a little better on the second day from the application of the blisters; improving and gradually becoming *good from* the fifth day of the treatment. The thirst was slight when the patient *entered* the Hospital. It appears, however, to have been considerably

increased after the application of the blisters, but the symptoms vanished in a few days.

Case 4.—Charles P., aged twenty, butcher, already previously the subject of acute rheumatism, was admitted on December 8, 1864, the fourteenth day of his illness, and discharged on January 18, 1865. Four blisters, equal to 118¾ square inches, were applied simultaneously, and with such relief that on the day succeeding the application all pain had disappeared, and his sleep, which for fourteen days had been wretched and without benefit to him, is reported as being good, and continuing satisfactory for the remainder of the time he was under observation. The pulse, which was 80 and 66 respectively on the two days before blistering, became 72, 68, 63, and ultimately 60. The temperature, 100·4° and 98·8°, before blistering, became 98·8°, 99·2°, 98°, and ultimately, on cure, 98·4°. The heart was free from disease on admission, and continued the same during the entire period. The urine presented after the blistering a very slight trace of albumen. The appetite, which had been bad since November 25, was very much improved on the day after the blisters had been applied. The report says, "he eats everything before him;" and it continued excellent from that date. The thirst was great on admission, only slight after the blisters were applied, and stated to be *nil* on the following day.

Case 5.—Susan J., aged twenty-two, needlewoman, of fair complexion and sanguine temperament, and who had had three previous attacks, was admitted with acute rheumatism on December 12, the fourth day of her illness, and discharged on February 2, 1865. Six blisters were applied; four on December 12, and two on December 14, equal to 328 square inches. On the day succeeding the first application the report states: "There is no pain now in the hands and ankles, but pain came on in the night in both shoulders, and she has had a very slight pain in passing urine." After the second application the report continues: "Feels much better; no pain anywhere; got out of bed this morning; wrist still swollen and red, but soft, moveable, and not tender to pressure." On the following day report states: "She cannot even invent a pain anywhere; looks well, and is only weak. The pulse fell in this case from 114 to 100, 90, and 70, and was 84 at the time she left the Hospital. The temperature fell from 101.7° to 99.7°, or two degrees. The heart was diseased on admission, and the systolic murmur at base and apex remained unchanged. The urine, at first acid, and containing a slight trace of albumen from the blisters, became alkaline on the second day from the time of the local application, and normal speedily after this date. Her sleep, which had been almost absent from December 8, is stated to have somewhat returned after the blisters were removed, to be much better on the fourth, and very good on the fifth night, and to have continued satisfactory until the time she was discharged. Her appetite, none on admission, was improved immediately after the application of the second set of blisters, and pronounced good on the fifth day after the last-named application. The thirst was very great on admission, unaltered the day after the blistering process, much less in amount on the following day, and none on the succeeding days.

[The following case occurred under Dr. Davies' own care at the London Hospital.]

A case at present under the care of Dr. Davies affords an excellent example of the safe and rapid results of the treatment. The patient was brought into the London Hospital on Friday night completely crippled from acute rheumatism, and presenting the usual symptoms of that disease. He described his agony as very intense. Ten blisters were at once applied, and a grain of morphia was administered internally.

On the Saturday evening he was free from all pain.

On the Sunday he declared himself to be comfortable, and his joints to be moveable.

On the Monday morning he sat up and washed himself, and in the evening was able to walk down the ward to the closet.

On Tuesday he was perfectly easy, with appetite returned, and thirst absent; and was ordered to have a chop and a pint of Bass's ale, as he had been accustomed *to live rather freely*. The urine in this case was found to

be strongly alkaline on reaction, and perfectly free from any trace of albumen as tested by heat and nitric acid. He had had no strangury. On being questioned as to the pain induced by the blisters, he said that he would gladly have them repeated were he unfortunate enough to have another attack of rheumatism. No cardiac mischief was developed. No medicine beyond the morphia already mentioned had been administered.—*Medical Times and Gazette, April* 1, 1865, *p.* 333.

8.—ON THE USE OF AN ALKALINE BATH IN RHEUMATIC FEVER.

By B. G. Everett, Esq., Ditchling, near Hurstpierpoint.

Having for the past seven years been, I might say, a martyr to rheumatic fever, very few months passing by (till within the last year) without my being confined to bed, perfectly helpless, for some weeks, I need not say I take a peculiar interest in the treatment of that disease; and having, I quite think, warded off two attacks within the past twelve months, I shall feel obliged if you will insert this in your journal, in the hope that some of your readers will be induced to adopt the treatment in similar cases, and kindly inform me, through your pages, the results.

During a severe attack of rheumatic fever about fifteen months ago, I found nothing relieved the pain or apparently did so much good amongst local applications as spongio-piline soaked in a warm saturated solution of nitrate and carbonate of potash, and applied to the joints; so I determined that when well again, directly I had any rheumatic pains, I would have a warm bath containing potash. As usual, only a few months passed; all the early symptoms of my old enemy returned. I was unable to use my extremities, and also had lumbago, showing there was both muscular and articular rheumatism. I at once decided to try the bath; it contained about twenty-four gallons of water at 100°, two pounds of bicarbonate of potash, and one pound of nitrate of potash. I remained in it ten minutes. The following morning I was much better; there was no fever whatever, and in a few days I was well, and able to attend to my practice.

Shortly after this period I had a patient with rheumatic fever, and it being in its early stage when I first saw him, before the joints were much swollen, I ordered him a similar bath to the one I had, and with a similar result. I also gave it to a little boy, who when I saw him was delirious, although the joints were not much swollen, and there was no muscular rheumatism or cardiac mischief. In less than a week the boy was down stairs.

About a fortnight ago I again had all the premonitory symptoms of acute rheumatism; but at once had the alkaline bath as before. In two or three days I was well (although the knees were stiff and painful for a week), and had not a symptom of fever.

I doubt not I have to thank the bath for saving me from two attacks of rheumatic fever; for until I used the alkaline bath, whenever I had any rheumatism about me, it was sure to run on to the fever, and keep me a prisoner for several weeks.—*Lancet, Jan.* 21, 1865, *p.* 82.

DISEASES OF THE NERVOUS SYSTEM.

9.—ON SCRIVENERS' PALSY, OR THE PARALYSIS OF WRITERS.

By Samuel Solly, Esq., F. R. S., Senior Surgeon to St. Thomas's Hospital.

[*This disease must be very rare, for it is not mentioned even in Copland's Medical Dictionary.* Cases, however, do occur, and are liable to be mistaken *for a symptom* of incipient softening of the brain.]

The disease, as the name implies, shows itself outwardly in a palsy of the writing powers. The muscles cease to obey the mandates of the will. It comes on very insidiously, the first indication often being only a painful feeling in the thumb or forefinger of the writing hand, accompanied with some stiffness; these unnatural sensations subsiding during the hours of rest and sleep, to return with the writer's work on the next day.

The loss of power is not sudden, as in a paralytic stroke; nor is it a complete paralysis of any group of muscles. The paralysed scrivener, though he cannot write, can amuse himself in his garden, can shoot, and cut his meat like a Christian at the dinner table; indeed, he can do almost any thing he likes, except earn his daily bread as a scribbler.

Scriveners' palsy is not the only instance of a set of muscles being cramped and paralysed by long-continued exertion. There is, as has been observed by Virchow, shoemakers' cramp, milking cramp, the musicians' cramp, compositors' and the sempstresses' cramp.

When scriveners' palsy first commences, the victim of it only feels its direful influence after a hard day's work. He regards it only as a sign of fatigue, and, as he starts fresh the next morning, attaches no importance to it as the first attack of a serious enemy; but in a short time he is obliged to rest earlier in the day, and hails his early dinner hour with joy, as giving him some respite from the fangs of his tormentor. He tries to overcome his difficulty by holding the pen firmer, but this really only increases the evil. Suddenly he finds his pen dash off at a tangent, and the word that he intended to write in the proper line is, to his horror, commenced in the left-hand corner of the page. Not unfrequently the act of writing is arrested, not by such sudden diversion, but by trembling, and a shaking palsy limited to the right hand.

In the cases that have come under my own notice, I have observed much disturbance also in the function of the nerves of sensation. There is generally a feeling of weariness and slight pins and needles, but usually not numbness. Virchow, who has seen more of these cases, says: "Ordinarily no disturbances of the sensibility show themselves, except an undefined feeling of straining or fatigue; sometimes a pressure in that part of the muscles; a painful drawing of the nerves in the direction of the trunk; a cold feeling in the whole arm. These morbid sensations only appear as a rule, after strained efforts to overcome the spasmodic hindrance (or impediment); probably from their very passing appearance lasting so short a time, are not observed. The whole of the sufferings come after long-sustained and strained employment of the fingers and hands; and the cramp only appears later. In the case of Tallerond, after a wound from an exploding shell, (?) a fragment of which remained for a long time in the wounded middle finger, a constant pain followed, which was accompanied later by a winter's croup, which resisted all attempts at cure. Fanciful peculiar tremblings, as with Romberg's nailsmith, without any other consequence, give rise to severe pains, which only accompany certain movements, and are followed by tonic cramp of the muscles. A smith complained to me from the same cause, besides sensitiveness in the arm, of a frequent firm contraction of the fingers of the right hand, accompanied by the feeling of its having gone to sleep. These appearances were perceptible in rest, as well as in different movements. I myself once got severe pain in the muscles of the hand after long-continued work at anatomy; and I was then for a considerable time subject to it in holding the forceps, and to powerful tonic contractions of the muscles used for that purpose."

My own experience in the treatment of these cases is very decided: rest—entire rest from the occupation that has produced the disease. If the disease is recognised early, then two months' entire rest will arrest its progress; at the end of that time strychnine is of service, but it is injurious when given in the early stage of the disease, and before rest. During the time of rest the health should be sustained and improved in every possible way. Fresh bracing air—mountain air if attainable—is of more service than all the tonics in the world. As regards tonics, they must be varied to suit the peculiarities of each constitution. Zinc, iron, and quinine are the tonics on which I rely most.

The following is one of the most striking cases of paralysis I know of, induced suddenly by little more than an hour's continued strain upon a nervous centre, confirming all that I have said regarding the controlling and co-ordinating power of the spinal cord:—A gentleman got into the five o'clock express train for Brighton. He was no sooner seated than he felt a strong desire to have the bowels relieved; but there was no help for it—the train started, nor did it stop till it reached its destination at a quarter past six o'clock. All this time the desire to go to stool increased more and more, but he restrained it successfully by a violent effort. The moment the train stopped he sprang out of the carriage, when the sphincter ani relaxed, and the contents of the bowels were poured out into his clothes. The curious part of the case is, that he became paraplegic. The spinal cord had been over-exerted, exhausted and paralysed by the long-continued effort. In this case the brain, from which I suppose all must admit that the will emanated by which he desired to control his sphincter, did not give way. His intellect was not impaired, and the whole muscular system was not paralysed, only that portion which is supplied by the lower portion of the cord. I regard this as identical in its pathology to scriveners' palsy, only that the destruction of the nervous centre was sudden, not gradual. I do not know what treatment was adopted in this case, but, reasoning from analogy, I should have advised complete rest; that some arrangement should have been made to prevent his ever having to control the action of the bowels if he had retained in any degree that power. —*Lancet, Dec.* 24, 1864, *p.* 709, *and Feb.* 4, 1865, *p.* 114.

10.—ON MUSCULAR ATROPHY, OR WASTING PALSY.

By Dr. C. B. Radcliffe, Physician to the Westminster Hospital, and to the National Hospital for Paralysis and Epilepsy, &c.

The disease upon which I am about to make a few comments has been distinctly recognised only within the last ten years. It is that disease which is now generally regarded as primary idiopathic granular or fatty degeneration of the muscular fibre, analogous in all essential features to fatty degeneration of the heart; and to which the names of progressive muscular atrophy, wasting palsy, or Cruveilhier's disease, are most generally given—the latter name in honour of the distinguished physician who was the first to call attention prominently to the subject. The case which will serve me as a text terminated in perfect recovery, and this termination is not the smallest feature of interest belonging to it; for the common impression is that the disease of which it is an example is one which sooner or later ends fatally. This is the impression, for example, which is gathered from the perusal of M. Trousseau's excellent clinical lecture on the subject, seeing that not one single word is said in this lecture on remedies or cure. Moreover, the very epithet *progressive* implies, more or less distinctly, a fatal tendency. In fact, however, there are other cases on record which show that a less gloomy impression is justifiable; and that this is so, abundant proof may be found in the excellent work of M. Duchenne (de Boulogne) on "Electrisation Localisée," and in the model essay of Dr. Roberts on "Wasting Palsy."

Case.—Caroline B., aged nineteen, of Ticehurst, Sussex, a domestic servant, admitted into the National Hospital for Paralysis and Epilepsy on the 21th of March, 1864.

Present state.—The patient complains of weakness and wasting of the left hand, and says there is nothing else the matter with her. She also says that this wasting and weakness began five months ago only.

In the left hand, the muscles forming the ball of the thumb (the thenar group) are not more than a third of the size of the corresponding muscles in the other hand, and the hollowness of the metacarpal interspaces is such as to show that the interossei and lumbricales muscles have disappeared in great measure. The special muscles of the little finger (the hypothenar group) are *also* considerably wasted. The fingers are curved in the position which *belongs to incipient* "bird's-claw deformity" or "main en griffe"—the de-

formity which M. Duchenne (de Boulogne) has shown to be the direct result of atrophy of the interossei and lumbricales muscles. The wasted muscles present no signs of fibrillary or other involuntary movement. Those belonging to the ball of the thumb contract freely under the action of the magneto-electric current; but no sign of contractions can be produced in this way in the spaces where the interossei and lumbricales muscles ought to be. No numbness is complained of at the present time, and none can be detected by the compasses. Comparing the left hand with the right, the surface is found to be cooler, bluer, and moister in the former than in the latter. The pulse at the wrist is also more feeble on the left side than on the right. The muscles of the left arm and of the rest of the body present nothing wrong in form or function; indeed, with the exception of the condition of the left hand which has been described, the patient (who is a short, healthy-looking, well-developed florid girl, with dark hair, light-grey irides, and large but regular pupils) would seem to be in perfect health, both bodily and mental.

Previous history.—Nearly five years ago the patient noticed that her left hand was weak, wasted, and occasionally numb. About the same time the little finger became bent, and got "into the way;" and from that time to this the condition of the hand has gone on gradually altering into that which exists at present. Latterly, she says, the hand may have "gone wrong a little more quickly." She also says that from the first the fingers would frequently "die," particularly when cold, and recover with "pins-and-needles." Of late, also, there appear to have been occasional attacks of palpitation.

Previous to the commencement of the wasting of the left hand, the patient appears to have enjoyed uniformly good health; and she distinctly says that she never sprained or hurt this hand in any way. Her family history is also good. She is one of ten brothers and sisters, all living and well, as she believes, except three, who died of fever; and both her parents are living.

Treatment.—Faradization, kneading, and suitable localized movements to be used every day. Half a fluid drachm of syrup of iodide of iron to be taken thrice a day, with full diet and porter.

Progress of the case.—April 9th. All the treatment carried out. Faradization has been used eleven times. Slight action in the lumbricales and interossei muscles.

20th. Is now able to straighten the first and second fingers, and to do something towards straightening the third and fourth. The hand is warmer to the touch. The Faradization has been repeated eighteen times.

May 13th. The hand has almost entirely recovered its full form and functions. The Faradization has been repeated twenty-six times.

June 23rd. The wasted muscles have now fully recovered their bulk and uses. The form of the left hand is now perfectly natural, and its bulk quite equal to that of its fellow. The patient, in fact, has perfectly recovered, and is to-day discharged *cured*.

This case is not an example of Cruveilhier's atrophy in its most marked degree. It is not one in which the arms, reduced to little more than skin and bone, hang uselessly by the sides; or in which the chest is so far stripped of muscle that scarcely enough power is left to carry on the breathing; or in which the tongue is wasted to such a degree as to be of little or no use in speech or mastication; or in which the pharyngeal muscles are attacked, and death from starvation can only be averted by using the stomach-pump. But it is one which may serve my present purpose not indifferently.

The atrophy of the muscles is the first point to which the attention had best be directed. The muscles of the left hand, especially those in the metacarpal interspaces, are considerably wasted; while the muscles of the left arm and of the rest of the body are perfectly plump and healthy. Now this wasting is peculiar in more respects than one. Some muscles are, as it were, dissected out, leaving hollows which seem all the more hollow from the fact that the neighbouring muscles may be almost or altogether unchanged. This wasting is partial in a sense which forms marked contrast to that general wasting which occurs after paralysis. There is also something very peculiar in the situation of the atrophy. At first the impression may be that the wasting of the muscles is the result of *some* paralysing lesion in some nerve or nerve-centre;

but it soon becomes evident that it is difficult to account for the selection of the muscles upon this view. The muscles, in fact, are not selected in those groups in which they are affected in hemiplegia, in paraplegia, or in any other manner which points clearly to some paralysing lesion in a nerve or nerve-centre. There seems to be something capricious in the selection of the muscles which are the seat of disease; unless it may be that the obscure disposition to a symmetrical affection of corresponding muscles on the two sides of the body may point to some disease in the blood, rather than to any disease in the cerebro-spinal nervous centres, or in the nerves belonging to these centres. Moreover, the wasting in the disease in question is an early phenomenon—perhaps the one first mentioned, and not a late one, as it is in the wasting which is consequent upon paralytic affections.

Weakness is the symptom which has next to be noticed. The muscles of the hand are weak as well as wasted. Now, this weakness is quite distinct from paralysis. The muscles waste, and as they waste they become weaker and weaker; but so long as any muscular structure remains, the will is capable of acting upon it, and the degree of muscular force called into action in this way is proportionate to the amount of unaltered muscular structure remaining. *There is no true paralysis.* The relation of the weakness to the wasting is also distinct, the weakness being posterior to the wasting, not anterior. The case cannot by possibility be confounded with sudden paralysis from cerebral or spinal disorder; nor need it be confounded with that paralysis arising from softening of the brain or spinal cord, which may be ushered in slowly, and in which the muscles may waste *pari passu* with their loss of power; for in this latter case the hemiplegic, paraplegic, or otherwise "regular" character of the paralysis, and the presence of other unequivocal "head symptoms" or "spinal symptoms," will remove all doubt as to the true nature of the malady, if only moderate care be used in the examination.

Numbness of the hand is one of the symptoms mentioned in our case; but this numbness is not a constant symptom. It could not be detected at the time of the first examination, and it was not detected afterwards. It came on when the hand got cold, as the hand very readily did. It seemed to be rather the result of want of blood from failure of the circulation in the part (the circulation in the part was at first very feeble) than anything else; so that this symptom cannot be looked upon as a sign of any disease in the ordinary nerves of the hand, or in the nerve-centres belonging to these nerves. Nor does dumbness or pain or any other sign of sensory disturbance occur as a constant feature in other cases to invalidate this conclusion. On the contrary, the remarkable freedom from such disturbance in cases of Cruveilhier's atrophy is one of the most striking features of the disease.—*Lancet, Jan.* 14, 1865, *p.* 30.

11.—EPILEPSY AND THE ADMINISTRATION OF BROMIDE OF POTASSIUM.

By Dr. G. Goddard Rogers, Physician to the West London Hospital, and late Medical Registrar to St. George's Hospital.

[The good results following the administration of the bromides in cases of epilepsy are undoubted. It cannot but result in disappointment if it is expected to be equally beneficial in every case, and our next endeavour must be to ascertain to what class of cases it is most suitable. The following cases are interesting.]

Case 1.—R. B., aged thirty-four, married, with two children, the youngest eleven years old, first came under treatment at the West London Hospital in May, 1861. About six months after marriage she miscarried, and had for three weeks frequent attacks of syncope, followed a few weeks later by a convulsive seizure, during which she lost her consciousness. She does not remember being told afterwards whether she foamed at the mouth or bit her tongue. These attacks were few in number for some years; but she often had "fainting fits" and "sighing fits," during which she "felt lost"—*(petit mal?)*

In the early part of 1861 the fits recurred more frequently; so that she was obliged to give up her employment as occasional nurse. Feeling low-spirited, and despairing of aiding her family, she came to the hospital. She was rather below the middle height, of florid complexion, with a wild staring expression, and very excitable manner. The heart's action was somewhat feeble, the breathing tranquil, and the bowels apt to be somewhat relaxed. Pressure of the crowd in the waiting-room, or the anxious struggle to be amongst the first served with medicine after seeing the physician, would sometimes bring on a seizure. One I particularly remember was most violent, and lasted a long time. Although the true fits, or, as she always termed them, the "struggling fits," were principally confined to the pre-menstrual or post-menstrual epochs, yet they occasionally followed coition, as I learned from the husband, who, by the way, has been a noted pedestrian in sporting circles during the past twelve years.

I first gave turpentine and castor-oil draughts; but no worms were dislodged. Bichloride of mercury with bark was next tried; and remembering that valerian had been mentioned by some authorities as putting in a claim for approval, I tried it in many forms—infusion, ammoniated tincture, valerianate of iron, of zinc, and of quinine. Dr. Duncan Gibb, who was about that time my colleague, had been giving the bromide of ammonium to allay irritability and decrease sensibility of the larynx and pharynx prior to using the laryngoscope. Thinking the same drug might act beneficially on the mucous lining of the uterine organs, I administered it freely to my patient; but with no good effect. The same was the case with iodide of potassium; and I then accidentally refreshed my memory by reading Sir C. Locock's paper on the bromide.

From the 21st November, 1862, to the 3rd December, R. B. took bromide of potassium in four-grain doses three times a day, with compound tincture of valerian. Between these dates she had two fits of a mild character. The medicine was intermitted until the 9th, when she came in great distress, saying that she was suffering from severe flooding, and had had several violent fits, on one occasion nearly falling into the fire. She was ordered to resume the medicine.

Jan. 2nd, 1863. To leave off the tincture of valerian, and take ten grains of the bromide in water three times a day.

8th. Two severe attacks.

16th. No seizure since last entry.

She now took quinine and ammonia up to March 16th. The seizures were very frequent during a portion of this time, and she suffered severely from menorrhagia.

From the 13th March to the 12th May she took ten grains of the bromide three times a day, and much improved. In fact, she relied solely on the medicine, and declared that the fits would return directly she left it off. To test this statement, during the latter half of July I merely prescribed a placebo of aromatic water; but so great was her distress that she implored me to allow her to resume the original medicine, and during August and September she took fifteen grains of the bromide three times a day. I afterwards increased the dose to a scruple. Beyond this dose I never found occasion to go with the patient, and, from my experience in other cases with larger doses, I am compelled to differ from Dr. M'Donnell, when he says that ten grains three times a day is "too small a dose to develope any good result."

In January, 1864, I discharged my patient, she not having experienced a severe fit, or one of the *petit mal* description, for more than three months. She resumed her former work of nursing, and I saw nothing more of her until the 10th of June last, when she came to me complaining of vertigo and "fulness about the throat," the "choking aura," which was the sure precursor of a seizure at a menstrual period. Two days later the seizure occurred, but it was of a very mild type. I kept her on scruple doses of the bromide up to the 15th July, seeing her during this time on six occasions. She remained quite free from attacks up to the time of her dismissal in July, and on the 3rd November I heard that her good health had been uninterrupted. I do

not mean to say that she is cured; but in 1861 and 1862 her life was a burden in consequence of frequent attacks, and from these she has enjoyed an immunity for three, four, or five months.

Systematic writers on materia medica have dwelt much on the anaphrodisiac influence of bromide of potassium. I do not think its beneficial influence is to be looked for in a simple lowering of the sexual power. When that power is unduly drawn upon, a degree of irritation is produced which the bromide may calm. But in the case above related, I had the fullest opportunities of learning from the husband that there never had been any diminution in his wife's *empressement*. And to arrive at greater certainty, the husband came under my care for some trivial ailment during his wife's attendance at the hospital, I took the opportunity of administering the bromide of potassium to him in fifteen and twenty grain doses three times a day. The information supplied after the lapse of a month satisfied me that here, at all events, the depressing property of the drug was *nil*. Where there is irritation and frequent priapism, the result of onanism or venereal excess, and where there is every reason to conjecture that such irritation is at the bottom of the epileptic seizures, then the bromide of potassium is, I think, of service. Dr. M'Donnell has promised us some further observations which it is probable will throw still more light on this important subject.

As confirmatory of the conjecture hazarded above respecting epilepsy in the male subject, I add a few notes of an hospital case which will bring this paper to an end.

Case 2.—E. B., aged sixteen, came to the West London Hospital in June last. His appearance is heavy, he complains of loss of memory, is partially deaf, and the pupils are much dilated. At twelve years of age, when working in a gun-factory, he contracted bad habits, which he has continued to indulge up to the present time. Sometimes he has practised self-abuse three and four times a day. The bowels are costive, and there is a feeling of weight at the vertex. Before he was thirteen years of age he had several fits, which his friends described to me as most severe, his struggles became extremely violent, and his tongue often bitten through. There was an intermission of eighteen months, followed by a fresh series of fits; and he then went to Salisbury Infirmary. The change of air and the medical treatment effected but little good; and when he returned to London he had "twitching of the face," and fits almost every day. In June and July he was under my care, and I gave him the bromide three times a day in doses gradually increased to twenty-five grains. The attacks gradually became less frequent, and he was entirely free from them all through August. On the 2nd of September his friends brought him again, as he had suffered from a slight fit during the night. I resumed the bromide; and, after taking it a fortnight, he felt better, and had no "twitchings" and no sign of a fit. Up to the present date (Nov. 21st) this lad has had no further seizures; and I think it reasonable to regard his improvement as due to the influence of the drug, coupled with obedience to my enjoining a strict abstinence from vicious habits.—*Lancet, Dec.* 10, 1864, *p.* 656.

12.—CASE OF GENERAL HYSTERICAL PARALYSIS TREATED BY THE CONTINUOUS GALVANIC CURRENT.

Under the care of Dr. SAVAGE, at the Samaritan Free Hospital.

An unmarried woman, aged nineteen, was brought to the hospital by her mother in September last, to ask the advice of Dr. Savage. She had always been in indifferent health, and during the last three years gradually lost the power over her arms and legs to such an extent that she is scarcely able to walk even when supported, and entirely incapacitated from doing any work whatever. She first menstruated at fifteen years of age, but has always been very irregular, and the discharge very pale and slight. Sixteen months ago *the catamenia* ceased altogether, and from that time she became nearly *idiotic*. At present she is very listless, has a vacant look, and considerable

dilatation of both pupils, more especially of the left one. The left iris is scarcely at all influenced even by a strong light. Her voice is almost entirely gone; she can only speak in a faint whisper. She is frequently troubled by pain in the head and the back. Her hands and feet are always quite cold. She complains of sickness in the morning, total want of appetite, and constipated bowels. She has had hysterical (epileptiform) fits. She has been under medical treatment for a long time, but without any benefit.

Seeing this condition of the patient, Dr. Savage consulted with Dr. Althaus, with the view of applying some form of galvanic electricity in order to rouse her nervous system. The latter gentleman made a most careful examination of the state of sensibility all over the body, and found that there was nearly complete anæsthesia of the whole left side, including the conjunctiva. On the right side the loss of sensation was not so much marked as on the left; but the prick of a pin was only felt as if it were a touch by some blunt instrument, and the examination by Weber's compass-æsthesiometer did not lead to any results, as the patient could nowhere distinguish whether she was touched by one point or two. The muscles appeared to be very badly nourished, but they contracted tolerably under the influence of the electro-magnetic current.

In cases like the foregoing, the application of the continuous galvanic current, such as produced by a large number of cells of Bunsen's or Daniell's battery, feebly charged, generally proves curative. It is a matter of doubt whether the current acts directly on the spinal cord, as is asserted by Prof. Remak, or whether it merely acts on the nervous centres by reflex action from the sentient nerves of the skin. The latter opinion is held by Dr. Althaus, who thinks that the resistance offered to the passage of the current by the bones and membranes enveloping the nervous centres is too great to be overcome by a current of such tension as it is safe to apply in the human subject. The principle of reflex action, moreover, is quite sufficient to explain the physiological and therapeutical action of the continuous current, so that is not necessary to go out of our way and assume principles of electric conduction for the human body different from those which obtain for other physical bodies. However this may be, thus much is certain—that the continuous current, if applied in a proper manner, has a distinct action on the nervous centres, such as is not possessed by the interrupted current. In cases, therefore, where the affection proceeds from a lesion of the nervous centres, the continuous current may be used; while for local ailments invading peripheral nerves, the interrupted current is more beneficial.

In the case just mentioned, a current of from forty to fifty cells of Daniell's battery was sent in an inverse direction to the spine, from the nape of the neck downwards to the sacrum, for ten minutes. The operation was repeated twice a week. The pain caused by this proceeding is very insignificant in the majority of cases, and to some patients the sensation is positively pleasant. After six weeks' treatment in the way described, the patient was so much improved that she could walk three miles at a time without support; she could dress and feed herself; was able to do heavy work about the house; had always warm hands and feet; the voice had returned; the bowels acted regularly; and she was cheerful, and took an interest in everything relating to her affairs. The pupils now were quite normal. An examination by means of the æsthesiometer showed that sensation had been quite re-established, on the left as well as on the right side. She was discharged on Nov. 19th, being then in perfect health, with the exception of the amenorrhœa, which still persisted, and an occasional feeling of sickness in the morning.

In this case the disorder was evidently of a merely functional character; but cases which are at present under Dr. Althaus's care seem to show that even in severe structural lesions of the cord and its appendages, the continuous galvanic current, if properly applied, produces highly satisfactory results. One of the cases is that of a girl, aged thirteen, who six years ago had meningitis spinalis lumbaris, contracted by sitting in winter on a cold door-step; and who having become completely paralysed in the lower extremities, *was admitted into Guy's* Hospital, under the care of Dr. Gull.

She remained in the hospital nine months, but without in the slightest degree recovering the use of her limbs. In October last she was sent to Dr. Althaus by Dr. Leared, under whose care she had been for some time previously. When she was first brought under the influence of the continuous current the paraplegia was complete, the patient being perfectly helpless, and the legs being wasted to the last degree. She is now so far improved that she can move her legs in all directions; and although she is not yet able to stand without support, Dr. Althaus holds out the expectation that by continuing the treatment she will entirely recover. The bulk of the legs has considerably increased although no local application of the current to the legs has been made.

The continuous current also proves beneficial in what has been recently described as "ataxie locomotrice" by Duchenne, but which is nothing else than the disease long known to the profession as atrophy of the posterior columns of the cord, and described by Romberg as "tabes dorsalis."—*Lancet, Feb.* 18, 1865, *p.* 175.

13.—ON THE TREATMENT OF CHOREA.

By Dr. James Turnbull, hysician to the Liverpool Infirmary.

[Chorea occurs in females most frequently just before the catamenial function is established, and when it comes on after this time there is generally some menstrual irregularity, accompanied with more or less anæmia. And the connexion between chorea and rheumatism is a most singular and interesting fact, suggesting the inquiry whether they are not both dependent upon the same *materies morbi* in the blood. Dr. Turnbull then passes on to the treatment of chorea, and observes:—]

The following case is interesting, both as an illustration of what may be termed hysterical chorea, and as affording an example of successful treatment by sulphate of aniline, whether such result may be considered to have been due to its direct sedative influence on the nervous system, or partly to the mental impression it produced on the patient—an influence in the treatment of such cases which should not be overlooked.

A lady, aged about 25, was placed under my care on November 16th, 1861, on account of a peculiar nervous affection, from which she had suffered more or less for eight years. In the early period of her illness, she had been under the care of the late Sir B. Brodie, and had been afterwards under the treatment of several irregular practitioners, but without receiving permanent benefit. The nervous affection consisted of an inability to control the muscles of the right arm, and in a slighter degree those of the left. There was an almost constant contraction of the muscles of the right shoulder and arm, causing a painful fidgeting motion, and compelling her to hold the arm with the left hand. There was plenty of muscular power; but she could not regulate the movements so as to write or do any kind of work. In walking, she had not perfect control of her movements, and exhibited some of the peculiar jerking seen in St. Vitus's dance. As regarded the general symptoms, the complexion was rather anæmic; the muscular development good; the bowels sluggish; the pulse quick; and menstruation regular, but scanty. The treatment was first directed to the improvement of the general health by the preparations of iron combined with ammonia, and by out-door exercise, the use of dumb-bells, &c. Warm hip-baths, with mustard, were given; sedative embrocations, with atropia, &c., were used to the arm and shoulder; aloetic aperients were administered. By these means, the general health was improved; but no influence was produced on the nervous disorder.

On the 10th of January, after she had been nearly two months under the previous treatment, I prescribed sulphate of aniline in three-grain doses thrice a day. It produced blueness of the lips and face more rapidly than *in any other* case I have seen; and in other respects she seemed peculiarly *sensitive* to the influence of that remedy, which always produced in the even-

ing, after the third dose, not only decided blueness, but also nervous depression, discomfort at the epigastrium, slight headache and palpitation, quickness of pulse, and coolness of the hands. At the same time, it had a marked beneficial influence on the disease; the muscular irritability was rapidly diminished, and she regained the control of the right hand and arm. The depressing effect of the remedy made it necessary to watch carefully its action, and to reduce the quantity to two doses daily. It was continued for three weeks, with occasional intermission for a day; and she had then completely regained the control of all her muscles, and could play on the piano. The blueness in this, as in all cases, went off within twenty-four hours after laying aside the remedy. About the middle of February, she had some return of the nervous symptoms, which soon yielded to the same treatment; and, at the end of the month, she returned home quite well.

Aniline has a powerful influence on the nervous system when taken internally, and still more when inhaled. From being accidentally inhaled, it has in several instances produced serious poisonous effects, though it has not been known to cause fatal effects on man. In recommending it as a remedy in chorea, I advised that the sulphate should be given in doses of two or three grains; and that it should be diminished or omitted when it produced the depressing effect, which is usually experienced in proportion to the temporary blueness of the surface it causes.

Dr. Frazer, who published some cases in which he gave it unsuccessfully, appears to have administered it in a different way—in large and increasing doses, as sulphate of zinc has sometimes been given—and as if his object had been to test its toxic as much as its therapeutic effects. In one of his cases, he tells us that the patient took twenty-eight grains daily. In thirty-four days, the quantity taken was four hundred and six grains; and it was continued all this time, although it caused frontal pain and aggravated deafness. "The result," he observes, "points nevertheless to the efficacy of sulphate of aniline in five cases; and is, therefore, *pro tanto*, a clinical fact to be noted." I believe that any agent which produces a violent impression on the nervous system may prove injurious in this disease; and I advise the employment of aniline only so as to produce a gentle sedative influence; and, as not one out of five cases thus treated in hospital improved or recovered under this mode of treatment from a disease which Dr. Wilks has shown that many patients get well of under regimen only, the result would seem to show further, that thus given, as an antidote against the disease, it may prove not merely inefficacious but injurious.

Having referred to the toxic properties of aniline, I would here observe that they have been erroneously regarded as of similar character with those of a more highly narcotic agent—nitro-benzine—which has very noxious properties, but has nevertheless been much used for flavouring and in perfuming, on account of its resemblance to the oil of bitter almonds. The effects of aniline and nitro-benzine have been confounded together from these circumstances. When aniline is introduced into the system, it undergoes oxidation, by which the mauve colouring matter is produced, as I was the first to point out. When nitro-benzine is introduced either through the stomach or by inhalation of the vapour, a portion is first converted into aniline, and this again undergoes further change, so that the same blueness is caused by both. The effects of nitro-benzine are, however, essentially different, and more persistently narcotic than those of aniline, as has been shown by Dr. Taylor in Guy's Hospital Reports, in a recent paper in which he has fully examined the various cases in which the toxic effects of these agents have been known to be produced; and he has recognised the difference in the effects of aniline and nitro-benzine. There is one fact, however, which appears to have escaped attention. It is this: that nitro-benzine is one of a series of chemical compounds which have similar effects on the nervous system. They are the xyloids—a series of substitution compounds formed by the action of nitric acid on starch (xyloidine), on sugar (saccharoine), on cotton (pyroxiline or gun-cotton), on glycerine (glonoine or nitro-glycerine), and on benzine (benzoine *or nitro-benzine*). Organic compounds contain-

ing nitrogen have often a powerful influence on the animal economy, as is the case with the vegetable alkaloids; and these are all nitrogenous compounds of similar chemical constitution.

Dr. Edwards, in a paper on the Physiological Properties of Xyloids, published in the "Liverpool Medico-Chirurgical Journal," first drew attention to the action of these agents, and showed that they have similar and peculiar effects on the nervous system: the amount of their action being, in a measure, proportioned to their solubility. "They all appear" (he says) "to possess, more or less, the peculiar action on the nervous system characteristic of strychnine; and I believe that further experiments on men would prove that they possess peculiar and valuable medicinal properties. Physically, they are allied; being all highly explosive bodies, deflagrating at a low temperature or by concussion; and physiologically, they all produce a powerful effect on the heart's action, trismus, and a series of tetanic convulsions, terminating in death."

Having witnessed most of Dr. Edwards' experiments I have taken this opportunity, of not only pointing out that nitro-benzine is different in its effects from aniline, but that it is one of the xyloids, and that to Dr. Edwards the merit is due of first ascertaining the poisonous properties of nitro-benzine, as well as its relation to the other xyloids.

Looking at chorea in a strictly practical point of view I would now make a few remarks on the general treatment of the disease. In cancer, which is one of the most incurable diseases, many remedies have been, and will no doubt continue to be, tried; and in chorea, which is one of the most curable, many remedies have likewise been tried. Mere number points, in reference to the first, to its intractable nature; and, in regard to the second, to the fact that, though generally a curable disease, none of the remedies in use can be relied on to control the disease with any certainty. Indeed, practically, we know that there are cases which for months, and even years, resist both judicious and expectant treatment; and, therefore, the study of therapeutics in reference to this disease is still worthy of attention.

The remedies which have been chiefly used are, purgatives; the mineral tonics—iron, zinc, and arsenic; cod-liver oil; electricity; and the shower-bath. Purgatives I have given only when the state of the bowels rendered their use necessary; and not, therefore, with the curative intention with which they have sometimes been administered. The mineral tonics undoubtedly exert a beneficial influence. I do not believe they have any direct curative power; but they improve the general health, which is more or less below par, and thus they appear to lessen the excitability of the nervous system. Iron I believe to be the most generally useful, and especially in anæmic females. Dr. Hughes, in his digest of one hundred cases, makes these remarks:

"The different mineral tonics are very variously estimated by different practitioners, and one is often regarded as especially suited to the cure of chorea. While one practitioner regards arsenic as a specific for true chorea, another cures nearly all his cases with the sulphate or the oxide of zinc, and another considers the sulphate or the carbonate of iron as an almost infallible remedy. This may perhaps arise, in a great measure, from habit. It is possible that all may be nearly equally efficacious. I have seen each of these, and many other remedies cure the complaint; and I have seen them all occasionally fail."

Zinc has been given in large doses, especially at Guy's Hospital; where Dr. Hughes has stated that thirty-six grains of the sulphate have been administered daily. It appears such an abuse of medicine, to prescribe these enormous doses of an irritant drug commonly used in such quantities as an emetic, that we are at a loss to discover how it should ever have been thus given. What Sir John Forbes has termed the autocracy of nature, could not have been understood by those prescribing it in such a manner; and Dr. Hughes observes:

"In the practice of the hospital, I am not aware that it has been shown ***that small or moderate doses*** of the mineral, continued an equally long time, ***will not effect a cure.***"

In my own practice, I have used iron and arsenic more frequently than zinc; and I consider that, of the metallic tonics, iron is the best, and that arsenic stands next to it.

Cod-liver oil is a remedy which is sometines of service, by improving the general health; and it may be given with, or subsequent to, the preparation of iron. Electricity seems, in some cases, to have acted beneficially, and in a very rapid manner.

The shower-bath is a remedial agent which gives tone to the system, and may be used along with other tonic treatment. I have seen it of the greatest use in many cases. In the case of a young lady, in whom the movements were incessant and violent, and prevented sleep, the movements subsided very much directly after the bath, and she soon fell asleep, and ultimately recovered. The emaciation and exhaustion of the patient from the incessant violent movements and the loss of sleep, had rendered the recovery of this patient very doubtful.

It is not less important to glance now at the means which may prove detrimental, and which should therefore be *avoided* in the treatment of this nervous affection. All violent impressions on the nervous system should be guarded against. We know that fright is not an unfrequent cause of the disease. It is probably on this account that the shower-bath may prove injurious when injudiciously employed, as well as very frequently useful. Narcotic remedies, such as opium, morphia, and hyoscyamus, have appeared to me (contrary to what might be expected) to be not simply useless, but positively hurtful. It has been said that chloroform is of use; but I saw the inhalation tried in two very severe cases, which proved fatal; and though it produced a temporary sedative influence, it appeared to me to act injuriously on the general progress of the disease.

In all cases, due attention should be paid to hygienic as well as medicinal treatment, so as to improve in every way practicable the nutritive functions and the general health. The diet should be nutritious, and wine or malt liquors are often of service; and out-door exercise is desirable in the less severe cases. When the disease proves chronic, change of air is advisable; and in the summer the change may be made with advantage to the sea-side, where sea-bathing may be practised.—*British Medical Journal, Jan.* 14, 1865, *p.* 33.

DISEASES OF THE ORGANS OF RESPIRATION.

14.—ON INFLAMMATION AS AN EXCITING CAUSE OF TUBERCULOSIS.

By Dr. James Jones, Physician to the Metropolitan Free Hospital, and to the Infirmary for Consumption and Diseases of the Chest.

[There is a very marked analogy between the various cacoplastic formations found in cachectic subjects, and the plasma exuded in ordinary sthenic inflammation, the chief difference consisting in the amount of their vital capacity or power of organization. In some, the resemblance amounts even to idenity. Who, for instance, could draw a line of demarcation between tubercular infiltration and the low plasma found in the pneumonia of debilitated subjects?]

If we commence at the bottom of the series by examining what is known as lardaceous exudation, we find a plasma, if, indeed, it merit that name, which is capable of advancing little or nothing in organization. Its vitality is so low, that as it exudes so it remains, with scarcely a trace of organization.

Advancing a little higher, we find the plasma known as tubercular infiltration. This, although but little superior to the lardaceous, asserts its claim to a higher place amongst the plasmata, by exhibiting a certain amount of capacity for organization in a scanty fibrillation and cellulation.

Next in the ascending scale comes the miliary, or grey semi-transparent tubercle, showing under the microscope a still greater development of fibre and cell.

Then comes the yellow, or common form of tubercle, to which, I suppose, we may give the next place in our ascending scale, inasmuch as it is found to consist of abortive cells, granules, &c., which indicates the presence of a degree of vital power sufficient to originate a cell-growth, but insufficient for its completion.

The next higher place may be assigned to cancer, inasmuch as it exhibits a greater degree of vital capacity for organization; for instead of an abortive, we find a well-matured cell-growth. And although incapable of any higher organism, its cells are endowed with the power of reproduction, and consequently of unlimited increase; hence its malignancy.

Next we have a plasma capable of an organization, nearly approaching normal growth, as found in fibrous tumours, &c.

The next point worthy of notice in our comparative examination of the cacoplasmata is their relation to the state of health under which they are formed. We find that their formation is invariably preceded and accompanied by a condition of general cachexia; but we also find that this condition of cachexia may exist for an indefinite time without being attended or followed by the production of any one of those substances. Therefore the condition of cachexia is not, *per se*, capable of giving origin to any one of them; it must be aided by some other factor.

Thus, a person who inherits a cachectic constitution from cancerous parents may live to a good old age, and finally die of natural decay; but a brother or sister may, from the irritation of a tobacco-pipe, or a blow on the breast, die of cancer of the lip or breast.

Again, in a large family, the offspring of consumptive parents, we often find the well-known characteristics of phthisical cachexia stamped on each one; yet some of them will survive to a good old age without a trace of tubercle, while the others die in early or middle life of consumption. Now, it is evident that in the cases of the latter, some new and additional influence came into operation, taking the position of a complimentary factor in the production of tubercle (cachexia + × = tubercle).

The occasional occurrence of cases in which tubercle is formed in the offspring of consumptive parents without any *apparent* exciting cause, is considered by many well-known pathologists to be sufficient evidence to prove the doctrine of *spontaneous origin*; but it appears to me that such inference is not legitimate, and that it is much more probable that the immediate exciting cause or local factor escaped notice than that it had no existence. There is no caprice in Nature; there must be cause for effect.

We see the tubercular cachexia continuing for years, or even a long life, without a particle of tubercle being formed, even under circumstances not the most favourable to the patient's escape. Now, it appears to me that one single instance of this kind is sufficient to upset the theory of the spontaneous origin of tubercle, for it shows that there is something more than a morbid constitutional diathesis necessary in order to bring tuberculosis into action.

Again, in all forms of cachexia, we find the general nutrition of the body to proceed *normally*, although languidly. The various parts and organs receive a supply of nourishment sufficient for the purposes of life, *but a period at length arrives in which, in some part or organ, an abnormal plasma or blastema is formed*, and this act of abnormal nutrition initiates the local disease. Now, I would ask, What is the cause of this sudden alteration in the nutrition of a part? Is it not the effect of some new power coming into operation, local in its action, and capable by its presence of perverting nutrition? What is this new power or factor?

I will now attempt to show by typical examples that in a large number, if not in all, of at least in one form of cacoplasm, namely, tubercle, inflammation is the active factor or additional power sought for, not the sthenic inflammation of vigorous subjects, but inflammation of a low type, a type modified by the constitutional diathesis, whose products are low, imperfectly vitalized, and incapable of assuming any high degree of organization. (In the term "inflammation" I comprehend all those abnormal conditions of the circulation and innervation of a part which give rise to effusion of some form of coagulable lymph or plasma, or [illegible] abnormal increase in functional activity

of organs under their influence. I include in it, therefore, the condition commonly called "irritation" as one of its phases.)

I shall also point out the importance of watching for the earliest manifestations of inflammatory action, and the early use of such remedies as science and empirical experience indicate for its removal, and thus, by anticipation, to oppose the onset of tuberculosis; also the necessity of contemporaneously pursuing a constitutional treatment suited to the peculiar nature of the cachexia.

Those who have an opportunity of seeing large numbers of cases of consumption must be struck with the fact that a majority of patients attribute the origin of their illness to "catching cold," or an "attack of bronchitis," or "inflammation of the chest," and that, if careful inquiry be made, their statements are found to be correct.

The following case is a type of a numerous class in example:—A patient, although showing symptoms of low health, is to all appearance free from actual disease until, through some indiscretion, a cold is caught. Cough comes on, with tightness of the chest, and chills, alternating with flushes of heat, are felt. After, a white expectoration appears, the cough continues, the urine is loaded with lithates, the tongue is coated, and the appetite fails; auscultation reveals nothing more than the ordinary signs of bronchitis, and *there is no dulness on percussion*. The patient hopes soon to get better of his cold, but he is disappointed; the cough continues, the expectoration is no less, and is not unfrequently tinged with blood. He loses flesh and strength, and is easily made to perspire. By-and-by, if the chest be examined, *dulness* under or behind one or both clavicles, or the supra-spinal ridge of the scapula, is *readily made out*. Tubercle has formed. This is not an ideal case, but one of a very large number, the history of one of which is the history of all. It is pulmonary phthisis, originating in catarrh. The patient was in pretty good health, or, at all events, free from tubercle until the continued disturbing influence of the catarrhal inflammation produced a condition of active tuberculosis.

I will next give a case of acute tuberculosis, consequent on severe bronchitis. It is taken from the work "On Diseases of the Chest," by Dr. William Stokes, p. 392:—A patient previously in good health is suddenly attacked with the most violent symptoms. There is high fever, extreme dyspnœa, lividity, and tenacious expectoration. *In the early periods the chest sounds clear*, but the signs of the most intense bronchitis, affecting the tubes of all diameters, are universally audible. The symptoms and signs continue with unabated violence, and *after some time the whole chest presents a certain degree of dulness*. The patient dies from the violence of the pulmonary inflammation, and on dissection every bronchial tube is found inflamed, and the lung equably and closely studded with miliary and granular tubercles."

I have selected this highly acute case from the pen of an eminent pathologist and careful observer, as showing most clearly, although casually, the fact that the dulness on percussion is not found until the inflammation has existed for some time, thus establishing the chronological sequence of the tuberculosis to the disturbing action of the inflammation. In the more common forms of miliary tuberculosis, the symptoms of antecedent bronchitis are less strongly marked, but to the careful inquirer they are unmistakably evident. The cooing, or musical râle, and the moist râles can always be found on full inspiration, but the *diminution in resonance on percussion* begins to manifest itself only *after bronchitis has existed for some time*. We have now the same sequence of signs, the chest resonant in the early part of the attack becoming dull to the stroke after the inflammation has continued for some time. Now, if any faith may be placed in physical diagnosis, we have in this sequence of signs sufficient to prove the fallacy of the doctrine, that irritation arising from the presence of tubercle produces the bronchitis.

Again, if we watch closely the progress of ordinary phthisis, we find each fresh attack of tuberculosis to be preceded by the usual signs of bronchitis. A. D——, æt. twenty-two, came under my care at the infirmary for Consumption. On examination, I found dulness on and under the left clavicle, and on the suprascapular ridge, with feeble respiration and prolonged expiration in the same parts. She one day got a severe wetting and took cold. On examination, *there was no alteration* in the extent of the dulness on

percussion, but auscultation revealed a new set of signs, moist and cooing notes in the apex of the opposite lung, and also in the lung first affected, but extending far below the seat of the percussion dulness. No further change of signs or symptoms occurred until three weeks had elapsed, when dulness began to be detected in the apex of the right lung, and was also found to have extended lower than before in the left lung. In fact, the inflammation had given rise to a fresh crop of tubercle below the original deposit in the left lung, and a primary crop in the apex of the right. Thus, we find the same chronological sequence of tuberculosis to inflammation in the progress as in the origin of tubercle.

This sequence of signs is so constant, that we may predict the presence of tubercle above by a lurking bronchitis below, which is absent in the corresponding portion of the opposite lung. (Stokes.)

I will now examine ordinary chronic bronchitis as a cause of tubercle:—C. B., when to all appearance in good health, caught cold at the beginning of the winter; cough and spitting, with more or less dyspnœa, continued to torment him all through the winter, but passed off when the warm weather set in. The next winter the same symptoms returned. and again passed off in the summer. My notes say, "Except for the cough and dyspnœa the patient appears in good health, the digestive organs are in good condition, and there is no loss of flesh or strength. *The chest sounds well on percussion.* On the return of the fourth summer, the patient expected the cough to subside as usual, but it continued to trouble him all through the summer. On percussing chest, *some dulness was now found* to exist in the *apex of one lung*, and after a lapse of some weeks *the dulness was found to have increased*, and to engage the *apex of the other lung also.* He now lost flesh and strength, but not rapidly. His case proceeded slowly to a fatal termination. Now, in this case, so long as it remained an ordinary chronic bronchitis, the patient maintained flesh and strength, but the advent of dulness on percussion was contemporaneous with the loss of both, and clearly fixes the time at which tuberculosis set in." I have notes of a large number of cases of this kind, which clearly exemplify the same chronological sequence of dulness on percussion to the disturbing influence of chronic bronchitic inflammation.

These cases require careful and frequent examination in order to guard against error of diagnosis. "Dilated bronchi will sometimes so closely mimic advanced phthisis, that a post-mortem inspection can alone remove our uncertainty; but the progessing dulness on percussion, the regular progress of the symptoms, and the absence of the bronchial breathing and resonance of the bronchial voice, which exist in dilated bronchial tubes," are generally sufficient to enable us to differentiate between them.

I will now give a case or two of tuberculosis from parenchymatous inflammation of the lung. Jane S., aged twenty, four years ago, when apparently in very good health, was seized with inflammation of the left lung, for which she was treated by an eminent physician to whom her father was coachman. She kept her bed for six weeks, and has never since recovered her strength nor felt as she did before. About one year ago she had a sharp attack of hæmoptysis. On examination, the right lung was found to be resonant throughout, and the respiration highly puerile. The left lung was dull throughout, the dulness being most intense in the apex and diminishing downwards to the base, but there was no ægophony. In the infra-clavicular region there was well-pronounced gurgling, posteriorly and inferiorly coarse moist râles, cough very troublesome, some nummular sputa, loses flesh, sweats at night. The father of this patient is suffering from some chest affection, supposed to be phthisis. This case is a type of a large class, in which a constitutional tubercular diathesis is roused into a condition of active tuberculosis by local parenchymatous inflammation.

In the following case inflammation and debility, induced by the mode in which it was treated, appear to be the sole factors of tuberculosis:—George E., admitted under my care at the Metropolitan Free Hospital. Tall, thin, sallow complexion, face marked with small-pox. Chest flat, left side somewhat collapsed, nails curved; mother living, and healthy; father killed by accident. Was in perfect health up to three years ago, when, being then a

soldier stationed at Shorncliffe, he was "told off" with a party of men to bathe in the sea, at the end of October. Chills came on the same evening, followed by fever and pain in the left side, dyspnœa and expectoration of red-stained glutinous sputa. He was bled largely from a vein in the arm, and cupped four times. This was followed by extreme weakness. He remained in the Military Hospital thirty-two days, and was then discharged as unfit for service. He has never since regained health and strength. About two years ago hæmoptysis came on, and it has recurred from time to time ever since. The day prior to his admission it was very copious. He expectorates half a pint of thick yellow phlegm every day, and loses flesh rapidly. The left pleura now contains no fluid, but there are well-pronounced signs of a large cavity in the superior part of the left lung. There are also signs of condensation in the apex of the right. Had this poor fellow been treated less *heroically*, he would probably have recovered from the effects of the inflammation; but the vital powers being reduced by the treatment, the plasma exuded was probably of a very low type, which, if not tubercular *ab initio*, rapidly degenerated into tubercle.

I will now relate a case in which tuberculosis was excited by an attack of pneumonia, in a subject debilitated by the action of other disease. Charles D., a sailor, was attacked by fever on the West Coast of Africa. During convalescence, between two and three weeks before his arrival in the port of London, he caught cold from exposure to the weather. Chills and flushes, cough, and dyspnœa came on. On his admission to the Metropolitan Free Hospital, the right lung was found to be somewhat dull on percussion in its lower and posterior part, and auscultation revealed a fine crepitation. The patient had lost flesh, and was very weak. He continued in this state, without any improvement, for some weeks. There was occasional hæmoptysis. The apex of the same lung then became dull on percussion, with coarse moist sounds. The expectoration was a copious muco-pus. He became extremely attenuated, had night sweats, and frequently a sharp diarrhœa. He was removed into the country by his friends, and I lost all trace of him, although he promised to write.

One more case, and I have done. J. B., aged thirty-five years, of irregular habits, obtaining a living by "jobbing" about the docks and river, had been out of work and badly fed, when, on going to a "job," he fell into the water and then allowed his clothes to dry on him. The next night he had shivering and flushes of heat. On admission to the Metropolitan Free Hospital, crepitation, rather fine, was audible in the posterior inferior part of the right lung. His symptoms were not severe, but remained unchanged for a *month*, and then slight dulness began to appear. The crepitation became coarser, and in three months from the commencement of his illness the signs of a cavity were obvious. The expectoration became copious and nummular. He lost flesh rapidly, and died with all the symptoms of plumonary consumption.

We can differentiate between these cases and mere pneumonia, by the long continuance of the crepitation, by the absence of bronchial or tubular breathing, by the slight degree of dulness, and the lateness of the period at which it manifests itself. Also between them and capillary bronchitis by one lung only being engaged (a rare thing in bronchitis), and by the slow progress of the symptoms as well as by the general history of the case.

I will merely allude to the inhalation of stone-grit, &c., as a cause of tuberculosis (a subject already ably handled by Dr. Peacock), although its bearing on the question under discussion is most important, inasmuch as by it we can trace tubercle back to inflammatory irritation caused by the presence of a mechanical irritant in previous healthy subjects. In all those cases *dulness is a late symptom*, appearing only after the health has been impaired by the continued bronchitis. I have seen cases of this form of tuberculosis in granary men, who inhale the silicious dust from the tunic of the grain while shovelling it. The grinders' rot of Sheffield and Birmingham is a form of tuberculosis produced in a similar manner by inhaling particles of steel cast off in the process of dry-grinding cutlery, &c.

The above cases *are sufficient* to exemplify the important position which

inflammation holds as an exciting cause of tuberculosis; but we must, at the same time, avoid the grave error of ignoring a predisposing cause.

Inflammation does not, I believe, in any case give rise to the formation of tubercle in a healthy subject. There must be also a condition of low health or cachexia present. This cachexia may be either inherited or acquired. In some of the cases I have related it was of the latter character. In the soldier it was probably caused by the depletion, in the sailor it arose from the fever, in the dockyard man from irregular living and insufficient food. I would argue, therefore, that neither the cachexia nor the local inflammation is capable *per se* of producing tubercle, but that tuberculosis is the result of their combined action.

It may be objected by some that the inflammation in these cases was the consequence of irritation produced by tubercle already present, but the difficulties attending this theory are much greater than the other; for, in the first place, the physical signs of tubercle are at first wanting, and show themselves only after the inflammation has continued for some time, and, in the next place, there is the difficulty of comprehending how a few soft or semi-fluid tubercles in a yielding and easily-compressed structure like the lungs could possess such irritative qualities, or why they should, after remaining dormant for an indefinite time, suddenly assume the character of irritants capable of exciting inflammation. Viewing tubercle as a cacoplasm, it must be subject to the same general laws as all other plastic exudations. Consequently, it must be the result of a disturbing action. As well might we attribute pneumonia to the presence of the plasma which we all admit to be a product of preceding inflammation. If the presence of tubercle in the lungs were capable of causing bronchitis or pneumonia, why are those consequences not always found? Why is the caprice? Why is the effect not constant?

I shall now give a brief recapitulation of the points exemplified in the foregoing cases.

1st. That a condition of low health or cachexia, although a necessary factor in the production of tubercle, may exist for an indefinite time without giving rise to any local lesion.

2nd. That, for the production of tubercle, some additional factor, in the form of a local exciting cause, is necessary.

3rd. That inflammation, in the form which it assumes in cachectic subjects, is capable of acting this part, and is probably the only exciting cause of tuberculosis.

4th. That a close analogy exists between the cacoplastic or lowly-vitalised formation found in cachectic subjects and the more highly-vitalized products of sthenic inflammation in healthy persons, and that this analogy is especially close in the substance called tubercle.

5th. That the condition of constitution known as tubercular cachexia, from whatever cause it may arise, whether an inherited vice of constitution, or the result of depressing agents, is capable of modifying the products of inflammation so as to give them the characters of tubercle.

6th. That the varieties of tubercle depend on the acuteness of chronicity of the inflammation, the degree of the cachexia, and the nature of the structure on or in which they are formed.

7th. That the presence of tubercle in its first stage is insufficient for the production of inflammation.

8th. That the doctrine of spontaneous generation of tubercle is fraught with danger to the patient by diverting the attention from watching for and guarding against the immediate exciting cause, and is also calculated to make our treatment one-sided by being directed solely to the constitutional malady.

9th. That, in the treatment, our remedies must be directed both against the constitutional vice and the local malady, and that both indications may be fulfilled contemporaneously.

When the subject of tubercular diathesis is seized with catarrh, bronchitis, or other pulmonary inflammation, or with pain in the clavicular or humeral regions, with fever and sonorous or moist râles in the corresponding part of the lung, his state is highly critical and entails a fearful amount of responsibility on the physician who undertakes the treatment; for if the local inflammation

or irritation be allowed to proceed, tubercle will form; but if, in his zeal for its removal, he adopt measures productive of permanent debility, such as leeching, cupping, purging, and very low diet, he aids powerfully the constitutional vice, and lessens, if he do not destroy, the patient's chance of recovery. If, on the other hand, he looks only to the constitutional malady, leaving the local mischief to the *vis medicatrix naturæ* and doses his patient with beef-steak and brandy, &c., a crop of tubercle is the sure result. The most successful practitioner is he who, keeping in mind the ever-present cachexia, deals with the local inflammation as the immediate cause of tubercle. The local inflammation being of a low type and slow in progress, it is easy to combine the treatment necessary for both indications. I have found excellent results from a combination of ammonia salines with tartarised antimony and morphia. When the inflammatory action is severe, this treatment should be continued for some days, aided by blisters of acetum cantharidis or other counter-stimulants over the affected parts, while the strength is supported by beef-tea and other light nutriments. The tartarised antimony appears to control inflammation without producing any permanent debility. As soon as the inflammation is somewhat subdued, a combination of perchloride of iron and nitrate of potass, given contemporaneously with the antimonial saline, is eminently useful. When the inflammation is less severe, the two modes may be combined from the very commencement. The dose of tartarised antimony should be much smaller than is usually given in sthenic inflammation. I find from ten to fifteen minims of the antimonial wine of the London Pharmacopœia every four hours to be sufficient. I know of no drug which possesses so powerful a sedative action in pulmonary inflammation as tartarised antimony. In small doses it calms irritation, while at the same time it promotes secretion. Coupled with morphia or cannabis Indica, and given with ammonia salines, it rarely fails as a remedy for low pulmonary inflammation.

The topical application of hot steam to the bronchial mucous membranes by inhalation is a valuable adjunct to the other treatment. When the inflammation is dry, not attended with secretion of mucus, the vapour of hot water alone is very useful. It should be inhaled at frequent intervals, and at as high a temperature as the patient can comfortably bear. Its effects are so soothing that few patients fail to repeat its use. When the sputum is viscid and hard to expectorate, vinegar may be added to the hot water. The French white wine vinegar, owing to the presence of a peculiar aroma, is usually most grateful to the patient. It can be got at Crosse and Blackwell's, Soho Square. When the cough is troublesome, the addition of acetum opii (Pharm. Dubl.) is useful, as are also chloroform or spirit of chloroform, Hoffman's anodyne ether, and Battley's sedative solution of opium when inhaled conjointly with hot water vapour. When a more perfect inhaler is not at hand, the ordinary small circular stoneware teapot is a very good substitute. The vapour is inhaled through the spout, removing it for expiration and depressing the handle to keep the liquid contents from entering the spout.—*Medical Mirror, April*, 1865, *p.* 202.

15.—ON FEVER AND BRONCHITIS AS EARLY SIGNS OF PHTHISIS.

By Dr. James Russell, Physician to the Birmingham General Hospital.

[The most common form of pulmonary inflammation which attends the deposit of tubercle is bronchitis. The bronchitis of early phthisis possesses two especial characteristics. It is so mild that under other circumstances it would attract little of our attention, and the kind of breathing which belongs to it is often spoken of as being merely rough or creaking and is accompanied with some degree of crepitation. Again, local bronchitis is generally a suspicious sign, for it indicates local irritation.]

In certain cases, tubercles are deposited very rapidly, and at the same time very generally, *being disseminated* in small isolated granules throughout

the entire of each lung. As a consequence of the rapidity of their development, bronchitis is set up; and because the deposit of tubercles is general, the bronchitis is general also. Moreover, as there is no aggregation of tubercles in any particular spot, none of the evidences of consolidation are presented. Here, then, the adjuncts to the bronchitis are wanting; and the case presents only the characters of one of general bronchitis. It is only as the disease goes on without showing any disposition to abate, and as other symptoms of a constitutional and local character are superadded, that you are enabled to assure yourself with certainty of the true nature of the case. I may, however, observe, that the difficulty in diagnosis affects chiefly adults, since persistent general bronchitis occurring in children is of itself strong evidence in favour of the presence of tubercles in the lungs.

The following example will illustrate the extent to which a case such as I have described may be deprived of the indicative circumstances so necessary for its diagnosis.

It occurred in the person of a young lady whom I attended with a surgeon in this town. Her age was nine. She was the last surviving member of her family, all her sisters having been cut off by consumption. Her health had been excellent up to the day of her fatal illness, which commenced as an ordinary attack of continued fever, entirely without any chest-complication. At the end of a fortnight, she seemed to improve, and her fever subsided; but it returned in two days afterwards with increased severity, and now was attended by evidence of irritation within the lungs, afforded by the presence of crepitation throughout the chest. It was at this time that I first saw the patient, and the remainder of her illness comprised only the short term of nineteen days. Her symptoms were those of high feverish reaction, without delirium of importance until the close of the case. Perspiration was present at times during the day, and sometimes was profuse; at night, the skin was dry and hot. The pulse at first was 120; respirations 60; but each afterwards rose to 180 and 70 respectively. General crepitation continued throughout the chest; but there was neither local dulness nor prolonged expiration; yet there was very little cough; and, as she swallowed her expectoration, it was probably in small quantity. As the disease progressed, the lips became livid, hæmoptysis occurred, and delirium set in. I was not able to obtain an examination of the body; but, as the case developed itself, no doubt could be entertained that the bronchitis, which was the only disease present to our observation, depended upon a special cause affecting the entire lungs, and that such cause consisted in a general formation of tubercle.

I may just add, that Dr. Evans, who saw the patient with us, mentioned to me a very similar case which had occurred to himself, in which also phthisis ran an acute course without any cough at all, although the physical signs of bronchitis were present throughout.

In the cases of which I now speak, the tubercles are deposited rapidly; and the progress of the case is usually rapid in proportion, though rarely so rapid as in the instance I have just detailed. Hence these cases are termed acute phthisis; and, as I shall mention to you immediately, other difficulties may arise in the diagnosis than that of distinguishing the case from one of pure bronchitis.

Precisely the same remarks which I have now made in relation to the bronchitis of phthisis apply also to the fever which accompanies the deposit of tubercle. As is the case with bronchitis, the liability to fever may depend on extraneous circumstances; but most generally it depends on the degree of rapidity with which the tubercles are formed. Hence the serious significance of a feverish attack in one suspected to be disposed to phthisis. The history of many consumptive patients is made up of a succession of apparently trivial attacks of fever—attacks which, in another individual, would hardly be thought worthy of special care.

After such an attack, you will commonly find your patient's health occupying permanently a lower level; and you may be assured that the constitutional reaction has been the outward evidence of an addition having been *made to* the disease already present in the lungs.

But it is a very important circumstance, that like bronchitis, there is

nothing in the fever of tuberculosis which of itself distinguishes it as belonging to that disease; its connexion is determined by collateral circumstances. The remark I made in respect to bronchitis, that we use comparison in forming our diagnosis far more largely than we perhaps imagine, is especially true of fever; and, as fever is so common an occurrence, and attends so many different maladies, the possession of means for instituting such comparison is of still greater importance than it is in the instance of bronchitis. We are sometimes left with the fever as our only guide, and are then forcibly reminded of our need of further help.

I attended a little boy, aged six, with one of the most experienced surgeons of this town. His illness lasted for eight weeks; my attendance comprised the last week only. His sole symptom was persistent feverish reaction, though with scarcely any delirium; the one simple symptom of a local character was constant rapidity of breathing. The child's chest was searched over and over again, both by my colleague and by myself, for any indication of disease; but, to the very last, percussion was normal; not even a wheeze could be discovered; and there was entire absence both of cough and of expectoration. Death was rather sudden—I suspect, from fainting. On examination, both lungs were positively filled with tubercles.

Not long afterwards, a second case, precisely similar as regards the entire absence of every symptom referable to the lungs, came under my care, also in consultation; yet the lungs were occupied by tubercles in every part. Here, however, there was inflammatory softening of the central parts of the brain; and symptoms of cerebral disease were prominent. Close attention was, however, bestowed upon the condition of the chest, for the purposes of diagnosis.

These instances exemplify another form of acute phthisis, in which, from the absence of explanatory indications, the fever at the outset of the disease is apt to be mistaken for typhoid fever.

But, after all, it is not of very frequent occurrence, that we are unable to diagnose the fever attendant upon the deposit of tubercle in the substance of the lungs, thanks to the assistance we mostly derive from the employment of auscultation and percussion. There is, however, another organ in which tubercles are liable to be produced, at least at an early period of life, and sometimes also in adult age, to which we have no means of applying physical examination. The cerebral membranes are often the seat of tubercle in childhood, and give rise to a most fatal affection. But the affection is also of a most insidious character. In the early stage of the disease, no characteristic symptoms of cerebral disorder develope themselves; the fever which attends the formation of the tubercles is alone present. Here there are no physical signs to aid us; and the diagnosis of the disease, previous to the setting in of decided cerebral symptoms, is confessedly a matter of great difficulty. "A few years ago," says Dr. Wilks, "I collected a large number of cases of this affection in children, and in none of them was a correct diagnosis made during the first few days of illness, the case being styled one of fever or stomach disturbance; and this error of diagnosis, or rather want of appreciation of the real nature of the case, is the rule." Dr. Wilks, however, draws attention to one distinctive characteristic, which may serve to arouse a suspicion as to the true cause of the fever; and I am sure that his observation will be found correct. He notices the irritability of the patient in the fever of acute cerebral disease, which contrasts in a most marked manner with the torpor of the fever-patient. When the disorders "are at their height," he observes, "and the patient is extremely ill, the fact is often very striking; the one with the inflammation of the brain being distressed by the slightest movement; it is irksome for him to be spoken to; he coils himself up in bed; is peevish, and wishes only to be left alone . . . . . whereas a patient with fever generally suffers nothing, and he evinces no trouble or alarm when touched."

Were we but able to apply to the brain the same method of physical examination we employ in the cases of disease of the lungs, no doubt the proportion of doubtful cases of this disease would be reduced to the smaller proportion observed in the instance of pulmonary tubercle.—*British Medical Journal*, *Jan.* 28, 1865, *p.* 81.

16.—ON THE USE OF HOT WATER AS A REMEDY FOR PROFUSE PERSPIRATION.

By Dr. Robert Druitt, London.

If a part of the body that is prespiring be bathed with quite *hot* water till it becomes decidedly hot and *red*, the skin will become dry, and will continue so for a greater or less period of time.

If *cold* water be used, the part remains cool for some time and then becomes gradually warm or glowing; if *tepid*, it is usually made unpleasantly chilly and flabby; if *warm*, it is left perspiring; if *hot*, it is left hot, red, and dry.

The terms cold, tepid, warm, and hot, are merely relative: what would be warm to one would be lukewarm to another person; but when I say *hot* water for our present purpose I mean water as hot as can be borne without pain. It may be used by sponging or immersion, and must be continued until the parts treated are hot, red, and *tingling* with heat—almost scalded, in fact. A good wipe with water at 130° is easily borne; for immersion the heat must be less; but the feelings are the only guide.

The circumstance which led me to recommend this remedy was the observation of the painful dryness of a hot skin in feverish attacks, and of the dryness produced by using too hot water in a bath, and by the clumsy use of the lamp-bath, which may make the skin dry instead of moist if not well managed.

The cases in which I have recommended it with benefit are—first, those of general tendency to perspire to a distressing degree in hot weather, the patient being in good health. If a man who has thoroughly used a cold bath in the morning be obliged to change his shirt in the middle of the day, for example, he will keep his skin comfortably dry for a certain time by a good wash with very hot water.

The next class of cases are those in which, with or without tendency to perspire over the body generally, but most probably without, there is a tendency to distressing perspiration of some particular part; as the axillæ, hands, feet, &c. Patients who seek relief for this are generally young persons from thirteen to twenty, and they often endure great misery and persecution in consequence of this symptom, which really admits of easy medical treatment. The health is sure to be improved by free purgation, and by quinine, air, exercise &c. But the distressing local symptom may be got rid of for hours at a time by the thorough application of the hottest water to the offending part until it is red, hot, and tingling as if scalded.

Thirdly, there are the cases of true hectic; diurnal shiverings, followed by heat, and drenching perspiration of an earthy, sickly odour, and depending (as we suppose) on absorption of decaying pus from some internal organ, probably lung, &c. I have caused profuse perspiration by the lamp-bath in the afternoon without preventing the access of hectic and perspiration in the evening; and confess my remedy inert—or next to it—in these cases.

But there is a fourth variety—the ordinary night sweat of the phthisical, not preceded by regular hectic paroxysm, but induced by all that relaxes, lessened by all that strengthens, and coming on when the patient falls asleep. For many of these cases the hot water gives relief, to a certain extent, especially if the perspiration begin, as it often does, on one especial part of the body by preference, as the chest, hands, or feet. In such cases I have the testimony of patients that the hot water greatly helps to control the sweat. The way is, when the patient goes to bed, to have the chest reddened with hot water, and wiped dry. One patient, whom I see daily, and who is confined to his bed, calls out for it so soon as he sees the dampness beginning, and has it used to chest, hands, and feet, and by this means often, not always, passes a night without being obliged to shift his linen.

In conclusion, let me say that I only offer this as a contribution towards the relief of an unpleasant symptom, and not as a cure for a disease; and that whoever uses it must recollect that it is not *warm*, but *hot* water, just *below scalding* point, that is to be employed.—*Medical Times and Gazette, March 4, 1865, p. 222.*

17.—ON THE AUSCULTATORY SIGNS WHICH PRECEDE CREPITATION IN PNEUMONIA.

By Dr. A. H. T. Waters, Physician to the Liverpool Northern Hospital.

Most authors since the time of Laennec have regarded the crepitating râle as the first recognizable sign in the pneumonic inflammation. Dr. Stokes has, however, asserted that an "intense puerility of respiration" precedes this phenomenon. "The physical signs of the first stage of pneumonia," he says, "are still to be determined with accuracy. Without possessing a sufficient number of observations to determine the point, I am led to the belief that an *intense puerility of respiration in the affected part* will be found to be the principal phenomenon. In cases in which inflammation was spreading upwards, I have often found that a puerile respiration preceded the crepitating râle for some hours; and that this was not a general but a partial condition was shown by its being much more intense in the vicinity of the disease than in the opposite lung. Indeed, in cases presenting great puerility of respiration with fever, we may often prognosticate the occurrence of the crepitating râle. Thus in a case in which the disease attacked both lungs as well as the pericardium, I observed this sudden appearance of intensely puerile respiration on three distinct occasions: in two it was followed by the crepitating râle and other signs of pneumonia, and in one it was removed by bleeding before the above signs had occurred."

I think objection might be fairly taken to Dr. Stokes's conclusion, founded on his observation, that a puerile respiration is heard in a pneumonic lung above the level of existing crepitation; for it might be said that near to the inflamed portion the lung would be doing greatly increased work, and thus we should expect to meet with a loudness in the respiratory murmur. Nor would such objection be fully met by his statement of the fact that the murmur was less puerile in the opposite lung than above the seat of crepitation. But the observation on which Dr. Stokes mainly depends in support of his view is the one he last refers to. In the case in question he met with the phenomenon on three distinct occasions, and in two of these unmistakable evidence of pneumonic inflammation followed.

To render the observation of this phenomenon, however, perfectly trustworthy, it must be made on a case, not where there is progressive inflammation, nor even where there are consecutive attacks of inflammation, but where, the lungs having been previously healthy, acute primary pneumonia comes on suddenly. Two such cases have recently occurred under my care, and have enabled me to confirm by observation the view which was originally advanced by Dr. Stokes.

Case I.—Patrick F., a carter, was admitted into the Northern Hospital, under my care, on August 8, 1864. On the day of admission, at an early hour, he was out in a shower of rain, got very wet, and did not change his clothes afterwards. In the course of two or three hours he felt pains about the limbs, and had severe rigors.

When admitted into the hospital about midday, he was seen by the house-surgeon. He then complained of pain in the lower part of the left side of the chest. There were no febrile symptoms, and no abnormal physical signs about the chest.

On the following day, about noon, his condition was as follows: The pulse was 120, and full; respiration, 32; skin very hot and dry; tongue coated with a white fur. The pain in the side had increased. There was no cough, but much dyspnœa. There was no deficient movement of the left side of the chest, and the percussion sound was natural. *At the lower and back part of the left lung a loud, harsh, peculiar respiratory murmur was audible.* No such sound could be heard elsewhere. The patient was ordered a grain of opium three times a day, with small doses of tartar emetic.

The next day the pain in the side had almost gone. The pulse was 104; the respirations were 28. The physical signs were as follows: Deficient movement of the left side; dulness at the left base, with crepitating râle over nearly the lower half of the *left lung*. The crepitating râle, which was dis-

tinctly of a pneumonic character, occupied, in fact, this day the seat of the harsh, loud respiration of the preceding day.

It is needless to follow the history of the case further. The crepitation was succeeded by bronchial breathing and all the symptoms of confirmed pneumonia. The patient made a satisfactory recovery, and was convalescent on the eighth day of the attack.

Case 2.—A Frenchman was admitted into the Northern Hospital under my care, on Jan. 23rd of the present year. He complained of dyspnœa and pain in the chest. On examination, *a loud, harsh, respiratory murmur* was heard over the lower and back part of the left lung. The movements of the side were good, and there was no crepitation nor dulness. The breath-sounds over the opposite lung were normal. On the following day the physical signs were as follows: Slight dulness at the base of the left lung, and well-marked crepitation over about the lower half of the same lung. In fact, as in the preceding case, the puerile respiration of one day was replaced by the crepitating râle on the next. The patient subsequently had all the symptoms of confirmed pneumonia—dulness, bronchial breathing, and rust-coloured sputa. He is now (Feb. 1st) convalescent from the attack.

M. Grisolle, in the recent edition of his work on Pneumonia, referring to the alleged puerility of the respiration in the disease, says that he has never met with it as an initial symptom, or as a precursor of the crepitating râle. He has, however, he says, frequently called attention to another phenomenon which he has observed in some cases of the disease—viz., a weakness of the respiratory murmur. He has observed this, he says, on several occasions as a precursor of crepitation.

I have never met with the physical sign which M. Grisolle speaks of; but of the existence of a loud, rough, harsh respiratory murmur, in the cases I have referred to, as an initial symptom of pneumonic inflammation, there can be no doubt. And although I would refrain from asserting that in all cases of pneumonia this harsh respiration precedes the crepitating râle, yet I cannot but think that the state of the respiratory membrane, at the commencement of the affection,—a subject to which I shall refer in a future paper on the morbid anatomy of the disease,—is just that which would be likely to produce the phenomenon.—*Lancet, Feb.* 11, 1865, *p.* 147.

18.—ON THE TREATMENT OF PNEUMONIA BY RESTORATIVES.

By Dr. John Hughes Bennett, Professor of the Institutes of Medicine, and Senior Professor of Clinical Medicine in the University of Edinburgh.

[During a period of six years and a quarter of active duty in the Royal Infirmary of Edinburgh, Dr. Bennett has treated 129 cases of acute pneumonia. Of these 105 were uncomplicated, and *all* recovered, although many of them were very severe. Amongst the twenty-four complicated cases were four deaths.]

In the 4 fatal cases, death was evidently caused by complications independent of the pneumonia. To arrive at true statistics with regard to treatment, therefore, it becomes necessary to eliminate them, and to fix our attention on the 125 cases which recovered.

Sex—Of the 125 cases, 85 were males and 40 were females.

Age—The average age of the males was 31¼ years. The average age of the females. 28½ years. The average age of both 30¼ years. Between the ages of 5 and 15 years was 1 case—a girl; between 10 and 20 years, 29 cases—12 females; between 20 and 30 years, 35 cases—11 females; between 30 and 40 years, 23 cases—7 females; between 40 and 50 years, 24 cases—6 females; between 50 and 60 years, 11 cases—1 female; between 60 and 70 years, 1 case—a female; and between 70 and 80 years, 1 case—a female.

Simple or uncomplicated pneumonia.—Of the 125 cases, there were 105 simple or uncomplicated, and 20 complicated. Of the former, there were 74 males and 31 females. 79 were single and 26 double cases. Of these, I find *that the clerk has omitted* to state either the exact day of rigor or of conva-

lescence in 6, so that no conclusion can be derived from them as to the duration of the disease. Of the remaining 99 cases, 73 were single and 26 double. The duration of the disease in the 73 cases of single uncomplicated pneumonia, counting from the occurrence of rigor to the commencement of convalescence, was as follows: 2 cases recovered in 5 days, 4 cases in 7 days, 5 cases in 8 days, 2 cases in 9 days, 8 cases in 10 days, 7 cases in 11 days, 7 cases in 12 days, 4 cases in 13 days, 13 cases in 14 days, 2 cases in 15 days, 3 cases in 16 days, 3 cases in 17 days, 3 cases in 18 days, 1 case in 19 days, 2 cases in 20 days, 3 cases in 21 days, 1 case in 22 days, 2 cases in 23 days, and 1 case in 26 days; the average duration, 13$\frac{3}{4}$ days. The duration of the disease in the 26 cases of double uncomplicated pneumonia, counting from the occurrence of the rigor to the commencement of convalescence, was as follows: 2 cases recovered in 8 days, 1 case in 9 days, 2 cases in 10 days, 2 cases in 11 days, 1 case in 12 days, 1 case in 13 days, 4 cases in 14 days, 1 case in 15 days, 2 cases in 16 days, 2 cases in 18 days, 2 cases in 19 days, 1 case in 20 days, 3 cases in 21 days, 1 case in 27 days, and 1 case in 55 days; the average duration, 16$\frac{3}{4}$ days. Of the 105 simple or uncomplicated cases, there were 9 bled by venesection and subjected to an antiphlogistic treatment before or immediately upon admission, before I saw them. The amount of blood extracted varied from 12 to 36 ounces; the latter in two bleedings. The duration of one case is not stated. Of the remaining 8 cases, the duration was as follows: 1 case recovered in 7 days, 2 cases in 14 days, 1 case in 16 days, 1 case in 17 days, 1 case in 20 days, 1 case in 27 days, and 1 case in 55 days; the average duration was 21$\frac{1}{4}$ days.

The average duration of residence in hospital of the single uncomplicated cases of pneumonia—excluding 2 cases in which the date of dismission is ommitted, making 77 cases—was 21$\frac{7}{8}$ days: for the males (52), 18$\frac{3}{4}$ days; and for the females (25), 27$\frac{1}{2}$ days. Of the 26 double uncomplicated cases, the average duration of residence in hospital was 23$\frac{3}{4}$ days: of the males (20), 23$\frac{7}{10}$ days; of the females (6), 22$\frac{3}{4}$ days. The average duration of residence in hospital of 8 cases bled by venesection early in the disease (the 9th case being excluded in consequence of the day of dismission not being entered in the case-book) was 32 days.

Complicated cases of pneumonia.—Of the 20 complicated cases of pneumonia, 16 were single and 4 double. Of the 16 single complicated cases, the duration of the disease cannot be determined in 3. Of the remaining 13, the duration was as follows:—1 case recovered in 7 days, 2 cases in 9 days, 1 case in 10 days, 1 case in 12 days, 2 cases in 14 days, 1 case in 15 days, 2 cases in 16 days, 2 cases in 19 days, and 1 case in 48 days; the average duration, 16 days. Of the 4 double cases of complicated pneumonia, 1 case recovered in 9 days, 1 case in 14 days, 1 case in 15 days, and 1 case in 18 days; the average duration, 14 days.

The extent of pulmonary tissue involved in the pneumonia was carefully determined in each case, and the average duration of the disease in the 95 single cases, deducting the unsatisfactory ones, counting from the rigor to the commencement of convalescence, was as follows:—One-quarter of the lung (2 cases), average duration 8$\frac{1}{2}$ days; one-third of the lung (12 cases), 12 days; one half of the lung (25 cases), 15$\frac{1}{4}$ days; two-thirds of the lung (34 cases), 14 days; three-fourths of the lung (6 cases), 14$\frac{1}{2}$ days; four-fifths of the lung (1 case), 12 days; the whole lung (15 cases), 10$\frac{3}{4}$ days. Of these 95 cases, the right lung was affected in 58, the left lung in 37. Among these 95 cases also the pneumonia was confined to the upper lobe in 11 cases, or nearly 1 in 9 of the whole; and the average duration of the disease in these was 13 days, and of their residence in the hospital 14$\frac{1}{2}$ days.

A careful study of the preceding facts will, I think, tend to establish some new truths and correct several prevailing errors with regard to pneumonia. I must remind those, however, who may yet be sceptical as to the value of a restorative treatment, and imagine that some of these cases might not have been pneumonia at all, that they were all diagnosed and treated publicly in the Royal Infirmary; were all examined, not only by myself, but by intelligent clerks and assistants; and were all made the subject of clinical lectures and commentaries *at the bedside*. There is therefore the positive certainty,

not only that every one of these cases was a genuine case of pneumonia, but that no other cases of the disease but what are tabulated were treated by me during the period referred to. It should be explained, however, that a few cases were partly treated by my colleagues, either before I assumed duty or after I left it, in the too frequent rotations which occur among the clinical professors in this University. Such cases are not inserted. It is also necessary to point out that two or three cases brought into the house by the police in an exhausted condition died before I saw them, and are also not inserted. It is the more important to refer to such occurrences, because they serve to account for the differences which must always exist between general hospital and clinical statistics. Every hospital physician must be aware that the general records of the house afford no index whatever to the number of cases of acute pneumonia treated clinically, comprehending as they do not only consecutive, latent, and chronic pneumonias, but not unfrequently cases of pneumonia, which have entered in a dying condition and have not been treated at all.

1. The first great fact which the preceding figures serve to establish is, that a simple primary pneumonia, whether single or double, if treated by the restorative plan, is not a fatal disease. Surely 105 cases, of which 26 were double, were sufficient to establish this proposition, especially when it is considered that they were diffused over sixteen years, and occurred in all seasons. Amongst these, also, the whole of one lung was involved in no less than fifteen cases, and the symptoms in many of them were exceedingly severe. Neither will any theory as to strength of constitution or change of type in disease explain the result, as several of the cases were those of healthy, vigorous young labourers, whilst others were those of weak and broken-down sempstresses. In any and every case the disease appears to go through its natural progress, so long as the body is not too much exhausted, and the physician as early as possible supports it by nutrients and restoratives.

2. As a general rule, prostration and weakening complications or remedies, not only materially lengthen the period of the disease, but especially prolong the convalescence. It is easily understood, therefore, how it happened that the antiphlogistic treatment of former days proved so fatal. The facts collected for me by Dr. Thorburn, from former case-books of the Royal Infirmary, prove that in weak cases a lowering treatment was still employed, though not, perhaps, to so great an extent as in robust persons.

3. It is generally supposed that the extent of the disease and the amount of lung affected must influence the result and duration of the disease. As to the result, all my cases recovered, even the 15 cases where the whole of one lung was involved, as well as the 26 cases where portions of both lungs were affected. In one complicated case, the whole lung on the right side, and two-thirds of the lung on the left side, were simultaneously affected, thus leaving only one-third of a lung to respire with; and yet, without bleeding, and with the aid of nutrients and restoratives, she was convalescent on the fourteenth day, and left the house quite well after a sixteen days' residence. With regard to duration, the extent of the disease does not exert so much influence as is generally supposed. If only a fourth of one lung be affected, the recovery may take place in eight days; but after that, whether the half or the whole of one lung, or two-thirds of both lungs be affected, it does not appear to cause much difference. Cases with half a lung pneumonic recovered in 15, with two-thirds of a lung in 14, with a whole lung in 10, and with portions of both lungs in 14 days on the average.

4. Since the observations of Louis, it has been supposed that a pneumonia at the apex of a lung was more fatal and more prolonged than one at the base; and so it may be with an antiphlogistic treatment. But with a restorative treatment, the preceding facts show that in 11 cases where the disease was confined to the apex, recovery took place in all, on an average, on the thirteenth day.

5. As means of palliating symptoms, and especially pain and dyspnœa, warm fomentations and poultices I believe to be the best and safest. Chloroform has been given by Varentrapp and others with good effect. No doubt *also small bleedings*, to the extent of eight or twelve ounces, give relief; but *in debilitated* persons are dangerous, and in all tend, by weakening the

strength at a period when the depressed system is struggling to regain its equilibrium, to prolong the convalescence and favour dangerous sequelæ. Still a small bleeding may be employed as a palliative with caution, to relieve engorgement of the lungs and congestion of the right side of the heart, although it is very rarely required. It should be remembered, in cases of double pneumonia, that there is often great dyspnœa on the sixth or seventh day, which will generally yield to warm poultices locally, and moderate doses of wine.

6. As a curative treatment I am satisfied that the best plan is rest in bed, nutritive drinks, especially good beef-tea, from the first, assisted by port wine—from four to eight ounces—if the pulse become weak, and solid nutrients as soon as they can be taken. The elimination of the exudation may be further assisted by salines (acetate of ammonia and small doses of tartar emetic—one-sixth of a grain) and diuretics (nitric ether), although nature will accomplish this herself if the strength of the body be maintained. All active purgatives, contra-stimulants, depressants, anodynes, and lowering remedies of every description should be avoided.

After carefully studyng all that has been written on the subject of pneumonia, as well as the numerous statistical tables of the results of various kinds of practice, I can only account for the remarkable success which has followed the restorative treatment in my hands by supposing that acute pneumonia is not a fatal disease, if the strength be supported and there be not complication. The former idea of medical practitioners, that it was a dangerous disorder, and required active depletion and antiphlogistics to prevent its passing into the suppurative or fatal stage, was erroneous, and the result of the imperfect knowledge of pathology which then existed. Suppuration, so far from being fatal, is, as we have endeavoured to show, necessary to the resolution of the disease; and a fatal result, so far from being avoided, was produced to the extent of one in every three cases. The late Dr. Todd, while he supported, also stimulated to a great extent, and the result of his practice was a mortality of one in nine cases. In not one of my uncomplicated cases has there, I repeat, been a death for sixteen years, although 26 were double cases, and 15 were cases in which the whole lung was involved. Why, therefore, any such case should have died in the practice of Dr. Todd I can only ascribe to unnecessary stimulation, as that seems to be the only point in which his practice differed from mine.

In an able article in the 'British and Foreign Medico-Chirurgical Review' for July, 1865, it was endeavoured to be shown, from the contrary results obtained by statistics, that the result was governed by hygienic laws or peculiarities such as age, season, climate, &c. I consider that my cases prove the fallacy of such reasoning; and that, looking at the time over which they extend, as well as all the other circumstances which are adverted to, it might be easily shown that the uniform good results in my uncomplicated cases depend on some other cause. That cause, I can have no doubt, is simply supporting the patient by nutrients and restoratives from the commencement. It is the want of that support which explains the mortality in the practice of those who, while they do not actually lower their patients, fail to see that, in a certain proportion of cases, either the disease itself or excessive stimulation exhausts and proves fatal.—*Lancet, Feb.* 25, 1865, *p.* 195.

19.—ON RHEUMATIC BRONCHITIS.

It will be granted that in reference to treatment that it is a matter of very great importance to study the nature of tissue changes in disease. It is more important to know that certain thoracic symptoms depend on rheumatism even than to diagnose the exact position of the pulmonary disease. In Dr. Laycock's lectures, published a few years ago, much attention was paid to this subject. If the views of Dr. Jones be correct, we have, in certain cases of bronchitis, more rational grounds for treatment. We can treat rheumatism by general remedies, as by the iodide of potassium and alkalies, with more hope *than a more or less local* disease like bronchitis, by expectorants or

other empirical measures. We may refer to some cases recorded in our Hospital Reports by Dr. Sutton a few months ago, illustrating, in a very definite manner, the relations of rheumatism to certain local affections.

We must, however, keep in mind that rheumatism and chronic bronchitis are very common diseases, and their co-existence does not by any means imply a relation. Besides, it is a common and useful practice to give iodide of potassium in chronic bronchitis, especially with emphysema, whether it be attended by rheumatism or not. Moreover, in Dr. Jones's second case the treatment was too complicated to enable us to judge what share the iodide had in the cure. The two cases here related can, then, only be considered as a text for remarks embodying the more general experience of a Hospital Physician on an interesting subject. We report Dr. Jones's remarks, in order that he may speak for himself.

Acute Rheumatic Bronchitis (under the care of Dr. JAMES JONES).—A. E., aged twenty, was admitted into Gurney ward with acute rheumatic arthritis, sharp fever, scanty high-coloured urine copiously depositing pink lithates. There was also much dyspnœa and cough, with all the physical signs of capillary bronchitis throughout both lungs, but no carditic complication. The dyspnœa was generally most severe at night. She was treated with alkalies in large doses and morphia: the joints were enveloped in cotton wool. Under this treatment both the arthritic and bronchial affections steadily and *pari passu* subsided, and in three weeks she was able to leave her bed.

Chronic Rheumatic Bronchitis.—M. R., a man of middle age, was admitted an out-patient with cough, dyspnœa, and chronic arthritic rheumatism. There is nothing in the physical signs to distinguish his case from one of idiopathic chronic bronchitis, but the symptoms run somewhat a peculiar course. The dyspnœa and cough suddenly subside, and as suddenly become aggravated without any apparent cause. The chest symptoms do not appear to be in any way influenced by the state of the arthritic affection, but seem to run an independent course. Under the use of iodide of potassium, bicarbonate of potass, compound tincture of camphor, and decoction of senega, both the bronchial and arthritic inflammations were greatly diminished. The patient declares that no medicine he had ever before taken did so much to his cough and breathing.

Dr. James Jones in commenting on these cases observed that the influence of chronic constitutional diseases in the production of bronchitis or the effect of constitutional diathesis in modifying bronchial inflammation, were subjects of great importance, and afford a field for inquiry which had been but little explored. As a part of this very extensive subject, he would direct attention to a form of bronchitis not by any means uncommon, yet requiring a special mode of treatment—the rheumatic. His attention was drawn to this form of disease by the large number of cases of chronic bronchitis in rheumatic subjects to be found amongst the out-patients, especially during the winter months, and the mode of treatment which afforded them the greatest amount of relief. In this disease clinical medicine demonstrates what pathology suggests—from their complex anatomical constituents—that the bronchi must be liable to rheumatic inflammation. There appear to be two forms of this disease—the one purely rheumatic, of which the first case affords an example; the other of a mixed character, produced by the modifying influence of rheumatism, or the rheumatic diathesis on ordinary bronchitis exemplified by the second case. The majority of cases of rheumatic bronchitis exist in the chronic form. The physical signs give no clue to its nature, as they are identical with the corresponding idiopathic form. But the irregularity of its course, the seeming caprice which rules its exacerbations and its subsidences, and its amenability to those remedies which are found useful in relieving the arthritic rheumatism will enable us to differentiate between it and the idiopathic disease. Like arthritic rheumatism, it is often aggravated by the warmth of the bed, the dyspnœa becoming so severe as somewhat to mimic a paroxysm of spasmodic asthma, being like it also, *relieved* by a copious expectoration. Dr. Jones has not yet had any opportunity of determining the nature of the structural lesion, or whether it differed *anatomically* from that which accompanied the idiopathic disease. The

treatment which he had found successful consisted of a combination of the alkalies and opium in the acute form, with the addition of iodide of potassium and senega in the chronic; an underdress of flannel, and the use of a respirator, as a protection against those atmospheric changes which exercise so great an influence over rheumatism.—*Medical Times and Gazette, Jan.* 7, 1865, *p.* 10.

20.—ON THE VALUE OF CHLOROFORM IN THE TREATMENT OF ASTHMA.

By Dr. Hyde Salter, F. R. S., Assistant Physician to Charing Cross Hospital.

[The inhalation of chloroform is beyond doubt one of the most powerful methods of treatment of the asthmatic paroxysm that we possess. The most common fault of it is that the relief afforded is transient only, and merely coextensive with the insensibility produced. In some cases however, when the insensibility passes off the asthma does not re-appear, and in some the relief is produced without any insensibility whatever.]

There can be no doubt, I think, that chloroform dissipates the asthmatic spasm by relaxing muscular contraction, just as it will dissipate hysterical contraction of the *rectus abdominis*, and thus disperse a phantom tumour, and that it acts through the general nervous system. But I have seen one case, which I shall relate, in which it seemed to act directly on the bronchial muscle. I conclude this to have been so because I think the effect was too immediate for it to have taken place *viâ* the circulation and the general nervous system: the first act of inspiration would be accompanied with a sensible relief long before the blood charged with the chloroform could have reached the nervous centres.

Patients and their friends have often asked me if there was not danger in giving such an agent as chloroform in the height of an asthmatic paroxysm. And truly, looking at the alarming state of semi-asphyxia to which the asthmatic paroxysm often amounts—the turgid face, the small pulse, the struggling respiratory muscles, the almost absolute standstill to which both respiration and circulation are brought—one would be apt to think that it would take very little more to stop both the one and the other, and that it was not exactly the condition for which to administer a drug having so depressing an influence on both these functions. I can only say, however, that I have given chloroform in the very agony of the worst attacks; that so far from fearing it under such circumstances, it has been to relieve the intensest asthma—that which nothing else would reach—that I have most given it, and that I have never seen any bad effects from it. Indeed, the immediate and direct effect of the chloroform is to remove that which is the whole cause of the asphyxial stoppage—the bronchial spasm,—and to set the pulmonary circulation free. No sooner does it enable the patient to fill his lungs than the loaded right heart disburdens itself, the lividity and venous turgescence disappear, and the pulse regains its normal volume. The intensity of asthmatic asphyxia, so far from being a reason against the administration of chloroform, is the great reason for its immediate employment. I grant that if the same amount of lung-stoppage depended on any other cause than the bronchial spasm (at least on any cause that chloroform would not relieve) its administration would be highly dangerous.

I may add that my experience does not induce me to believe that the presence of valvular disease, or muscular weakness of heart, adds anything to the danger of chloroform, unless these conditions exist to such an extent as materially to affect the circulation. I believe that chloroform may be as safely given to a man with an aortic bruit as to one without one, provided there be no *symptoms* proper. I believe the circumstance that determined whether chloroform shall exercise a fatal influence on the heart's action is, not the presence or absence of organic heart disease, but some idiosyncrasy of nervous organization.

Case 1.—F. E.——, *aged sixty-three*, engaged in the practice of the law,

a sufferer from asthma of many years' standing, first began to take chloroform at the end of 1861, two years and three quarters ago, most probably at my suggestion, to relieve the violent attacks from which he then suffered. The dose in the first instance was small, and the effect always satisfactory and immediate. Not more than fifty minims was given on each occasion, and probably it was some time before that dose was reached. It never failed. One effect noticed then, which has been remarked ever since whenever the chloroform is given in full doses, was that it always made him feel chilly when recovering from its influence. It was applied on a handkerchief twisted into a hollow cone, into which the chloroform was dropped. The time at which it was exhibited depended entirely on the time at which the asthma was sufficiently bad to require it, and that was generally in some part of the night. The effect was then, as it has been ever since, and is now, immediate; after a breath or two had been drawn the spasm was felt to be giving way, and within a minute it was gone; and consciousness was gone too.

At this time the effect was not only immediate but final; the asthma was over for that bout, and the quantity necessary to finish it off was certainly never more, as has been stated, than fifty drops, probably not more than thirty. After having had an attack of asthma cured by chloroform, he was in the habit of considering himself "good for a week."

Since that time the asthma has gradually changed its character, and the attacks, instead of occurring in paroxysms of great severity at long intervals, have occurred more and more frequently and in a milder and milder form. As a result of this the chloroform has been more and more frequently called into requisition, and, again as a result of this, has gradually lost its power. Thus the dose required to relieve the spasm has continually increased.

The quantity required at different times would be various; sometimes ten minutes would produce a complete and marvellous effect—completely stop the asthma, and send him off in a tranquil sleep for two hours; at other times dose upon dose would produce no effect, or next to none. On one occasion he took three ounces of chloroform between one o'clock in the morning and nine o'clock in the evening. The reason of the difference at different times seemed quite inscrutable. I have observed it in other cases.

From the beginning, the taking of chloroform was followed, many hours afterwards, by two symptoms—nausea, and a copious secretion of viscid saliva from the mouth. If, for instance, he took the chloroform at the usual time, say four o'clock in the morning, he would have no nausea at the time, nor in the morning on taking his breakfast, which was generally a most substantial one, nor at chambers. But almost immediately after leaving chambers, at half-past four P. M., as soon as his mind was relieved from the preoccupation of his duties, he became conscious of the nausea. On his way home it would become very troublesome, accompanied by this profuse secretion of saliva, which was so great that, to his infinite disgust, he had constantly to get rid of it in the streets. It was frothy and viscous, so that sometimes he had great difficulty in getting his lips clear of it. On reaching home the nausea was sometimes so bad that he could take no dinner; generally he could and did, and usually found that the act of taking food relieved the sensation. Sometimes, however, after having taken a good dinner it all came up again. For the rest of the evening he was *hors de combat*—could do nothing, sat in his arm-chair in a state of exhaustion with a basin by his side; and this would continue till it drove him early to bed at eight or nine o'clock. The next morning on waking, supposing he had no asthma or chloroform, he would still feel the nausea, but in a diminished degree, often, however, so much that he could take no breakfast. On reaching chambers all sense of nausea would vanish, and would not be felt till business was over; then it would be felt again, but in a still less degree. And so it would go on till the second or third day, the flow of saliva and the nausea both gradually decreasing, when they would both finally cease. If, however, he took chloroform the second night, the nausea at breakfast-time the next morning would be still stronger, and would last longer—go on an additional day.

Besides these effects of the chloroform, which may be called the primary *and immediate ones*, certain more remote results gradually developed them-

selves in proportion to the length of time that the habit had existed and the increased quantity taken on each occasion. These secondary effects of the prolonged use of chloroform, as shown in this case, were—

1. Insomnia.
2. Deafness.
3. Apathy.
4. Tremulousness of hands.
5. An increase of the asthmatic tendency.

The most strongly marked and the most distressing of all was the insomnia. It came on very gradually, and for a long time the cause of it was not suspected. The patient was habitually a good sleeper, and the wakefulness that gradually showed itself was a strange and unaccustomed thing. In spite of all he could do, the hour at which he fell asleep became later and later—two, three, four o'clock, till at last it was six or seven. Indeed, he may be said ultimately to have had no sleep at all; two or three hours in a week was all he would get, and it became a wonder both to him and to me how he could exist with so little sleep and not only exist, but pursue professional labours involving a great deal of close attention and mental wear and tear. When he went to bed he lay perfectly tranquil; there was not the slightest restlessness, but it seemed as if all tendency to sleep was abolished, as if he had no sleep in him, and never should want to sleep again; he was tranquilly wide awake. Finding sleep hopeless, he spent the chief parts of his nights reading. Everything that could be thought of in the shape of a sedative was tried. At one time it was thought that Indian hemp was giving him sleep, but it turned out afterwards that it was the remission of the chloroform that *allowed* him to sleep, and not the Indian hemp that was *making* him sleep. The dependence of the insomnia upon the chloroform was discovered in this wise: The first exhibition of the Indian hemp was accompanied by a remission of the asthma, and therefore with a cessation of the chloroform. After it had been given a night or two it was observed that the patient was sleeping a little towards morning, and night by night the time at which he fell asleep became earlier and earlier, till at last he would be so sleepy at ten o'clock (or even nine I think) that he could not keep himself awake, but was obliged to go straight to bed. The Indian hemp had been given early, at seven o'clock, in order that its influence might come on the sooner; it was now, therefore, given later, just before bedtime, but so great did its soporific effect appear to be, so immediately did an irresistible sleepiness come on, that the dose was reduced, and great was the credit that the "bang" received as an invaluable hypnotic. The sleep was everything that could be wished, and the neck of the insomnia was supposed to be broken; the Indian hemp had done its work, and was left off. But now came asthma, and with it chloroform, and within a few days the insomnia difficulty re-appeared: sleep came on later and later. The "bang" was again resorted to with confidence, but this time without success; the sleeplessness became worse and worse, till it was as bad as it had been before the hemp was given. The murder was now out; it was clear that it was the chloroform that produced the insomnia; that its former cure had depended on the remission of the chloroform, and not on the administration of the Indian hemp, and now that the chloroform was keeping it up, the hemp was powerless. It was very curious to see, as the chloroform continued to be given, the gradual way in which the time that sleep would come on was postponed, just as previously, when the chloroform was left off, the time at which sleep supervened was a little earlier each night. I may here mention that, in confirmation of this view, the return of sleep has again followed the remission of the chloroform. My patient has now, at the time that I am writing these notes, left off the chloroform for twelve days, and he has, in the same gradual way as before, recovered his power of sleep, and this time without the use of the Indian hemp. I am not aware that this tendency of the protracted use of chloroform to produce insomnia has ever been noticed before.

Another effect was *deafness*. My patient has long been a little deaf, but after he had taken the chloroform for some time this deafness was greatly aggravated, and became a matter of much inconvenience to himself and his

friends. His mind also seemed to be affected; his intellectual powers were as good as ever, but he seemed to have lost the disposition to use them, and he would sit for hours in a sort of apathy, neither writing, nor reading, nor conversing, although naturally a man of great mental energy. He noticed, too, that his hands were tremulous, and that he had not his customary steadiness in delicate manipulations with them.

There cannot be a doubt, too, I think, that although the chloroform never failed to relieve the asthma at the time, it tended to increase the asthmatic tendency in the long run, and this probably by rendering the nervous system more susceptible, shaky, and irritable. Certain it is that the longer the chloroform was given, the more frequent and obstinate did the asthma become, and the more transient was the control that the chloroform exerted over it. Slighter things, too, would bring it on: the patient has told me that he is sure that an attack of cough which, now that he has left off the chloroform, has no power to bring on the asthma, would infallibly have done so at the time he was taking it in such quantities. At that time, too, phlegm coming away would secure no immunity. A good deal of this "induced" asthma, however, was not "the genuine article," but a spurious imitation. I was sent for one night in great haste to see him, and was told of the immense quantity of chloroform that had been taken, and the persistent recurrence of the asthma in spite of it. But on closely watching my patient I soon discovered that what he was suffering from was not true asthma at all; there was no real distress about the breathing, and I believe no narrowing of the bronchial tubes. The differences between this "induced" and true asthma are, that it is rather a rapid and panting than a tight constricted breathing; that though loud, it is unaccompanied with wheezing; that it will suddenly cease spontaneously, remain absent from two and a half to three minutes, and then suddenly re-appear; that whenever he speaks it ceases, or whenever anything emotionally excites his attention. On the occasion in question I refused to give any more chloroform for the mitigation of this spurious dyspnœa, and in a quarter of an hour it was gone. I believe that by keeping up the chloroform condition it might have been indefinitely protracted. From that time my patient and his attendants have been able to recognize the difference between true asthma and this spurious dyspnœa. And the recognition of the difference is practically important; as the chloroform, although so effectual in relieving the true asthma, is not only useless against the spurious form, but inevitably keeps it up.

Such in this interesting case, were the secondary results of the prolonged use of chloroform. They are such as I think offer a very serious bar to the further use of the drug. It was impossible to observe the shattered condition of my patient, and not feel that the further pushing of the remedy would entail a worse condition than the asthma; that of the two evils, it would be the choice of the greater. And, with this case in view, I should, on any future occasion on which I might advise the use of chloroform, look forward to the possibility of having to prohibit its further employment, lest a worse thing than asthma came upon my patient. Chloroform-taking is like other dram-drinking, gains the same hold upon the patient, and leads to the same results. Indeed it is impossible not to see, in the case that I have related, a very close resemblance between the symptoms and those of chronic alcoholism—the vomiting, the hand-tremor, the mental enervation, the insomnia. It is a good rule, and of universal application, that we should be slow in ordering the habitual use of any remedy whose ultimate effects are deleterious, especially in cases in which the dose is likely to become large. And just as I should be chary of commencing the habitual use of alcohol or opium in any case of a chronic nature, so should I that of chloroform.

Case 2.—L. B., a lady aged forty-nine, who has long been a victim to asthma, is accustomed to get relief from her symptoms in the following way:—About seven or eight o'clock in the evening, when she feels she could go to sleep, she pours ten drops of chloroform on a pocket-handkerchief, and inhales it. In two minutes her asthma is gone, and, partly probably from *the cessation* of the fatiguing efforts of breathing, and partly from the effects *of the chloroform*, she is sound asleep; sleeps, perhaps for six hours, and

wakes breathing quite freely. On one occasion, when I was called to see her in a very bad attack, knowing that so small a dose of chloroform as ten minims would stop an attack in the way I have described, I determined on administering it, in the confident expectation of giving her immediate relief. What was my surprise, however, to find that, although each dose that I administered gave her temporary ease, as soon as the effect of the chloroform passed off the asthma returned. I continued dosing her for about an hour, having given her in that time three or four drachms; and, when the effect of the last dose passed off, her asthma returned as severely as before I had administered any. It was quite clear that I was gaining nothing, and that for permanent relief the chloroform was worthless. I therefore desisted.

Subsequent experience has cleared up the mystery, and shown that chloroform only gives her permanent relief when it sends her into a continuous sleep; and that it only sends her into a continuous sleep when she takes it at such a time as she would, if free from the asthma, be likely to sleep continuously—as, for example, near her ordinary bed-time. Given at any other time, it produces just as much momentary relief; but the state of sleep being transient—the chloroform sleep not passing into natural sleep—the asthma returns with the consciousness. This curious fact is consistent with another fact in this lady's case, and probably depends upon it—that she never has asthma when she is asleep, and that if she has asthma and can in any way get to sleep, her asthma is sure to cease; so, on the other hand, she is never awoke by asthma, like other asthmatics, but wakes free, her morning asthma appearing *immediately after* she is awake, and the time that it comes on depending entirely upon the time she wakes. The correct interpretation, therefore, of the cure of this lady's asthma by ten minims of chloroform is, that the chloroform temporarily relieves the asthma and sends her to sleep, and the sleep prevents the recurrence of the asthma.

I have repeated the chloroform experiment in this case on several occasions, and always with the same result. Any quantity is valueless to give more than a transient relief, unless given just at the time when sleep is disposed to supervene; but given then, ten minims is just as potent to cure an attack now as it was formerly.

The incompatibility of sleep and asthma which characterizes this case is quite peculiar; indeed, it is the very reverse of what one ordinarily sees, sleep being commonly one of the most powerful predisposing conditions of asthma. However, it is a peculiarity that is very fortunate for the possessor of it, for it evidently imparts to chloroform, in her case, a power that it would not otherwise possess.

Case 3.—The following short notes of the effect of chloroform in one of the most distressing and intractable cases of asthma I ever saw I received only a day or two ago:—"And now about chloroform. I can hardly tell you much about it, as I have tried none since I left Surrey-street; and there, and elsewhere before, I only tried it once or twice at a time. It struck me, however, that its bad after-effects were greatly in excess of the short relief I obtained by the stoppage of the paroxysm. Latterly I inhaled from half a drachm to forty minims during the height of a paroxysm. In a few minutes I began to feel queer, but a delightful sort of queerishness; the paroxysm disappeared, and I could lie back half asleep, and breathing, as it appeared, naturally. But this effect wore off in half an hour or so, and the paroxysm would begin again, aggravated by a very distressing sort of feeling. I have not tried it in repeated doses, the first after-effects being so bad; in fact, it acts very like any preparation of opium does on me, but quicker, and the relief does not continue so long; the paroxysm returns sooner, and the distress is greater than after opium; but it does not make me so unwell the next day."

Case 4.—A medical man, formerly a patient of mine, and in whose case chloroform was the only thing that gave any relief, thus describes, in notes that he sent me, the effect that it produced on him, and the method of its administration:—"The effect of the chloroform was not immediate in subduing the paroxysm, but was instantaneous in giving temporary relief by relaxing the spasms of the muscles of the bronchial tubes; but if I ceased its inhalation the symptoms returned in unmitigated force. As to the time I had to

continue its use, that depended upon the severity of the paroxysm; and as to the quantity, I can scarcely give you a correct estimate, as Mrs. H. poured about a teaspoonful upon a handkerchief, and this was repeated as soon as the chloroform had evaporated, and continued until the urgency of the symptoms was removed. The effects were always accompanied by an accelaration of the pulse; and after the dyspnœa was relieved I usually went off to sleep."

I might multiply such narratives as these to a great extent, but I have given enough to show the value and efficacy of the remedy, and to illustrate some points of its physiological action.

On the whole, my experience of chloroform induces me to conclude—

That it holds a high place amongst the remedies of asthma; that there is probably no one agent that relieves in so large a number of cases.

That it operates with very various completeness in different cases.

That even where it does not *cure*, it is of great value by a temporary respite.

That no amount of asthmatic dyspnœa or asphyxia is any bar to its use.

That, if given constantly, however, in large doses, for a long period, a state of things arises which does, in my opinion, constitute a bar to its continuance. —*Lancet, Nov.* 5, 1864, *p.* 515.

21.—ON THE USE OF THE LARYNGOSCOPE.

By Dr. E. H. Sieveking, Physician in Ordinary to the Prince of Wales, and to St. Mary's Hospital.

Before speaking of laryngeal pathology as exhibited by the laryngoscope, let me say a few words anent the apparatus to be employed, the method of using it, and the points to be observed.

What do you want to do? In plain English, you want to look round a corner into a dark hole. This sounds paradoxical, but the paradox is solved by the laryngoscope.

The light either of the sun or of a lamp is received on a mirror attached to the observer's forehead, from which it is reflected on to a small mirror which is introduced into the fauces of the patient, and from this mirror again the light is thrown down upon and into the larynx. The angle of incidence being equal to the angle of reflection, you have merely to place your mirrors in such a relation to one another as to secure the proper direction of the rays of light, and a perfect and distinct image of the parts illuminated will be visible on the small mirror.

The sun, at all events in London, cannot be relied upon at any hour of the day; and patients would not, even if the sun were always shining, present themselves when his rays penetrated a given locality. We therefore find it much more convenient to trust to artificial light altogether, and accordingly have recourse to a gas moderator, or, as you see here, a paraffin oil lamp, which are not amenable to the caprices of the weather.

It is well to darken the room in which you make your examination; but even this is not absolutely necessary if the light is protected by a convex mirror on one side, and concentrated by one or more lenses on the other. Various complex apparatuses have been devised for the purpose of intensifying the light. What you see here is an arrangement for which I received the suggestion from my friend, Dr. Johnson, and which has been most satisfactorily executed by Messrs. Elliot and Co. The lamp is a paraffin oil lamp, which gives a brilliant white light; on one side is a convex mirror, which reflects the light upon the lens opposite to it, and this, an ordinary bull's-eye, concentrates the light upon the reflector. The lamp should be placed close to the patient's head, on either side most convenient to the observer, and so that the lamp and the patient's and observer's heads are in the same horizontal plane. The patient, sitting erect, should push his head back so as to straighten the neck and facilitate the introduction of the small mirror.

Before attempting to do this, the observer should first ascertain that the *reflector is* properly adjusted. If he finds that the mouth is fully illuminated,

he may fairly conclude that he will be able to throw the light upon the fauces. Beginners generally find a little difficulty in adjusting the reflector, but this is speedily surmounted. Whether you place your mirror on your forehead, as recommended by Dr. Johnson, or whether you prefer to use it as I generally do, is a matter of little moment. Try both ways, and adopt the one you like best. The mirror I employ has, as you perceive, a central uncovered spot. It is an improvement upon the original mirror employed by the father of laryngoscopy, Professor Czermak, of which I show you a specimen. The one I recommend has a small universal joint, and, being placed a little distance from the eye, is much easier of adjustment. I admit that with Professor Czermak's instrument I had a difficulty in seeing through the central orifice, but with this modification I find it advantageous first to adjust the reflector so that I am enabled to see the fauces through the opening with the eye covered with the reflector, and then I am certain to have the proper axis for both eyes. After you have satisfied yourselves that the lamp and the reflector (which is fixed on the head by an elastic band passing over the forehead) are in the right position, tell your patient to open his mouth widely, to protrude the tongue, and to breathe freely. Some patients will permit you to see into their larynx without in any way fixing the tongue, but this is the exception. As a rule, it is necessary that the patient, or the observer, should take hold of the tip of the tongue with a handkerchief or towel, to prevent this "unruly organ" from slipping back. Without this precaution you very often fail in your endeavours to see below the epiglottis. Your next step, after having got your patient in the proper position, is to warm the laryngeal mirror. This is necessary, to prevent the vapour of respiration being precipitated on, and dimming the mirror; but take care not to overheat it. As a precaution it is well to test the temperature by applying the back of the mirror to your own cheek.

Take hold of the stem of the mirror as if you were holding a pen; introduce it into the mouth without touching the tongue, teeth, or lips, sinking the hand at first, and then raising it gradually so as to allow the mirror to form a curve until it reaches the uvula. Almost the whole secret of the manipulation in laryngoscopy consists in this. If you touch the lips or teeth you tease the patient, but if you touch the tongue you are certain to excite reflex action, and the root of the tongue will arch up to impede your view. *Audacem fortuna juvat.* Push your mirror, after having passed the janitors, well against the uvula, and you will rarely meet with any serious impediment in getting a proper view of the larynx. An unsteady hand creates difficulties which need not exist, by bringing the mirror into contact with other parts than the uvula and soft palate. These are not very sensitive, but if you tease the root of the tongue or the arches of the palate, you infallibly excite reflex action, which will necessitate the withdrawal of the instrument. Having placed your mirror at an angle of about 45° with the horizon, it is well to rest the little finger of the right hand against the patient's cheek, by which means you will steady the instrument. You then secure a proper illumination of the laryngeal mirror, and by the necessary manipulation, which practice alone can teach, you successively examine all the parts exposed to view.

In making the examination and recording the facts observed, you must remember that you are looking into a mirror, which represents the parts in a different relation from their real position. The epiglottis, which in nature is turned from the observer, is represented as opening towards him; the base of the tongue, which is in front of the epiglottis, appears in the mirror behind it; and the vocal cords equally occupy in the mirror a reversed position from that which they really possess. As laryngoscopic illustrations represent the appearances as seen in the mirror, it is necessary to remember the true relation in order to understand the drawing correctly. The lateral relations will cause less difficulty than the antero-posterior relations. What ordinarily appears to the left or right in the subject of observation placed before us, still continues so. We readily make the necessary allowances here from the habit engendered by custom; but it is different in regard to objects placed in front of each other, and reflected in a mirror, because for this our daily life offers us fewer precedents. Perhaps the easiest way to realise the position of the parts as seen in the mirror is to imagine yourselves

looking at the epiglottis and vocal cords through a hole in the cervical portion of the vertebral column. These points will become more apparent if we briefly examine the diagrams and preparations which I have placed before you. To the beginner I would say, Make your first experiment upon a case in which you are informed there is no special difficulty; remember the rules laid down; bear in mind the anatomy of the parts and the direction in which you wish to carry the light; and, with a light and steady hand, you will scarcely fail, after one or two attempts, to see that which is ordinarily visible by the aid of this instrument.

Now, what are you to look for? The first point that always attracts the attention of the observer, after having got the light and the mirror duly placed, is the epiglottis. It is the Cape of Good Hope of the laryngoscopists, and it is at times a difficult matter, a very difficult matter, to round the Cape. You will find that it varies as much in form as the nose, and its position not unfrequently is so prone as to render a good view of the subjacent parts almost an impossibility. Then, too, in irritable persons it undergoes a variety of contortions and contractions, which give it a character for muscularity greater than anatomists show it to possess. Still, as a rule, it serves merely as a land-mark anatomically and pathologically: anatomically, because its well-known relations tell you in what direction to look for more important organs; pathologically, because the appearance of the mucous membrane covering the epiglottis is often a valuable indication as to the state of the subjacent parts. Examine the epiglottis carefully, the form, the colour, the attachments. It should be perfectly smooth, of a pale, yellowish-rose hue, and symmetrical. Look at the fossa formed by the front of the epiglottis with the base of the tongue, and note the state of any secretions upon or near the epiglottis. In disease you will see its colour varying from the dull white of anæmia to every shade of uniform, or streaky and patchy redness. It may present ulcers of varying size and depth; it may be deformed by old cicatrices or congenital malformation; tumefaction from inflammatory or œdematous thickening of the mucous and submucous layers may present itself; and it may exhibit extravasations of blood, or be more or less bathed in pus, or covered with a mucous secretion. Similar conditions may be discovered in the glotto-epiglottid fossa or on the glotto-epiglottid folds. In order to see the parts subjacent to the epiglottis, you will have to depress the handle of your mirror somewhat, so as successively to illuminate the posterior surface of the epiglottis; the arytenoid cartilages, with the corpuscula Santorini and Wrisbergii; the superior thyro-arytenoid folds, or the false vocal cords, as they are also called; and by turning the mirror laterally you will examine the right and left sides of the introitus laryngis, and especially the state of the ventricles of the larynx. These are cul-de-sacs intervening between the vocal cords proper and the superior thyro-arytenoid folds. The state of the ventricles has an important bearing on the production of the voice, inasmuch as their patulousness is essential to the free vibration of the vocal cords and the due production of voice. If the mucous membrane of the ventricles is swollen, the pitch and sonorousness of the voice is interfered with, and the more the pouch is obliterated the greater will be the interference with phonation. It is here that various secretions form and accumulate, and you will readily understand why these should more or less affect the voice, as they present obstacles to the passage of the air, or diminish the vibrations of the aërial pulse. If the secretions are viscid, you may at times be puzzled by strings of mucus extending across the entrance of the larynx, closely simulating the vocal cords themselves. Little patches of secretion may also simulate ulcers. The sponge or brush will readily remove such secretions, and show the condition of the subjacent membrane. The ventricles, moreover, as in the case of E. G., whom I have already exhibited to you, and whom you will have further opportunities of examining, are the frequent seat of morbid growths. All these points having been noted, you seek the vocal cords themselves.

In a healthy larynx the vocal cords stand out, with a clear pearly sheen *which is peculiarly characteristic. Once seen, the appearance is not to be forgotten; and* you will often, in doubtful cases of laryngeal disease, rejoice

at recognising this striking feature, because it will prove that the most important part involved in phonation (so far as the larynx is concerned) is healthy. The vocal cords, or inferior thyro-arytenoid ligaments, are mainly composed of yellow elastic tissue, but are endowed with the most marvellous capability of minute vibratile adjustment, subject to the controlling power of the will, exercised through the arytenoid, thyro-arytenoid, crico-arytenoid, and other muscles. It has been calculated that no less than one hundred muscles are brought into action in the ordinary modulation of the voice, but the note which is uttered depends upon the exact degree of tension of two ligaments, at the utmost seven lines in length, which is mainly determined by the two sets of muscles mentioned. You will have a measure of the minuteness of this adjustment when you reflect that a practised singer is capable of uttering three hundred different notes at will, for each of which on this minute vibrating cord a different stop must be applied.

The vocal cords are covered with mucous membrane, distinguished from the mucous membrane of the rest of the larynx, which is ciliated, by being squamous. The mucous membrane overlies the elastic tissue of the cords, and is liable—though to a much less extent than the mucous membrane in the vicinity—to congestion and the various morbid changes which are seen in this tissue elsewhere. An accurate knowledge of the anatomy and physiology of the parts will enable you more fully to appreciate the importance of minute shades of difference in the appearance of the parts. A roughness of the surface or a discoloration which would lead to no palpable results elsewhere, here affects the comfort, the occupation, the life of the patient, and is therefore well deserving the study of the practitioner by any additional physical means that may be placed at our disposal. Here, too, we have a good illustration of the difference between vital morbid conditions and the condition of the same parts as seen after death: a difference that you should always bear in mind, as you may otherwise easily be misled into a wrong interpretation of the phenomena presented on the post-mortem table. If you had merely seen the interior of a larynx removed from the body, you would scarcely anticipate the marked contrast that exists in life between the vocal cords and the adjacent parts, nor would it be possible satisfactorily to determine the mode in which the variations of sound are produced by the vocal cords. You will find that there was much uncertainty as to the theory to be adopted regarding vocalization even in Müller's time; and it was not until after the practical introduction of the laryngoscope by Czermak that the study became satisfactory and the conclusions definite. If I name Czermak, it is not because I do not appreciate the labours of others in this field; but whatever others have done, he certainly has compelled us all, by the demonstration of the comparative facility of laryngoscopic examination, to make it a part of our medical studies.

It would be an injustice, in speaking of the subject, not to mention the name of M. Garcia, a well-known singer and amateur physiologist, who established by laryngoscopic examination much that is now known as to the physiology of the larynx before Czermak had made known his method; but the pearl that M. Garcia discovered was not appreciated by our profession, and therefore, so far as the medical world are concerned, laryngoscope remained an unknown quantity until the appearance of Czermak's monograph.

It is not my object for the present to do more than to interest you in the practical employment of the laryngoscope in the recognition and treatment of disease; therefore I do not attempt to lay before you an account of laryngeal physiology—a branch of science which is capable of further development, and which some of you may feel called upon to promote. Allow me yet to revert to a few points connected with the pathology of the larynx, to which I would draw your attention, as illustrating the value of this mode of investigating disease. The practical examination of the numerous cases that present themselves in our hospital will serve to impress upon your memory and comprehension more vividly what I now merely show you *veluti in speculo*.

I have spoken of various morbid changes seen in or near the vocal cords, showing *increased or diminished* vascularity, congestive or œdematous swell-

ing, ulceration, cicatrices, growths, all of which I have myself seen. If you consider the muscular and the nervous functions of the parts, you will expect to see these also materially affected by disease. The *plus*—evidenced by spasm—is not likely to be very visible, because it will not leave you time for anything but immediate action to relieve your patient; but the *minus* of paralytic conditions is frequently observable in the irregular action or want of action of one or both vocal cords. It is here that stimulation, and notably the direct application of galvanism, is often of palpable benefit. And you will not examine many larynges before you will satisfy yourselves of the perfect facility with which you may direct the galvanic current, as well as any other medicinal application, to any given part of the larynx. This of course can only be learned by practice, and when the patients are before you; therefore I confine myself to exhibiting some of the apparatus suitable for topical applications, and leave the detail of treatment to clinical demonstration.

I have not, however, quite done yet. You may see further than the vocal cords. Their under surface can at present only be examined when there is a hole in the trachea, and at least one instructive instance is on record where this mode of exploration was practised with much benefit to the patient. But these are refinements of practice upon which it is unnecessary to dwell. I now merely speak of the ordinary employment of the laryngoscope, and I wish to remind you, that having explored the entrance to the larynx and the vocal cords, you should examine, as far as may be, the trachea. It is generally easy to recognise several rings of this tube, and you may, when the larynx is capacious and the patient steady, penetrate to the very bifurcation of the trachea. The only morbid condition that I have definitely recognised in the trachea, have been scattered ulcers; but it is manifest that, especially in those cases in which foreign bodies have slipped through the glottis, the discovery of their exact site by the laryngoscope may, as it already has been, prove of great practical value to the surgeon.

There is no better mode of initiating yourself into the practice of laryngoscopy than to examine your own larynges, or those of your fellow-students. By this kind of exercitation you will familiarize yourself with the use of the instrument no less than with the healthy condition of the parts. M. Garcia's interesting observations were entirely the result of examination made upon his own vocal cords, and as there is room for a further cultivation of this field of physiology, each of you may be enabled to advance science in this direction. But apart from this, the mere dexterity of manipulation will be increased by the practice recommended, and if you know from experience in your own persons how to behave, you will more readily advise your patients what to do, and sympathize with their difficulties. Not every one, however, is a suitable subject for autolaryngoscopy: the narrowness of the introitus laryngis and the prone condition of the epiglottis, no less than an unusual irritability of the parts, frequently render the process extremely difficult. Various methods have been suggested for the removal of undue irritability, such as the inhalation of small quantities of chloroform, the application of bromide of potassium, or the use of astringent gargles. Every now and then, even after you have acquired sufficient dexterity in the use of the instrument, you will meet with cases which present insuperable difficulties. You are then no worse off than your predecessors were without the laryngoscope. You will have to fall back upon those other symptoms which your knowledge of physiology and pathology will teach you to appreciate, and which this instrument is not intended to supersede.—*Lancet, April* 8, 1865, *p.* 360.

DISEASES OF THE ORGANS OF DIGESTION.

22.—PAIN IN THE STOMACH FOLLOWING INGESTION OF THE FOOD, SUCCESSFULLY TREATED BY MANGANESE.

By Dr. Arthur Leared.

Dr. Arthur Leared observes that pain caused by food is a very common functional affection of the stomach, and it affects women more frequently than men. Its ordinary position is at a spot just below the ensiform cartilage, but it may be localized at other points over the region of the stomach. At times it is diffused over a considerable space. When it has continued some time, and especially if its seat is the spot first mentioned, the affected part feels sore on pressure, and this soreness may remain after cessation of pain. In many cases pain extends a good way upwards beneath the sternum, more rarely it is diffused downwards towards the umbilicus, or even below it. It frequently extends towards the right in the track of the duodenum, and is sometimes experienced in the cardiac region itself. In many cases pain shoots from the part locally affected as from a focus, in various directions through the thorax, frequently to the upper part of the back.

The perception of pain by different individuals is variable, which partly accounts for the varying descriptions of the present affection. By some patients it is described as a dull continuous pain, by others as of a tearing, gnawing, or scraping nature, or like that which might be caused by a tight ligature.

The pain usually comes on from a quarter of an hour to an hour after a meal, but in severe cases is induced by taking even a few morsels of food. Its duration is uncertain, but it usually lasts some hours. There appears to be always a short interval between the time of swallowing the food and the occurrence of pain. A valuable means of diagnosis between it and the pain of ulcer of the stomach, in which pain generally happens immediately on swallowing solid food, is thus afforded. But the intensity of pain affords no measure of the gravity of the disease. The pain of cancer or of other organic diseases of the stomach may be less severe than that which is purely of a functional nature. Flatulence and a sense of distension are experienced in some cases, while in others they are entirely absent. Constipation is not a prominent feature of the disease.

The pain and tenderness are seldom connected with gastritis, as their transient nature, as well as the accompanying symptoms, sufficiently prove. They are simply an expression of exalted sensibility of the mucous membrane of the stomach, which becomes intolerant of the natural contact with the food, or else of the gastric juice itself. The facts that the pain does not come on until the food has been some time in the stomach, and that albuminous food —that which requires gastric juice for its reduction—causes suffering, while starchy aliments cause it in a less degree, or not at all, support the idea that the gastric juice is its source. I have been led by several circumstances to the belief, that the epithelial coating of the gastric mucous membrane is imperfect; that it is either shed too rapidly, or, owing to its imperfect growth, is inadequate for the protection of the delicate surface which it covers. Thus, the state of the tongue whose surface is continuous with that of the stomach, is generally very characteristic of the disease. Owing to a denudation of its epithelial covering, its extremity is very red, and its irritable-looking papillæ stand prominently out. The effect of treatment also proves that inflammation is not concerned in the disease.

I have thought it necessary to describe in some detail the nature of the disease before bringing forward a new remedy for it, because there are other painful conditions of the stomach which are not benefited by the same means. Its successful use will, therefore, greatly depend on accuracy of diagnosis. The gastric pain caused by gout, generally connected with an excess of acid, is not removed by the same treatment; the pain which attends organic disease of the stomach is little influenced by it; and the same may be said of the neuralgic pain *which is especially* apt to occur when the stomach is empty.

In the treatment of the disorder before us, direct sedatives seldom give more than temporary relief. Opium has the great disadvantage of inducing constipation. Prussic acid, with or without alkalies, so useful in some kinds of stomach pain, is generally futile. But the nitrate of bismuth has long been in deserved repute; more recently it has been almost superseded by its carbonate. These preparations are also open to the objection that they induce constipation. Both these substances are but sparingly soluble in the fluids of the stomach, and it occurred to me that their beneficial action may be exerted in a mechanical manner. It seemed possible that the diffusion of an inert powder over the walls of the empty stomach might, as it were, blunt the over-sensitiveness of its mucous membrane. With this view I tried, in several cases, the effects of silex prepared by precipitation from its solution, a perfectly inert and insoluble powder. The results were most encouraging. Passing then to substances which combine mechanical with a special action, I tried in many cases the saccharated carbonate of iron with little success. A fair trial of the magnetic oxide of iron yielded no better results. All the substances mentioned were given in doses of from x. to xx. grains. In pursuance of the same inquiries, the black oxide of manganese, freed from impurities, at length came under trial. Here the results have been unexpectedly satisfactory. After having used it in several hundred cases, both in hospital and private practice, and after an experience of some years, I do not hesitate to pronounce it a most valuable addition to our stomach remedies. It is certainly more efficacious in allaying the hyperæsthetic state of the mucous membrane than bismuth. This has been proved by alternately exhibiting each remedy for a week at a time in severe cases, and carefully noting the results. But, independently of this, manganese has one cardinal advantage —it does not constipate. Assuming them equal in other respects, this alone is enough to stamp its superiority over bismuth, in the use of which we are constantly obliged to tease the gastro-intestinal surface with irritating purgatives. Another advantage not to be overlooked, especially in hospital practice, is this—the purified oxide of manganese can be procured at one-sixth the price of the preparations of bismuth. I possess notes of a great number of cases treated by manganese which incontestably prove its efficacy. Space will not at present allow of these details, but the result of its trial in forty hospital out-patients, men and women, briefly stated, must suffice. The duration of the disease ranged from three weeks (the shortest) to several months, and even years. The dose of the manganese was, in almost every instance, ten grains taken three times a day before meals. Alterations in diet could have had little influence in these cases.

After having taken manganese one week,

Pain was quite removed	in	12	
Very much relieved	in	15	28
Relieved	in	10	
Unrelieved	in	3	
		40	

After having taken manganese two weeks,

Pain was quite removed	in	4	
Very much relieved	in	15	
Not so well as first week	in	1	
Patients did not attend		8	28

Some of these patients continued taking the medicine for several weeks, and although a few who ceased to attend may possibly have remained unrelieved, no instance of failure came to my knowledge.

I was particular in ascertaining the effect on the bowels, and in four cases it is noted that the medicine was thought by the patients to constipate, in three of these only slightly. More extended observations, however, convince me that it has no constipating effect. In one case, on the other hand, it was *stated to* have purged.

Manganese is also highly useful in pyrosis, generally removing first the *watery discharge*, and afterwards the pain in a short time. In certain irri-

table states of the stomach it is also of much service. In one remarkable case, in which a woman had been affected with vomiting for many months, after the failure of bismuth, manganese was very successful in quieting the stomach.

The purified oxide of manganese may be given in doses of from five grains to half a drachm, according to the severity of the case. I tried the carbonate of manganese in a few cases with good results as regards pain, but in doses of ten grains it is apt to induce nausea, or even vomiting. The sulphate given in solution also allays pain, but I found that even five grains is still more liable to disagree with the stomach. The ordinary black oxide of manganese is very impure, and therefore unfit for use. The purified manganese may be obtained from Messrs. Garden and Robbins, 372 Oxford Street, W. I shall feel indebted to any gentleman who may make a trial of the remedy, for a report of the results.—*Dublin Medical Press.—Glasgow Medical Journal, Jan.* 1865, *p.* 488.

23.—A CASE OF ILEUS ACCOMPANIED BY FÆCAL VOMITING SUCCESSFULLY TREATED BY GALVANISM DIRECTLY APPLIED TO THE MUCOUS MEMBRANE OF THE INTESTINE.

(Under the care of Dr. Stokes, Meath Hospital.) Reported by Dr. J. M. Finny.

[The patient was a man about fifty years of age. He had suffered from constipation, and took a dose of Epsom salts for its relief. The medicine acted next morning, but not so freely as usual, and he experienced pain in the abdomen about half an hour after its operation. This pain rapidly became very severe, and after a time vomiting and hiccup set in. The bowels were unacted on, the belly was swollen, hard, and tense, and injections failed to afford relief. On July 6th, ten days after the commencement of illness, the report is as follows:]

The patient is very restless, throwing his arms about, and frequently changing his position. He is covered by a clammy sweat; his face is drawn, and expressive of great anxiety; eyes sunken and heavy.

As all the treatment hitherto adopted has entirely failed in producing any decided and beneficial effect in evacuating the intestines, and as fæcal matters have been ejected by the mouth almost continuously for more than three days, ever since the night of the 2nd, with the intermission of about fifteen hours on the 4th; it is evident in such a case that unless some more speedy and efficacious treatment be employed, the patient cannot be expected to exist much longer.

Under these most serious circumstances the application of the galvanic stimulus to the mucous surface of the intestine, was proposed by Dr. Stokes as a *dernier ressort*. The manner in which it was applied is as follows:—

The patient was directed to place himself on his hands and knees, and while in this position, with his buttocks well raised and his head low, I passed up the long tube with the intention of administering a large enema of ordinary house medicine, previous to the application of galvanism. The tube passed up readily as far as seven or eight inches, when it met some obstacle in its progress. On its withdrawal some feculent matter was observed at its end. It was again re-introduced, and by steady pressure and frequent injections, it slowly passed through some solid obstacle without at all conveying the idea of elasticity. Once through, it was readily passed up the intestine to its full length, and the remainder of the injection was thrown up. On removal of the syringe there was no escape of flatus or return of the injection. The exhausting syringe was now applied to the external extremity of the tube, but failed in causing any flow through it. Without withdrawing the tube the patient was placed on his side, and one sponge connected with the negative pole of the electro-galvanic battery, being applied to the margin of the anus, the other sponge, in connexion with the positive pole, was applied in a rapid manner over the whole external surface of the abdominal walls. After a few such applications there was a gush of fluid matter through the tube, which was then withdrawn, and the sponge which had hitherto been ap-

plied to the verge of the anus, was, along with a portion of the metallic handle to which it was attached, passed up the rectum two or three inches, while the same proceeding as before was renewed with the other sponge. Each sudden application threw the whole abdominal muscles into the greatest spasm, and was immediately succeeded by a gush of fluid fæces. Three large basinfuls were thus got rid of. The operation which had lasted about ten minutes, and which was attended with the most intense agony, so much so that the patient writhed under it to a painful degree, was now suspended, as great prostration of strength had ensued, the patient's pulse becoming almost imperceptible. He soon, however, rallied on the administration of brandy, and expressed himself greatly relieved. He then fell into a quiet sleep. In the afternoon he had two fluid stools, and eight more during the night, entirely devoid of pain. No hard lumps were noticed to have been passed at any of these stools.

July 7th. The patient is greatly improved in appearance; he has had an easy and painless night's rest, and feels very much better; he has had no return of the hiccup or vomiting, and complains of no pain in the abdomen even when pressed upon; his appetite has already improved, and he wished for a chicken for dinner.

July 9th. Since the 7th he has rapidly recovered, his bowels up to to-day having acted regularly, and of themselves. The patient is, however, weak, and occasionally listless and heavy, as if his nervous system had sustained a severe shock.

Ordered a few compound rhubarb pills; oil to be taken at night.

July 11th. The patient left hospital to-day a little weak, but feeling otherwise perfectly well; his bowels are regular, and his stools natural.

On July 25th he again applied for admission, suffering from symptoms similar to those he presented before, but in a much less degree; his bowels having been confined for a few days, and complaining of some pain in the abdomen. He states that since he left hospital, on 11th, he has had two or three slight returns of abdominal pain, and that on one occasion his left leg swelled to a large size. The symptoms, however, left him after lying in bed for a few days. His present complaints are evidently augmented by fear, as he is very nervous about himself, and dreads a repetition of his late treatment. This, however, was not required, as, after a terebinthinate enema, a course of gentle purgatives, such as castor oil, administered every other morning, was sufficient to keep his bowels regular, and to free him of all pain.

This treatment was continued for about twelve days, and his strength and general appearance being much improved, he left hospital on 18th of August. Since then he has had no return of these attacks, and has been able to resume his work as a labourer.

In this case, so fraught with practical interest, there are some few points which particularly deserve attention.

In the first place, as to the immediate cause of the attack—what was it? In this is involved a question more easily asked than answered; for when no confirmation by dissection is afforded the actual cause must ever be clouded in a certain amount of obscurity, which no fancied diagnosis, lightened by the light of a far-seeing imagination, can ever thoroughly disperse.

The suddenness and severity of the attack following so soon after the dose of Epsom salts having been taken would at first almost point to the possibility of its being a case of poisoning.

Then, again, the fact of the pain being for the most part fixed in one locality, which in the first instance was relieved by the topical abstraction of blood, would be almost suggestive of enteritis; caused, perhaps, by an intro-susception. Both these ideas are tenable for a time, but are not carried out by the after-symptoms and progress of the case, as the pain both shifted its position, and though very intense the day preceding the application of galvanism, it had disappeared in four hours after the operation. To my own mind, taking into consideration the whole details of the case, is most strongly *suggested* the idea that the case was one of ileus, arising from a paralysed *state of the* muscular coat of portion of the great intestine. As all peristaltic

action was absent, the injections driven in by the force of the syringe were readily admitted past the affected portion of intestine; but were not again passed, from the loss of its normal muscularity. I should imagine that the violent peristaltic action produced by a large dose of sulphate magnesia, and the irritation of the mucous coat which it sometimes gives rise to, may have been the primary cause of, or in some way connected with, this loss of vital power in the muscular covering of the colon.

The other point of interest is the happy success with which this new mode of applying galvanism was crowned. It may safely be asserted that to its success the patient owes his life. This, then, is a matter of the deepest importance, as it furnishes the physician with a line of treatment worth trying in those cases where the ordinary treatment, such as was adopted in the early part of this case, has failed, as it often will. The acute suffering which it gives rise to, should, however, be borne in mind, as it is not every case which would be sufficiently strong to endure it without receiving future detriment.

As it has been but once tried in the Meath Hospital, and I belive was never before made use of in the United Kingdom, it would be presumption confidently to promise the same success in all future cases where it may be applied; but from the extreme severity of the symptoms in the case above detailed, and from the speedy and effectual relief it gave, it is not too much to hope that its application in future cases may be followed by equally happy results.—*Dublin Quarterly Journal, Nov.* 1864, *p.* 341.

24.—ON THE SUCCESSFUL TREATMENT FOR COMMON TAPE-WORM.

By Dr. John Brunton, M.A., London.

[After considering the modes of introduction of tape-warm into the system, and the symptoms produced by it, Dr. Brunton relates the following case.]

I was called during the night to see Ed. G., a butcher, who was seized with an epileptic convulsion. I learned from his wife that he had often such convulsions, and that she thought they depended on an injury he had received to his head two years before. I prescribed the following:—

℞. Chloric. eth., ℥ss.: tk. hyoscyam., ℥iii.; spt. mindereri., ℥iii; aq. camph. ad ℥vi. M. ℥j. 4tis horis sum.

Sinapisms to the nape of the neck, and perfect quiet.

He soon recovered, and was at his duties next day. I saw him in the forenoon, and on cross-examination found that he suffered from tape-worm. I then, after the usual preliminary treatment, prescribed ℥ii. of ext. eth. filic. mar. as a dose, to be followed by an ounce of castor oil. Next day thirty-five feet of worm were brought to me; but I still could not find the head, though it was apparent that nearly the whole worm came away, as the narrowest part of the worm produced was about the breadth of three plies of No. 6 cotton thread.

On tonic treatment E. G. remained well for four months.

Patient however, soon presented herself, as bad as before. I now resolved to give her kamela, which I had heard and read of as a first-rate anthelmintic. After preliminary treatment, and a good active purgative, I prescribed—

℞. Kamelæ, ℥iss.; mucilag., ℥i.; syrupi., q.s.; aq., ad ℥iii. M.

Half to be taken at bedtime, and half to be taken at 2 a. m.

The same result followed as with the male fern.

Patient got very well on tonics; but towards the close of 1862 she again presented herself, suffering all the symptoms as before indicated.

Finding that the male fern *alone* failed, that kamela *alone* failed, both in large single, double, and often repeated doses (for I prescribed kamela in repeated doses *to another patient* in the interval), I resolved to try a combi-

nation; I therefore prescribed the following (to be taken after the usual preliminary treatment):—

℞. Ext. eth. filic. maris, 3iss.; pulveris kamelæ, 3ii.; mucilag. et syrup., q.s.; aquæ cinnamom ad ℥iii. M.
Half to be taken at bedtime, and half at 2 a. m.

Next day patient brought the *whole* worm, about twenty-six feet in length.

It was with great satisfaction that I viewed the result of my labours to expel the tænia. On examination, under a powerful glass, I found the head to bear the following characters:—Bulging in a pear-shaped form from the neck, it had four circular equidistant black discs on its extremity, having, in a certain light, a hollow appearance, and in another, a prominent aspect; in the centre a round orifice like a mouth, armed at the edge with a circle of hooks, evidently the prehensile part, by means of which the worm attached itself to the mucous membrane of the small intestine. Running from the edge of the black discs was a series of parallel canals, branching as they came down towards the neck, and connected together by numerous vessels. The canals had the appearance of being parts of the alimentary canal, if the black discs may be assumed to be feeding orifices, which I believe they are. The joints next the neck were very small and narrow—towards the tail, or end, they were large, about half an inch in breadth, and greater in length than breadth; each joint was marked alternately by a prominence on the centre of the edge, and a foramen connected with a duct and ovarium. "The ova are fertilized by a fluid that escapes from a vesicle which leads by a duct to the same opening as that from which the ovum escapes, so each segment is male and female in one." Hence I concluded this to be a Tænia solium.

This patient in February, 1865, is quite well, having had no return of the worm.

I have since treated ten cases with the last prescription, viz., a mixture of kamela and male fern, with complete success, on one trial; no second dose being required.

Two cases I may further relate as showing the results of other treatment.

Case.—E. T., butcher, had been under his master's medical attendant, a homœopath, for nine months, for the cure of tape-worm; came in July, 1863. I prescribed for him male fern and kamela, as indicated above. Next day he brought me four inches of worm, containing the head. Had tonic treatment since, and is now quite well and robust.

H. P., Esq., aged 28, had been at Buenos Ayres for two or three years, where he contracted tape-worm. He was treated there with turpentine, kousso, kamela, and afterwards, in various parts of Britain, with male fern and other drugs, with only palliative results.

When in London in May last year, while walking in the street, he was suddenly seized with severe spasm of the bowels. He was brought to me, and I prescribed the combined mixture with complete results.

In conclusion, I can most heartily recommend the use of kamela and male fern combined, as pretty certain in result. It would be bad logic to expect an universal and sure result from so few cases; but the result of my experience in the above cases, and the successful issue, after so many failures, leads me to conclude that the troublesome and disagreeable tænia can be very surely divorced from a connection that he has no right to have.

I may also remark that none of the patients had difficulty in retaining the medicine, though slight nausea sometimes followed its administration. Nor have any evil results followed from the large doses which were prescribed—purgation was the usual result, followed, sometimes during the act, at others after evacuation of the bowels, by the expulsion of the dead tænia. In none of the cases did the worm come away alive when male fern and kamela were given in combination; but in some of the failures before recorded, parts of the worm came out alive, and lived for some time after.

In none of the cases could more than one worm be detected. There are many cases on record in which two, or more, have been found.

I have prescribed as much ʒ- ℥iii of eth. ext. of male fern at one dose, *without success, further than* [illegible] a term of three or four months.

In no case can we be certain of cure, unless the head of the worm be found. If it cannot (as it may escape notice in the evacuations), and a period of at least six months has elapsed after evacuation of the worm without return of the symptoms, then the cure may be considered complete.—*Glasgow Medical Journal, April*, 1865, *p.* 52.

DISEASES OF THE URINARY ORGANS.

25.—ON THE CONDITION OF THE KIDNEY IN THE GOUTY.

By MM. Charcot et V. Cornil.

Two forms of lesion of the kidney are found in gouty subjects. The first although dependent on gout, does not present any feature distinctive of it, while the other especially belongs to it. In the first category the lesions are simply those of albuminous nephritis; the second comprises deposits of uric acid or of urate of soda in the kidney. These renal deposits present two defined forms accordingly as they are formed of uric acid or urates. In well-marked cases, the urates deposited in the kidney in white lines are as characteristic of the disease as are the concretions of the same kind in the articulations. The authors, whilst they allow the frequency of the occurrence of the small, hard, atrophied kidney, in which the cortical substance is diminished—first described by Todd as "the gouty kidney"—in the gouty, deny that it is peculiar to the disease. They say that the same condition of the organ may be found in other diseases besides gout; and on the other hand they assert, on the evidence of cases observed by Garrod and by themselves, that the kidneys of the gouty may present anatomically all the lesions of Bright's disease (albuminous, desquamative, or parenchymatous nephritis). They give Garrod the credit of having first accurately described the special form of deposits of urate of soda in the kidneys. These appear as linear white deposits, following the direction of the tubules of the pyramids; the deposit shows a crystalline structure consisting of prisms of urate of soda. Their composition is always the same, as shown by the production of murexide on the addition of nitric acid and ammonia, and by crystallization of uric acid on the addition of an acid. The question, whether the deposit of the urate of soda is situated within or outside the uriniferous tubules, is an open one. Garrod's first observation led him to think that the white lines were due to a filling of the uriniferous tubules; and this also was Mr. Ceeley's conclusion, in a case communicated by him. Subsequent observations, however, led Dr. Garrod to modify his opinion. The crystals of urate of soda, commonly larger in the kidney than in the cartilages, seemed frequently to be situated in the cellular tissue rather than in the cavities of the tubuli. In a case observed by the authors, at first sight the crystals seemed placed in the intervals between the tubuli; but by adding acetic acid and dissolving the free crystals, it became clear that a part of the deposit was seated in the interior of the uriniferous tubules. The following are the authors' conclusions:—

1. That in a certain number of the gouty, in consequence of the irritation produced in the kidneys by the passage of a large quantity of urates, albumen appears in the urine, irregularly and in small quantity; and this condition is sometimes accompanied by œdema.

2. That these symptoms correspond with an anatomical alteration of the kidneys, which consists in a chronic form of albuminous nephritis (parenchymatous nephritis), or in a chronic alteration, characterized by atrophy of the parenchyma, with thickening of the fibrous partitions and of the arteral coats (interstitial nephritis, *gouty kidney* of Todd), but that these lesions have nothing which pertains specially to gout.

3. That two forms of deposit in the kidney belong properly to gout—1. Deposits of *uric acid* (*yellow* or *red* granules), the néphrite goutteuse of M.

Royer; 2. Deposits of urates, which are characteristic of gout, and completely identical with those of the articulations.—*Brit. and Foreign Medico-Chirurgical Review, Jan.* 1865, *p.* 257.

26.—ON GOUT.

By Dr. H. Bence Jones, A.M., F.R.S., &c.

The gouty diathesis consists in an excess of urate of soda not only in the serum of the blood, but in the fluid that diffuses from it all the vascular and non-vascular textures of the body. An attack of gout is a chemical process of oxidation set up in the parts where the urates are most able or liable to accumulate. By the oxidising action the urates are wholly or partly changed into urea and carbonates, which can more readily pass from the textures into the blood, and be excreted by the kidneys, skin, and lungs. The oxidation even in the bloodless textures causes increased flow of blood and mechanical pressure in the vessels nearest the inflamed part, and hence pain and redness, and then swelling and œdema, proceed. Though the gouty diathesis is a disease of the textures as well as of the blood, yet in its origin and situation an attack of gout is even more a disease of the tissues than a disease of the blood.

The urate of soda bears the same relation to gout that sugar does to diabetes; and as the want of oxidation of sugar is the cause of the diabetic diathesis, so the want of oxidation of the urates, and their consequent accumulation in the textures and blood, is the cause of the gouty diathesis. Errors excepted, as almost every grain of starch passes through the stage of sugar, so almost every grain of albuminous substance that enters the blood sooner or later, in its way out, passes through the stage of uric acid, and, if thoroughly oxidised, escapes as urea, carbonic acid, and water. The number of substances that are formed between albumen and urea are vastly more than between starch and carbonic acid; but whatever their number, uric acid is the penultimate, and urea the ultimate, product of oxidation. Hence there are at least two ways in which an excess of uric acid may occur in the blood and textures: 1st, from an excess of animal or vegetable albuminous food entering the system,—*i. e.*, from excessive production; and 2nd, from an arrest of oxidation,—*i. e.*, from want of destruction. Of course, the greatest accumulation of uric acid will occur when the albuminous food is excessive, and when at the same time the oxidation is least. Even if no excess of albuminous food is taken, yet if the oxidising action is deficient, uric acid may accumulate in the serum, and it will immediately diffuse even into the bloodless textures. On the other hand, an excess of albuminous food may be taken, and, provided the oxidising action is also in excess, no accumulation of uric acid in the blood or textures may occur. It is therefore evident that there are two modes of preventing the gouty diathesis: 1st, by diminishing the amount of vegetable and animal albuminous food; and 2nd, by promoting oxidation. In other words, the smallest amount of food and the greatest amount of air are the antidotes for the gouty diathesis. If an excess of fresh air is taken whilst a large quantity of food is eaten, these cause no gouty diathesis as long as the antidote (oxygen) destroys the *materies morbi* (urates); but if from any cause the oxidation becomes less, the urates accumulate in the liquor sanguinis, and pass by diffusion into and around the cells of all the vascular and non-vascular structures of the body, and remain unoxidised where there is least oxidation, and even form crystals of urate of soda in consequence of the slow deposition of the urates on the surface and within the structure of the non-vascular cartilages. Where urate of soda accumulates it acts like a foreign body, increasing friction, setting up irritation and inflammation, by which the uric acid is more or less oxidised, and thus being made soluble, it is more or less removed, and when the inflammation *subsides*, effused fibrin almost alone may remain to mark the situation of the *attack*.

If no inflammation comes on, the liquor sanguinis may free itself of urates

by the kidneys, and then the deposited crystals may more or less entirely re-dissolve, and diffuse back into the blood; or, if the serum continues full of urates, the chalk stones may gradually increase in the direction of least resistance, making the joints more and more rigid, and the skin more and more thin, until the pressure breaks the surface, and the chalk stones escape as myriads of microscopic crystals. Thus, the fingers and toes may become supplementary kidneys, to the great relief of the blood and textures, and the gouty diathesis may almost be considered as microscopic gravel in the textures, and an attack of gout as a chemical operation for the removal of the gravel from the part in which it had accumulated.

That the urates diffuse into the synovial fluid and on to the surface of the cartilages, the following case shows:—W. W., aged 48, was admitted into St. George's Hospital on November, 23, 1855. He was a painter and glazier in good circumstances, and had suffered more or less from gout for eighteen years; otherwise he enjoyed good health, and never had colic. The urine contained a small quantity of albumen; the heart's action was feeble; the right hand was red and swollen; his feet were slightly œdematous; chalk stones existed in his ears. The acute attack of gout in a fortnight gradually subsided, and he was about to leave the house when a fresh inflammation of the hands, feet, and knees came on, with great prostration of strength. In a few days he was seized with epileptic fits; the slightest motion immediately brought on an attack; the pupils were contracted, there was excessive tremor of the limbs, and he sank four weeks after his admission.

Both knee-joints were carefully opened. They contained two or three drachms of thick synovia, and in each cavity there was a flake of fibrin about an inch and a-half long, shreddy, with whitish specks visible to the naked eye. On examination under the microscope, small acicular crystals were seen mixed with fibrinous matter, and when a drop of dilute hydrochloric or acetic acid was added, the whole surface of the fibrin in a few minutes became covered with microscopic uric acid crystals. The moist fibrin from both joints showed these crystals of uric acid, and when the fibrin was treated with nitric acid and evaporated, there was distinct evidence of uric acid. On the surface of the cartilage there was only a very small spot free from the deposit of urate of soda. Both great toes at their tarso metatarsal articulations contained the same deposit on the surface.

The spleen was large and soft; the kidneys granular, and the cortex much wasted. There were some small deposits of red sand visible to the naked eye in the mammary processes.

A man, aged forty-one, was admitted into St. George's Hospital with continued fever, and died on August 20, 1849. On examination by Dr. Handfield Jones, gouty deposit was found in both knee-joints: 1st, under the synovial membrane at the margin of the cartilage; 2nd, in the superficial structure, or on the surface of the cartilage, from whence it could not be removed by gentle washing or scraping; 3rd, in the substance of the cartilage at some depth; 4th, in the cancelli of the subjacent bone (the patella).

The gouty matter appeared for the most part as aggregations of amorphous masses, varying in size, and encrusted over frequently with minute crystalline spiculæ.

The cartilage was remarkably thickened, being at least three time its ordinary dimensions. Near its middle it was much more elevated than towards its margin, and very soft and yielding. When a vertical section was made of it, the surface exhibited a marked fibrous structure, quite visible to the naked eye, the fibres being arranged vertically to the surface, and being much more marked in the deeper than in the superficial layers.

The microscope showed that the basis substance was considerably increased, the cells being in much scantier proportion than natural, but not essentially altered. The fibres were not isolated from each other. The fibres or bands were of some considerable width, separated by narrow intervals of healthy structure.—(*Proceedings of Pathological Society*, vol. ii., p. 103.)

On the Means of Detecting Gout.—The chemical test for the gouty diathesis—that is, for excess of urates in the blood and textures—is based on an analysis of the urine and of the serum of the blood.

Even long before the gouty diathesis is formed, the urine will give warning of the future disease. An excess of urea in the urine may be caused either by an excess of exercise or by an excess of vegetable or animal albuminous food. The amount of urea bears a certain, though not direct, proportion to the amount of daily exercise; when the exercise is moderate, an excess of urea in the urine indicates that albuminous food in excess is passing through the system; when the oxidation of the albumen is perfect, urea and carbonic acid are formed; but with less perfect oxidation, urea and oxalic acid result. Hence, oxalate of lime, with excess of urea, constitute the earliest evidence of imperfect oxidation of an excess of albuminous food. When the oxidation is still less, then oxalic acid and urea are not formed from the uric acid, but the urine shows an increase in the quantity of urates.

Simple inspection of the urine is quite worthless as a means of determining the amount of urates in the urine, for the clearest urine may have the greatest amount of urates dissolved in it, and urine that thickens on cooling may contain less uric acid than perfectly clear water. You will see in a paper I published in the Philosophical Transactions, part ii., 1849, p. 249, that urine passed at 5.10 p.m. became thick on standing, and contained 0·52 grain uric acid per 1000 grs. urine; urine passed at 11 p.m. remained clear on standing, and contained 0·87 grain uric acid per 1000 grs. urine. Again (p. 251), urine passed at 7·35 p.m. became thickish on standing, and contained 0·29 grain uric acid per 1000 grs. urine; urine passed at 10·5 p.m. remained clear on standing, and contained 0·33 grain uric acid per 1000 grs. urine. Again, urine passed at 7·55 p.m. became thickish on standing from urates, and contained 0·31 grain uric acid per 1000 grs. urine; urine passed at 10·45 p.m. remained clear on standing, and contained 0·90 grain uric acid per 1000 grs. urine.

When analysis shows that an excess of uric acid is thrown out of the body, this is a proof that the gouty diathesis is ready to form whenever the kidneys cease to remove the excess from the system. The presence of any urates at all in the urine is a sign that oxidation is not so perfect as it might be, and the more urates in the urine the more imperfect the oxidation in the system must be considered; but as long as the products of the imperfect oxidation are thrown out by the kidneys or skin, no gouty diathesis is produced.

Thus, then, first, excess of urea when little exercise is taken; secondly, oxalate of lime with excess of urea; and thirdly, urates in excess, by analysis, constitute the three tests of the different degrees of suboxidation which precede the establishment of the gouty diathesis.

When uric acid in excess is in the system, analysis of the urine may not indicate any excess there—indeed, the uric acid may be absent from the urine and be retained in the blood—as, for example, in Bright's disease. Then the chemical examination of the serum may give positive proof of the existence of this imperfect oxidation, and lead to the certainty of the textures containing the same urates ready to set up inflammation of a gouty nature at any moment; usually symptoms of gout begin to make themselves felt by pain, followed by heat and redness, and then the chemical methods for detecting gout are usually quite unnecessary.—*Medical Times and Gazette, April* 22, 1865, *p.* 407.

27.—ON ACIDITY, OR THE ACID DIATHESIS.

By Dr. H. Bence Jones, A.M., F.R.S., &c.

Acid and sugar, chemically, are as closely related as parent and child; and hence a great similarity exists between the symptoms that arise from an excess of sugar and an excess of acid. The nature of the acid diseases, the symptoms it produces the means of detecting it, the consequences it occasions, and the treatment requisite to cure it, are all essentially chemical questions. A most important difference, however, exists between the sweet and *sour disease*, namely this: the sweet disease is mostly free from secondary *mechanical* diseases; whilst acidity continually gives rise to mechanical

complaints. Two of these secondary mechanical, or mechanico-chemic l, complaints—gravel and gout—are the frequent lot of those who, living more by nerve than by muscle work, disarrange the healthy balance between the carbon and oxygen which go in and come out of the body.

Acid is essential for the performance of the functions of the body. Without acid the albuminous food could not be dissolved; the excretions from the blood could not be safely removed. If no acid was produced in the body, there would be no digestion and no depuration of the blood, and probably bony matter would accumulate everywhere. The vinegar cruet is only less essential to man than the salt cellar, because the animal machine is itself an acid factory. Very often more acid is made than is useful or beneficial; then chemical errors occur, and these set up mechanical diseases as surely as the effect of the cannon-ball depends on the combustion of the gunpowder. The mechanical results are vastly more serious than the original chemical action; but they are directly proportioned the one to the other. As the motion of the mass depends entirely on the motion of the molecules, so the mechanical disease arises solely from the chemical disorder.

It would be useless, even if it were possible, to give a list of the mineral, vegetable, and animal acids, between carbonic acid (CO_2) and uric acid ($C_{10}H_4N_4O_6$), which may be formed from changes in the textures of the body, or from the food. It is far more important to consider how an excess of acid may arise or accumulate in the system. This may occur in two ways—1st, by over-production; and, 2nd, by insufficient evacuation. When these causes co-operate the excess of acidity reaches its maximum.

First, on the production of acid in the body from the textures and food.

The living human body is, chemically, a varying mass of more or less complex carbonaceous matter, each portion being exposed to the action of oxygen. The chemical products everywhere vary according to the substances acted upon, and according to the time during which the oxidation is continued.

Outside and inside the capillaries, in every part of the body, in cells and out of cells, chemical changes are taking place. The oxygen of the air, the organic fluid, the catalytic membrane, act and re-act each on the other; and the products of the chemical action are reabsorbed into the blood, or are thrown upon the inner or outer surface, to be taken up by the alimentary mucous membrane, or to be removed by the lungs, the skin, or the kidneys.

At present, the chemical changes which take place in the textures are very imperfectly known, and the variations of action which occur in health and disease are only beginning to be observed. The healthy muscle, during the circulation of the blood through it, has no acid reaction; but for a time after death chemical actions continue, and sufficient acid is produced, and remains unneutralized by fresh alkaline liquor sanguinis so that markedly acid reaction can be obtained. Even during life, in tetanus, sufficient acid may be formed to give an acid reaction, and after violent epileptic convulsions excess of acidity may be found in the urine. Even the nerves and the electric organs of fishes show signs of acidity when the alkaline blood ceases to flow.

The production of some healthy and less complex secretion may enable us, from its chemistry, to obtain greater clearness on the production of acid in the body. The gastric juice furnishes by far the most striking example of the formation of acid in the body. Until the chemical acid factory in the stomach is fully understood, the chemical changes which are going on outside the capillary vessels throughout the body cannot be realised.

There can be no doubt that the formation of gastric juice occurs outside the capillaries of the stomach. Its rate of secretion depends on the rate of passage of the blood through the capillaries; and this is directly under the control of nerves that open or shut the blood vessels. In the fluid poured out of the capillaries chemical changes occur, acids are formed—perhaps Dr. Marcet's colloid acid; or by the oxidation of sugar among other acids, oxalic acid may be produced, *and this may* decompose the chloride of sodium, and the *most diffusible hydrochloric* acid may be poured out on the free surface

of the tubes; whilst the alkali passes with the organic acid, or its products of combustion, back into the blood.

In the Medico-Chirurgical Transactions, vol. 35, p. 41, you will find a paper "On the Alkalescence of the Urine from Fixed Alkali in Some Cases of Diseased Stomach." I shall here give you only one example in order to fix this inverse relationship of the acidity of the stomach and urine firmly in your minds. A man, aged forty-six, was admitted into St. George's Hospital with sarcinæ ventriculi. The eruption from the stomach occurred at irregular periods, once and sometimes twice daily; the quantity vomited each time was from one to four pints of intensely acid fluid. The urine was usually passed about six times in twenty-four hours, when the vomiting was most. The urine was acid twice, or at most thrice, daily before the sickness occurred; and for the rest of the day after the vomiting the urine was strong alkali. The patient remained in the house for a month, and during the last week he had only one attack of sickness; then the urine became markedly acid each time it was passed, and remained so throughout all the day.

After digestion is finished, when the food is dissolved in the stomach, the intensely acid fluid is in part returned into the blood. The serum might almost cease to be alkaline but for the neutralising action of the bile; thus the degree of alkalescence of the blood must be continually changing, and the fluid effused from the capillaries, and out of which the secretions are formed, must become less alkaline after the food is absorbed than it is whilst digestion is going on in the stomach.

An oxidation similar to that which occurs in and around the stomach cells must occur to a greater or less degree everywhere throughout the body; carbonic acid or other acids, arising from a less perfect combustion, must be formed outside the capillaries of the body. In the muscles and nerves, and in every other texture, power must be set free by the mutual action of the carbon and the oxygen, and the acid products would quickly neutralise the alkalescence of the blood if the acids did not pass off by the lungs, the skin, and the kidneys.

On the Removal of Acid from the Body.—Without cessation night and day free acid continues to pass into the air vesicles of the lungs from the blood, thus rendering it more alkaline than it would otherwise be, and counter-balancing not only the acid produced by oxidation in the different textures, but also the residue of the acid from the gastric juice and food which is not neutralised by the bile.

If the combustion in the body were complete, the whole of the products, except the ashes, might escape by the lungs; free respiration might carry off readily all the acid produced in the system, but imperfect oxidations do occur, and then other acids besides carbonic acid are formed and give rise to neutral or to acid salts which are not volatile, and these being unable to pass off by the lungs are thrown out by the skin and the kidneys.

There can be no doubt whatever that acid is produced outside the capillaries of the skin and kidneys in the same way, although to a much less degree than in the stomach cells. The chemical composition of the substances that pass into the urine and the sweat depends not only on the amount of action of the oxygen, but also on the chemical composition of the fluid poured out of the capillaries to form the secretion.

If the serum of the blood is at its highest point of alkalescence, the acids formed by oxidation during the secretion of urine may be too feeble to neutralise the alkali poured out, and alkalescent urine may result. If, on the contrary, feebly alkaline or acid salts are effused from the capillaries, a very small amount of additional acid produced by oxidation in the secreting structure of the kidneys will set free some of the acid from the acid salts, and thus free uric acid may appear in the urine.

The same formation of acid occurs in the secreting structure of the skin, and although it is not possible to demonstrate the daily variations of the acidity of the sweat, yet the extraordinary increase in its acidity with strong exercise, or in some diseases, as in rheumatic fever, shows that it is subject *to great variations.*

The variations in the alkalescence of the saliva were long since shown by Dr. Wright to be influenced by the digestion.—(*Lancet*, 1841, p. 787.)

The smallest amount of acid is thrown out of the system when the respiration is impeded, whether from insufficient exercise or from imperfect supply of air; then carbonic acid accumulates, and the less perfect products of combustion are increased in the serum. If these can escape by the kidneys and skin the system is relieved, and no secondary symptoms are produced; but if these vents are closed, and if by acid diet and an irritable stomach an excess of acid is poured into the blood, then an outburst must take place, and chemical and mechanical diseases—that is, unusual molecular or massive motions—occur until the healthy condition of the blood is again reached.

Excess of acidity or the acid disease may shortly be defined to be a want of free oxidation and an insufficient escape of the products of imperfect combustion.

On the Means of Detecting Acidity.—It is not easy for the Physician to determine accurately the escape of an excess of acid by the lungs, and still less so by the skin; but by both chemical and microscopical evidence he can follow the minutest changes which occur in the acidity of the urine, and thus he may know something not only of the state of the blood from which that urine is secreted, but he may obtain some information regarding the acidity of the processes of oxidation and nutrition outside the capillaries. Different litmus papers vary so much in composition and delicacy of reaction that no satisfactory conclusion can be reached by this test alone. A standard solution of soda furnishes the surest chemical proof of the amount of acid in a given quantity of urine, and thus variations may be proved and comparisons made with the utmost accuracy. But a readier method of coming to a practical conclusion exists in observing by the microscope the state in which the uric acid exists in the urine. The delicacy of this test for free acid far surpasses any other; indeed, litmus paper and standard solution cannot enable you to distinguish between acid salt and free acid; but the microscope by showing more or less free uric acid crystals tells at once the greater or smaller amount of free acid in the urine. As long as free uric acid crystals do not exist in the urine no very great amount of acidity is present. When acid urates alone are passed there may be very great effect on the litmus; but as long as the microscope and chemistry show that free uric acid is absent, the chief secondary mechanical disease, uric acid stone, is not likely to be set up.

Hence the determination of the presence or absence of free uric acid in the urine is a question of great importance.

A small bottle or test tube should be filled with fresh urine and examined by transmitted light. Uric acid crystals can be easily seen. If no red sand is present in twelve hours another examination should be made. Very frequently a deposit occurs as the urine cools. then the microscope must be used to see whether crystals of uric acid are mixed with the granular deposit. The amount of free acid may be known by the rapidity with which the uric acid crystals appear, by the size of the crystals, and by the more or less perfect decomposition of the granular urates.

When the free acid is in very great excess the uric acid may crystallise in the renal tubes or in the pelvis of the kidney or in the bladder. When very little free acid and very little uric acid has been present, I have seen the crystals of uric acid appear on the eleventh day after the water was passed.

Professor Scherer long since stated that an acid fermentation took place in the urine soon after it was passed; but I have been unable to satisfy myself that any increase of acidity occurs. His experiments were not made on healthy urine. When he made them, the state in which the uric acid existed in the urine was not known. The slow deposition of uric acid from a feebly acid solution of urates was not taken into account.

Very frequently, when examining the same specimen of urine day after day for uric acid crystals, I found that no free acid was deposited; until at last I was led to make exacter experiments to determine whether the free acid in the urine was increased after the water was made. But first a series of twenty-seven experiments were made on healthy urine to determine how

often, and how soon on standing, uric acid crystallised out. Four times only were these crystals found by the microscope twenty-four hours after the urine was made; and five times the acid crystallised out in two days. Eighteen times no uric acid was found free. In other words, eighteen times out of twenty-seven the urine under the microscope did not show this evidence of an increase of acidity. In the other nine times the uric acid might possibly have been set free by an acid fermentation, although another explanation was probable, namely, that the crystals only formed slowly because no great quantity of urates were present, and because the liberating acid was very weak. For determining between these views, a standard alkaline solution of caustic soda was prepared, and the acidity of the urine was measured as soon as possible after the urine was made, and then from time to time afterwards the acidity of the same urine was again noted.

When it was proved that sugar existed in small quantities in healthy urine, I thought that some evidence for or against an acid fermentation might be got by determining the amount of sugar in the urine, first when fresh, and afterwards when it had stood some days.

Three litres of healthy urine were divided into two equal parts. The one half was examined for sugar immediately; and the amount of sugar was 2·4 grains. The other half was left for twenty-five days, until it became feebly alkaline, and then the sugar was determined, and the amount was 1·9 grain. A loss within the limits of error in the method of analysis.

These and many similar experiments are opposed to any acid fermentation; and when the formation of uric acid crystals in the urine occurs, it may be taken as a proof that some free acid is secreted by the kidneys, and that this, sooner or later, has set free more or less uric acid from the acid urates in the urine.

The longest time in which I have seen uric acid crystallise out has been eleven days, but usually the first, second, or third day after the water is made the crystals form, if they form at all; and the formation of these crystals is the only certain and delicate test of the presence of free acid in the urine. I had hoped that the form of the crystals, varying as it does so greatly, might enable me to determine the nature of the liberating acid; but after a long series of experiments I can as yet come to no satisfactory conclusion in this respect.

Symptoms of Excess of Acidity.—Only when the skin or mucous membrane have become irritable by the continued action of the acid, do symptoms of acidity force themselves on the notice and require Medical aid. The amount of uneasiness or pain will vary with the sensitiveness of the membrane and with the degree of acidity. Slight degrees of acidity hardly irritate the skin; but itching, nettle-rash, eczema, and herpes are the outbursts of an over-acid state.

The mucous membrane of the stomach is less sensitive than that of the pharynx, œsophagus, and bowels. When 162 grs. or even 81 grs. of dry tartaric acid in four or six ounces of water are taken, the acid hurts the mouth and the upper part of the food-tube, and then ceases to be felt for three hours or more, when it causes violent griping pain for an hour or two. When the mucous membrane has become irritable the stomach becomes much more sensitive, and acidity causes pain, cramp of the muscular coat, and vomiting; the bowels feel more and move more strongly, and violent pain, colic, tormina, and tenesmus come on.

If the sensitiveness of the mucous membrane of the urinary organs is increased, the nerves and muscles become irritable; urgency and frequency of passing water gradually increase into constant pain and violent spasm of the bladder. In the urethra a scalding pain is felt, and constriction of the mucous membrane causes a complete stricture whilst the spasm lasts.

When the acidity is excessive, the skin, stomach, and urinary organs may all be simultaneously irritated. In other words, the amount of acid formed may be so great that it is thrown out everywhere. Most frequently the acidity only shows itself in the stomach, and in the urine; and the symptoms *produced* in the stomach and urinary organs are so closely dependent that

the same treatment that does good to the one mucous membrane will be found to do good at the same time to the other.

I might here, if time permitted, give you innumerable cases of acidity illustrating the great persistence and intractability of the deposit of uric acid, and its great liability to return as soon as the alkalies are omitted, unless the strictest diet is observed. The resemblance of these cases to cases of diabetes is very striking; and, moreover, in slight diabetes or intermittent diabetes the production of excess of acid is very frequently to be met with.

I must, however, give you two cases illustrating the rarer consequences of this excess of acidity. A gentleman, aged forty, consulted me for constant deposit of urates and uric acid in the urine. He had been subject to these deposits for years; for the last few months occasionally, from one to five hours after a late dinner, he was attacked by pain in the stomach, which in a short time became intermittingly spasmodic. The intensest pain was reached in half a minute, it then relaxed, and returned as badly as before in two minutes. His suffering lasted about an hour, when the pain gradually abated, leaving a tenderness on pressure and an irritability after food for two or three days. The urine passed after the attack gave on standing uric acid crystals. I advised him when another attack occurred to fill the stomach with hot water and then to reject it. This he did, and a considerable quantity of nearly clear acid fluid was thrown up; on repeating the emetic later during the same attack, a much more intensely acid fluid was obtained, showing that probably an hour-glass contraction of the stomach had existed. By careful diet and anti-acid medicines the attacks entirely ceased. In another patient, six or eight hours after food, attacks of violent cramp used to occur in the rectum, and last from half-an-hour to an hour. Relief at the time was obtained by pressure on a hard seat, and by careful diet and alkalies the attacks entirely ceased.

Before speaking of the treatment, a few words must be said on the

Consequences of Excessive Acidity.—In all I have hitherto said of diabetes and acidity a parallel might be drawn between the two diseases, with this difference—that the bland sugar acts as a diuretic, whilst the irritating acid affects the nerves and muscles, causing pain and spasm; but in the consequences of the two diseases an entire divergence must be noted. Diabetes causes a painless weakness and atrophy. Acidity is the cause of from 60 to 50 per cent. of all the cases of stone that occur in England. Dr. Prout says—"If a uric acid nucleus had not been formed and detained in the bladder, two persons, at least, out of three who suffer from calculus would never have been troubled with that affection."—(p. 583 "Stomach and Renal Disease.")

In the 26th vol. of the Medico-Chirurgical *Transactions* for 1843, I have given the analysis of the calculi in St. George's Hospital. In 233 calculi, 89 consisted of uric acid alone; that is, 38 per cent. of these stones were caused *solely* by acidity. Whilst 135 consisted of uric acid; uric acid and oxalate of lime together; and of these substances mixed with urates; that is, 58 per cent. were wholly or *partially* caused by acidity.

Gravel is still more frequently caused by over acidity. Seventy-five per cent. of all renal calculi are uric acid, caused by acidity alone. Hence three out of four renal attacks giving rise to almost as violent mechanical suffering as the human body can endure, are caused by a slight chemical disorder which often continues unobserved until the mechanical disease is set up.

Treatment of Acidity.—The relation that exists between diabetes and over acidity is even more evident in the treatment than in the causes and symptoms of these diseases. The best diet for diabetes is the best diet for over acidity. The same medicines are most useful in both complaints. The same immediate effect can be produced by treatment, and there is the same disposition to relapse after the symptoms have entirely ceased when wrong food is taken.

The treatment of acidity must be considered under two heads: First, the diet; and secondly, the medicine.

With regard to the diet. All that has been said against saccharine and farinaceous diet in *diabetes might* be repeated here, inasmuch as these sub-

stances give rise in the system to various acids in their progress to carbonic acid and water. Hence, as regards the action of sugar, dextrine, and starch, I may refer to the lecture on diabetes; but as the acid diathesis arises from slighter check to the chemical actions of the body than the saccharine diathesis, therefore a less strict diet will give a greater improvement in the disease than in diabetes. Moreover, medicines effect a more decided good over acidity than in the saccharine complaint, and on this account, also, greater liberty may be given in diet provided the acidity from the food is neutralised by alkaline medicine.

Another difference must be made in the diet of those who suffer from these complaints. In diabetes, fatty substances, cream, butter, fat, and oil check the wasting of the body; but in excess of acidity these articles of food often give rise to great uneasiness in the stomach; they are very liable to cause a peculiar feeling of irritation, which is known as heartburn. Thus, over acidity and heartburn can be removed and kept away by leaving off sugar and butter. In severer cases, potatoes, rice, arrowroot, and a considerable portion of the daily bread must be given up. Fruits of all kinds are mostly highly flavoured ether mixtures of starch, sugar, and acid. Wines and beer chemically differ from fruit only by containing alcohol instead of some starch and sugar. Alcohol itself, very probably in the stomach as well as in the air, gives rise to acid, and the weak spirit and sugar of half-fermented beer and effervescing wine more easily undergoes this acid fermentation than fully fermented wine and pure spirit. Hence, in extreme cases of acidity small quantities of lean animal food, including fish, flesh, fowl, game, and eggs, and brandy-and-water, constitute the best anti-acid diet. All fluids containing sugar should be avoided; and alkaline gaseous mineral water, natural or artificial, will neutralise a portion of the acidity which the imperfect combustion continually produces.

Exactly the opposite diet was advised at the dawn of animal chemistry, on the ground that the ash of vegetables containing alkalies, these would neutralise all the acid that should arise from the starch and sugar. But take, for example, potatoes: the amount of acid-producing substances is from 20 to 30 per cent.; the amount of alkali at most will not reach to 1 per cent. Or take bread: the ash of bread would neutralise at most 1 per cent. of acid. The amount of acid-producing substances in bread is 46 per cent. Any other fruit or vegetable would give equally good evidence against a vegetable diet in over-acidity.

The object of medicine in this complaint may be divided into—first, the removing, and second, the neutralising of acid.

The removal of acid is effected most immediately and directly by emetics, emptying the stomach at the time when the greatest quantity of acid is present, that is, during the digestion of a full meal. In the treatment of the acidity of children, this is the shortest and readiest way. Hot water, mustard and water, ipecacuan will remove more acid than would be neutralised by the usual amount of alkali that could be taken daily. The repetition of the emetic should depend on the re-appearance of uric acid crystals in the urine.

The free action of the skin by baths, especially Turkish baths and vapour baths, or by strong exercise, removes acid quite as effectually, although more slowly, than by taking away the gastric juice. Moreover, an increased natural action disarranges the system less than a stomach catastrophe, which, though scarcely felt by the young, is a shock unfitted for advancing years. Of all the methods of exciting perspiration strong exercise is the best, because this generally implies a freer respiration, and this means the greatest possible conversion of acids into carbonic acid and water. The more bracing the air is, and the more free from mechanical and chemical impurities in the form of dust, smoke of all kinds, fogs, and noxious gases, the more vivid is the combustion, and the greater is the amount of acid that is removed from the system through the lungs.

There remains to be considered the removal of acid by the kidneys. By increasing the action of the kidneys, an increased quantity of acids of many *kinds, from* carbonic acid to uric acid, is thrown out of the system. *Diuretics thus* become veritable anti-acids. Hence, pure water and nitre, and

mineral waters, by exciting diuresis, relieve the blood and textures of acid, but whilst promoting the action of the kidneys the acid may be neutralised, and this is the second object to be obtained by medicine.

In neutralising the acidity of the urine, by far the most effectual medicines are vegetable salines, containing fixed alkalies or earths.

Although five drachms of tartrate of ammonia be taken in the day urine is not made alkaline, nor even neutral; whilst after the same quantity of tartrate of potass the urine is alkaline for the greater part of the day, and at one period of the day reaches as low as thirty-two degrees; in other words, a thousand grains of urine contained three grains of carbonate of soda. Moreover, eighty grains of carbonate of ammonia daily in three different experiments did not make the urine neutral, though it acts more strongly than most alkalies in neutralising the acidity of the gastric juice in the stomach. Three drachms and a-half of liquor potassæ, containing 6¼ per cent. of potass, taken in one day, though it lessened the acidity, did not cause alkalescence; still, no doubt, the action of this and other alkaline remedies, when long continued, produces very decided effects in neutralising acidity, the comparative worth of equal quantities of different alkalies may be determined by their combining proportions.

Thus 10 parts lithia	=	12	parts	ammonia.
,, ,,	=	14	,,	magnesia.
,, ,,	=	19	,,	lime.
,, ,,	=	21	,,	soda.
,, ,,	=	33	,,	potass.
,, ,,	=	54	,,	bismuth.
,, ,,	=	25	,,	carb. of lithia.
,, ,,	=	27	,,	carb. of ammonia.
,, ,,	=	29	,,	carb. of magnesia.
,, ,,	=	35	,,	carb. of lime.
,, ,,	=	37	,,	carb. of soda.
,, ,,	=	48	,,	carb. of potass.
,, ,,	=	70	,,	carb. of bismuth.

In other words, 14 grains of magnesia will neutralise as much acid as twenty-five grains of carbonate of lithia, or forty-eight grains of carbonate of potass, or seventy grains of carbonate of bismuth. One grain of carbonate of lithia is nearly equal to a grain and a half of carbonate of soda or two grains of carbonate of potass.

In ordering these different alkaline substances, their constipating or their aperient action must be considered; and beyond all this, the peculiarities of different individuals must be taken, provided it does not cause pain or blood. The amount can be learnt by experience. That carbonate of soda to the amount of two ounces and a half daily may be taken for months with impunity may be seen in a case in Med.-Chirurgical Transactions, vol. v., p. 80. The blood had a strong buffy coat. The urine was alkaline. In speaking of the treatment of diabetes, I have dwelt on the alkaline action of different mineral waters. I must refer you to what I have already said. There is in the treatment of acidity an advantage in giving mineral waters which does not exist in the treatment of diabetes. In acidity pure water may be regarded as an anti-acid. It lessens the symptoms, and prevents the consequences of the acidity; where strong acid irritates, weak acid has no effect. Strong acid quickly sets free uric acid; whilst weak acid has to make up by time what it wants in force, so that simple dilution, or the amount of water that exists in the mineral water, becomes an important means of relieving acidity.

When the acidity is excessive, all these different methods of lessening acidity must be used at the same time. To give acid in the food and to neutralise it by medicine is child's play, unless the amount of alkali taken exceeds the amount of acid eaten. The greatest possible effect can be obtained by stopping acid from going in, by removing acid by perspiration, and by the stomach, skin and kidneys, and by neutralising the acid by lithia, ammonia, magnesia, or soda.—*Med. Times and Gazette*, Feb. 18, and 25, 1865, *pp. 165, 191.*

28.—ON THE TREATMENT OF THE OXALIC ACID DIATHESIS

By Dr. H. Bence Jones, A.M., F.R.S., &c.

[The cure of the oxalic acid diathesis is to be obtained by promoting oxidation in the body. Were it not for the oxalic acid combining with lime to form oxalic acid diathesis would be of very little consequence, being as it is usually a slight aberration from health. Hence it is of importance to prevent as much as possible the entrance of lime into the system.]

It is quite impossible to stop all lime from going in or coming out of the body; and, therefore, by the anti-lime treatment, only a partial good can be done, and other methods of stopping the crystallisation must be adopted, or still better the oxalic acid diathesis must be removed.

The second method of treating the oxalate of lime deposit in the urine consists in stopping the formation of the crystals. Removing the oxalic acid diathesis would stop the crystallisation as surely as it would be effected if no lime could be allowed to pass out of the body; and though both these methods may be of use, a still more immediate effect on the crystallisation may be obtained by the mineral acid treatment.

Dr. Prout first ordered a mixture of one part nitric and two parts hydrochloric acid. This is called nitro-muriatic acid. It varies in composition according to the temperature at which the acids are mixed and kept, and according to the time they remained mixed before they are used. Sometimes it is a mixture of nitric and hydrochloric acid, or it may contain chlorohyponitric, NO_2Cl_2, and chloronitrous acid, NO_2Cl, or even hyponitrous acid, HO_4, and chlorine. It is, no doubt, possible that a mixture of these substances may be more beneficial as a medicine than any one of them used separately; and if this can be proved to be true, such a mixture should be used; but the progress of our knowledge of remedies depends on the use of each substance separately, in order that the greatest possible clearness may be obtained as to the effect of each remedy; and, as it is by no means proved that so-called nitro-hydrochloric acid is superior in any respects to the mineral acids taken singly, I consider that an advance in clearness of intention, if not in power of treatment, may be obtained by using each acid separately, according to the properties which each must be considered or may be found to possess.

And first regarding hydrochloric acid. As this is more especially the acid of the gastric juice, it seems reasonable that it should be prescribed, more particularly when there is feeble digestive power; after long illnesses, when animal food begins again to agree, it should be taken almost immediately before each meal of animal food. Thus the greatest immediate use can be got out of hydrochloric acid.

Secondly, regarding nitric acid. This acid was at one time strongly recommended as a substitute for mercury in syphilis and in the liver diseases of India. Generally, it may be considered to promote secretion, and perhaps oxidation; and with this intention it should be given so as to be absorbed before food enters into the stomach. If taken one hour or two hours before meals on an empty stomach, the nitric acid acts on the system and not on the food.

Thirdly, sulphuric acid has been always used as an astringent to stop perspiration, to stop hemorrhage, to stop diarrhœa. It may be, therefore, regarded as the opposite of nitric acid in its action on the system; and to obtain the greatest action it also should be given in time to admit of its absorption before food is eaten.

Thus, then, rightly used, hydrochloric acid promotes digestion, nitric acid secretion, sulphuric acid constriction.

In addition to these special actions, there is a direct local action of these substances on the stomach. When used in small quantities, with care, they all probably render the stomach less irritable than it otherwise would be; but if the dose of any of them be too strong, it probably increases the sensitiveness of the mucous membrane, and may cause violent pain and spasm.

increased chemical action producing altered mechanical and chemical actions in the mucous membrane of the stomach.

All these acids also have a distinct action on the urine. Although there is as yet no positive proof that dilute mineral acids increase the acidity of the urine at all to the same degree that dilute vegetable acids will do.

If the amount of sulphates, for example, in the whole quantity of water passed in twenty-four hours for three successive days when no sulphuric acid is taken is compared with the amount of sulphates in the whole quantity of water passed in twenty-four hours for the three succeeding days when sulphuric acid is taken, it is certain that dilute sulphuric acid taken in very large quantity does pass off combined or uncombined in the urine. The third day after half an ounce of dilute sulphuric acid, specific gravity 1·1084, had been taken daily, 43 ounces of urine were passed of specific gravity 1025·4. 12·95 grains of sulphate of baryta were precipitated from 1000 grains of this urine; when no acid was taken, 42 ounces of urine were passed of specific gravity 1023·4. These gave 9·19 grains of sulphate of baryta per 1000 grains of urine.

All the mineral acids lessen the tendency of the oxalate of lime to form crystals in the urine. If two equal portions of urine are taken, and the minutest quantity of mineral acid is added to one portion, whilst the other is kept for comparison without acid, oxalate of lime may be found in the urine to which no acid has been added, whilst in the other no crystals will form. The same action may occur when these acids are taken as medicine, and with nitric acid there is the additional recommendation that the oxalic acid diathesis itself may, by the promotion of oxidation, be lessened, or, with perseverance, removed.—*Med. Times and Gazette, March* 11, 1865, *p.* 247.

29.—ON ACID GRAVEL.

By Dr. H. Bence Jones, A.M., F.R.S., &c.

Nature of the Gravel that is caused by Over-acidity.—Generally speaking, it may be said that all the albuminous substances of vegetable or animal food, before they are thrown out of the body, pass through the stage of uric acid. A particle of albumen absorbed into the blood from the food may become part of an organ and serve the purposes of life, and then undergoing change become resolved into uric acid, then into urea and oxalic acid, and finally into urea, carbonic acid, and water; or the particle of albumen may go through a shorter circuit, and without becoming part of any organ or texture it may be transformed into uric acid, and ultimately pass off as urea, carbonic acid, and water.

Thus uric acid, like sugar, may come from two sources—first from the food, and secondly from the textures; and as sugar is one of the results of healthy chemical action, through which, generally speaking, every particle of starch must pass, so uric acid is one of the substances through which each particle of albumen in health passes before it is thrown out of the body. When more food is taken into the blood than is wanted for the muscles, nerves, and texture generally, the excess is thrown out in a more or less perfectly oxidised form by the kidneys, skin, and lungs; it may be as uric acid, or if further changed as urea, oxalic acid, and water; or if burnt, as far as possible, as urea, carbonic acid, and water; or different portions of the albumen may be thrown out in all these different states at the same time.

The excess of uric acid in the urine of those who have just taken an excess of albuminous food, whether animal or vegetable, is very easy of proof.

The quantity of uric acid in the urine I found to vary (see Philosophical Transactions, part ii., 1849, p. 235)—

		Grains.	Grains.	Sp. gr.
Before food (mixed diet)	from	0·048	per 1000 of urine.	1026·3
	to	0·17	,,	1023·1
After food it varied	from	0·39	,,	1021·0
	to	0·92	,,	1031·1

	Grains.	Grains.	Sp. gr.
On vegetable food—			
Before food . . .	. 0·049	1000	1024.0
After food . . .	. 0·636	,,	1026·2
On animal food—			
Before food . . .	. 0·049	,,	1024·8
After food . . .	. 0·77	,,	1029·9
After three days of animal food only the highest amount was	1·022	,,	1027·8
After three days of vegetable food only the highest amount was	1·010	,,	1025·6

The second source of uric acid is in the muscles, nerves, and other textures of the body. Wherever albuminous substances exist—that is, in each portion of the body—integration is continually balanced by disintegration, and one of the products of this disintegration of albumen is uric acid, which returns into the blood and, more or less oxidised, passes off by the kidneys, and is found in the urine when no albuminous food at all has been taken for many days.

Derived from either source, uric acid exists in combination with alkalies dissolved in a more or less alkaline fluid in the blood. In the cells of the kidneys and elsewhere a process of oxidation occurs; other acids are produced, and the alkaline urates are made into more acid urates, which pass out of the kidneys to the amount of six to ten grains of uric acid daily dissolved in an acid fluid which is continually varying in acidity. Hence the composition of the urates in the urine is continually differing at different times.

In a paper in the Journal of the Chemical Society for 1862, vol. xv., p. 201, I have shown that in health potass, soda, and ammonia are all combined with uric acid in varying quantities, and these mixed urates constitute the ordinary granular amorphous deposit which is soluble in the warm urine. In three analyses I find—

	First Analysis.	Second Analysis.	Third Analysis.
Uric acid . .	94·36	91·06	92·11
Potassium . .	3·15	3·78	5·06
Ammonium . .	1·36	3·36	1·61
Sodium . . .	1·11	1·87	1·20

These acid urates are sometimes combined with uric acid, forming still more acid urates, corresponding to quadoxalates, from which a portion of the uric acid is set free even by washing the amorphous sediment with water. If any acid acts on the acid urates they are decomposed, and uric acid is set free more or less quickly, according to the strength and temperature at which the acid acts. Thus, if in the cells of the kidney by oxidation mineral, vegetable, or fatty acids are produced, the urates are decomposed either in the tubes of the kidney, the pelvis of the kidney, the ureter, or in the bladder. Hence any acid whatever except the acid of extreme oxidation—carbonic acid—may give rise to uric acid gravel. Whatever the acid that may be taken in excess in the food, or whatever the acid that may be thrown on the kidney by inaction of the skin, or whatever the acid that may be produced in the stomach or in the kidney itself, if it decomposes the urates it may give rise to uric acid gravel. Acid phosphate of soda is not able to set free uric acid from the urates: free acid must be present; and hence free uric acid is the most delicate test of other free acids in the urine.

Means of Detecting the Acid Gravel.—In contradistinction to white or phosphatic gravel, and to black or oxalic gravel, this may be called red or uric gravel: but colour, though striking, is apt to mislead. Pure uric acid is as perfectly white as phosphate or oxalate of lime, and, though uric acid attracts to itself the colouring matter of the urine, and so becomes reddish-yellow, yet there may be next to no colour to attract, and so the gravel may remain *white, and then* chemical reactions or microscopical appearances can alone *determine* the nature of the deposit.

The chemical test for uric acid is very characteristic. The smallest portion of sand or gravel heated with nitric acid at a gentle heat is immediately oxidised. Among the products is murexan, which gives, with ammonia, the richest purple murexid, and by this reaction uric acid may be always known. Thus, however, you cannot determine whether the uric acid is free or in combination. Chemically this may be partially determined by fully oxidising the sand or gravel; if, on combustion, an ash remains, the uric acid is combined with some fixed base; but ammonia is volatile, and thus the surest method of determining whether the uric acid is combined or free is to observe the deposit as it passes in the urine with the microscope. Moreover, thus a single crystal of uric acid can be recognised; a quantity much too small to be tested chemically can be determined microscopically with certainty. Of course, errors of observation may arise, but the practised eye will learn to recognise the various forms of uric acid crystals, and possibly the peculiarity of form may lead to a knowledge either of the acid which set the uric acid free or of the composition of the fluid in which the uric acid was set free.

Having determined that free uric acid is present, it is of importance to know how soon the crystals form after the urine is secreted by the kidney—whether they form in or out of the body. For this the urine should be passed into a bottle or glass and examined immediately, and if no crystals are then seen, every few hours a fresh examination should be made; and thus the amount of acid which is acting on the urates may be estimated and counteracted.

Symptoms Produced by Gravel.—When the red sand only forms after the urine is passed, it causes no symptoms whatever; but the acid or acids in the urine, which after a time liberate the uric acid, these may produce irritability of the bladder or of the urethra. Over acid urine sometimes causes excessive frequency and urgency of making water. The mucous membrane may be made so irritable that inflammation and all the symptoms and appearances of violent gonorrhœa may be produced; or the muscular texture of the urethra may be irritated, and spasm ending in spasmodic stricture may occur.

These chemical irritations do not, strictly speaking, proceed from the red sand; they come from the cause of the red sand; but they are necessary accompaniments of the mechanical irritations which the gravel occasions; and these we have now to consider.

As the gravel may form in the bladder, the ureter, or the kidney, so the mechanical irritations may begin in these different positions, and the symptoms will vary according to the part that is irritated.

We will take the mechanical symptoms produced in the kidney first, although the formation of gravel in the kidney is a sign of the greatest degree of acidity, because the urine only remains a few seconds in the kidney, whilst it may remain some hours in the bladder. The acid in the kidney must be much stronger, in order to set free the uric acid, than it need be in the bladder to produce the same effect, because less power in longer time produces the same result as a greater degree of acidity in a shorter time.

As long as the gravel is small, and lies unmoved in the pelvis of the kidney, no symptoms occur. During rest no microscopical or chemical signs or general symptoms show the presence of a foreign body in the kidney; but if sufficiently rough motion is made, then the gravel moves in the kidney, and slightly scratching the surface blood is mixed with the urine. The injury to the pelvis of the kidney may be far short of causing pain or visible blood; there may be so little that chemical test for albumen may give no reaction of that substance; but yet the microscope may show distinctly a very few blood globules. These may come with motion and cease with rest. With much motion, much blood will come, and be visible in quantity in the urine; and this may occur without the least pain. So that I often ask my patient with hæmaturia from this cause whether he would know that anything was wrong if he were blind, and the answer is usually positively no.

Gradually as the gravel increases in size in the kidney, slighter motion brings blood, and pain, and uneasiness in the loin. Distension of the colon, with flatulence, when it presses on the kidney, causes considerable pain.

If the gravel does not *pass into* the ureter, very little pain arises until the

increase of size leads to pressure on the kidney and absorption of structure. I have seen cases in which I have no doubt that calculi have been in the kidney for very many years. In proof of this I could mention, from among many others, cases of seventeen, twenty, and probably of fifty years' duration.

The symptoms produced by gravel in the ureter are much more severe than those produced by gravel in the kidney. The instant the gravel or stone produces mechanical pressure violent pain comes on. The funnel form of the commencement of the ureter, allows the gravel easily to escape a short distance from the kidney; and hence after some sudden jerk or even a turn in bed the pain begins suddenly and is referred to the kidney itself. Through nervous sympathy, vomiting soon follows, according to the size of the gravel or stone, and the size of the ureter. The narrow passage is more or less quickly passed, and with a slight obstruction at the still less elastic entrance into the bladder the mechanical impediment is over, and the symptoms cease.

The symptoms produced by gravel or small stone in the bladder correspond with those produced in the kidney. At first there are no symptoms at all: violent exercise may produce microscopic appearance of blood, but until the gravel tries to escape, or is thrown by contraction of the muscular texture on the neck of the bladder no pain is felt. Gradually as the stone increases, or the bladder becomes irritated and wants to contract frequently, blood becomes visible, and pain on motion and on emptying the water becomes severe. This pain is referred to the extremity of the nerves at the end of the penis. The flow of urine is momentarily broken by the gravel or stone stopping the passage. If the stone is small enough or the urethra large enough the foreign body escapes into the tube, and in a longer or shorter time it is driven out.

Although gravel may stop in the urethra and gradually increase there until mechanically removed, yet it is very unlikely that it should begin to form there. If the crystallisation commenced there, the passing urine would rapidly wash the crystals away before they caused any symptoms; so that the urethra does not suffer primarily from wrong chemistry. But the peripheral pain of the glans penis, and the scalding of the urethra from the acid urine, and the mechanical injury which the sharp fragments of acid stones occasionally produce in the urethra after lithotrity; these all show that even the penis does suffer from mechanical secondary disorders arising from primary chemical complaints.

Treatment of Acid Gravel.—In the treatment of every kind of gravel, the first question is regarding its solvency. The solution of uric acid gravel in any alkaline fluid that can be borne in contact with the mucous membrane of the urinary organs is scarcely possible. It can hardly be doubted that uric acid in a fine state of division as powder could be dissolved, but the problem is to dissolve uric acid in a more or less massive state. The mechanical force of cohesion of the crystalline particles as well as the chemical insolubility of each crystal has to be overcome. And notwithstanding the appearance on the surface of some uric acid calculi, and notwithstanding the supposed disappearance of calculi that have been considered to be in the urinary organs, and notwithstanding I have made numberless experiments, in the body and out of the body, to find a solvent for the stone in the kidneys or in the bladder, yet at present the state of aggregation of masses of uric acid cannot be overcome chemically. Acid gravel has not been dissolved in the kidneys or bladder, though my friend Dr. Roberts, of Manchester, tells me he expects to do both.

Until the gravel comes away or is removed, two chief indications for practice exist. Firstly, to prevent it from hurting the mucous membrane, and secondly to stop its enlargement.

Pain and blood are the evidences of hurt to the mucous membrane. Hence, any amount of exercise or of motion may be taken, provided it does not cause pain or blood. The amount of pain may be taken as the measure of the risk of inflammation, and the amount of blood may also be taken as the *measure of* the mechanical hurt to the kidney, and of the loss of strength *which results from* hemorrhage. Both pain and blood may be prevented or

temporarily removed by rest; but long-continued perfect rest would in most people soon injure the general health. So that the mean must be found by trials, the problem being to keep up the general health by exercise, but to avoid by rest inflammation or hemorrhage—that is, chemical or mechanical injury.

The second indication is to stop the growth of the stone. In other words, to prevent fresh acidity; for thus the gravel is kept so small as to admit of perfect cure by the escape of the mechanical mass through the urinary organs. The means of preventing acidity have been already brought before you. Here I shall only say a few words on the good of dilution, which not only delays the precipitation of the uric acid, but helps to wash out the gravel or stone. The best diluent is some alkaline mineral water, as Vichy, or Fachingen, or Seltzer water, taken not only with the food, but two hours before each meal. When there is no acidity, soft spring water night and morning is the best diluent.

This brings me now to the treatment of the passage of gravel or stone from the kidneys or bladder. The chief objects to be obtained are, first, to relieve pain; and secondly, to promote the passage of the calculus.

The pain is most rapidly removed by chloroform or ether inhalation. This, with care, may be continued for hours. The slightest unconsciousness is all that is necessary; but this must be kept up, for on the return of consciousness the pain is again felt.

The second remedy is the hot-bath at the temperature of 100° F. continued for hours, unless it causes faintness. Four hours' bath and then two hours' warm blankets; then three and a-half hours' bath and three hours' warm blankets; continue in this manner, gradually prolonging the rest and shortening the bath. Nothing weakens like pain. Or any hot applications may be used when the bath cannot be taken.

The third remedy for pain is opium externally or internally.

Externally it may be used as a fomentation in any quantity; as laudanum or solution of morphia poured on a poultice, or with one-eighth to one-sixteenth of chloroform on spongio piline. Internally, half a grain of acetate of morphia in solution may be injected into the loin; within a few minutes it will act potently on the pain; or opium, or any of its preparations, may be given by mouth freely, and the severity of the pain renders the system tolerant of large doses; but the ill effects of the sedative in causing inaction of the liver and constipation are very evident when the pain ceases. Half a grain or even two-thirds of a grain of morphia may be given by the mouth, and repeated in four or six hours, if the pain lasts; or thirty drops of laudanum may be taken, and repeated in two or three hours. For the relief of the suffering it is best that no aperient should be taken at the same time as the opiates, the action of the aperient counterbalancing the action of the narcotic on the bowels. Calomel in full aperient doses, as it stops sickness, may be given with the opium if there is great constipation, or if there is great intolerance of narcotics.

Secondly, the passage of the calculus is probably made easier by all the means that relieve pain. When the spasm is relaxed by chloroform and opium, or by the warm bath, or fomentations, the escape of the calculus is made more easy. Still, the pressure of the urine behind the obstruction is the great motive power in the passage of the gravel or stone. This force comes into action only when the obstruction is more or less complete, and then the pressure of the fluid is the same on the structure of the kidney as on the obstruction; and thus the secretion of urine is lessened and the pressure more and more slowly reaches its maximum. Diluents may early in the attack be of use in increasing the pressure; but after the symptoms of suppression of urine occur, they are of less use in this respect, and when urinous poisoning comes on, vomiting and purging give relief by removing some of the poison. Usually, without emetics or aperients, sufficient, sometimes excessive, evacuation takes place; and if this tertiary chemical poisoning does not bring the heart to rest, the obstruction of the ureter may be removed by the *continued pressure*, and then, the circulation through the *kidney being free, the blood is* rapidly purified, and a chemical disease far

more dangerous than the original chemical complaint (acidity) rapidly passes away.

When the obstruction cannot be removed, not only does the pressure of the secreted urine stop the purification of the blood by stopping secretion, but the urine that has been poured out must, by diffusion, pass back into the blood. When the blood is urinous, diffusion must carry the poison to every part of the textures, even into those parts where no vessels exist—the cornea, the crystalline lens, the cartilages, and perhaps, even into the nails and the hair. The uric acid, oxalic acid, and urea poison the nerves, and muscles, and blood-vessels, and other textures, and render them incapable of carrying on the actions of nutrition and oxidation.

When the gravel passes into the bladder, usually little or no treatment is requisite. After a longer or shorter delay the stone is forced into the urethra, and ultimately is driven out; but whilst it remains the same objects are still to be kept in view—first to promote the escape of the stone; secondly, to prevent its growth; and thirdly, to keep the bladder from harm.

Free dilution and alkaline remedies best fulfil the first two objects, whilst the last is chiefly to be gained by rest. Inasmuch as the bladder is nearer the external orifice than the kidney, two or three other remedies are possible.

First, there is great surgical authority for occasionally passing a catheter when a small stone is suspected to be in the bladder. If there be no stricture, it is not easy to see how this should help the passage of the stone more than passing a probang before food would make swallowing easier.

A gentleman came to me one morning, and from the microscopic blood in the urine and the pain in making it, whilst his general health was robust, I considered a stone had formed in the bladder. I went with him to a surgeon, who sounded him, and said "There may be a stone, but I cannot find it." The following morning the patient brought me the stone, which he had voided in the night. To him, and to his surgeon, it was beyond all doubt, that the passing the sound made the stone come away.

Secondly. The Astley Cooper forceps, by which a small stone may be removed whole through the urethra; and the lithotrite, by which a stone may be removed in pieces; and, still more, lithotomy, by which the foreign body is removed whole through an artificial opening, are mechanical proceedings belonging to the surgeon and not to the physician.

Thirdly. There is yet another method of cure, so far beyond all other methods in its philosophy, its safety, and its freedom from suffering, that success, though still postponed, must ultimately be attained. This is the dissolving the gravel in the bladder. There is no difficulty whatever, excepting want of patience and chemical knowledge, that hinders the solution of phosphates in dilute acid. A stream of diluted lactic or hydrochloric acid, made so weak as not to irritate the bladder, and passed at the temperature of 100° F., must dissolve any phosphatic gravel or stone or outer layer of stone, provided only that the current is continued long enough.

A warm stream of very diluted lithia or soda should dissolve any urates and attack uric acid. But gravel and stone vary very much in density, that is, in compactness of aggregation, and some uric acid gravel is so hard that it is vain to expect that any stream of water or of alkaline water will have any effect in any reasonable time.

Hence it becomes necessary for the solution of some uric acid stones, and for all oxalate of lime stones, to discover some way of exposing the gravel or calculi to the action of stronger agents than dilute lithia or soda.

In the Philosophical Transactions for 1852, p. 201, I have shown what can be done out of the body by disengaging nitric acid and potassa in contact with uric acid by the help of the voltaic battery. Although this is easy in glass vessels, it is very difficult to make a catheter that can cause this action to take place within the bladder, and after months spent in producing an instrument fulfilling all the electrical conditions for success, the equally essential surgical conditions are by no means perfect. There is a want of firmness and fixedness of the blades which make the introduction of the *litholitic instrument* difficult. Still, when surgeons skilled in electrical science, *and possessed of* sufficient time and patience, arrive, I have little doubt but

that uric acid calculi will be dissolved in the bladder. Some more energetic substances may be liberated in contact with the stone, or some way may be found of rendering the stone more soluble. At present progress is stopped more by the mechanical than by the chemical difficulties of the problem.

Oxalate of lime gravel or calculi are so hard and so insoluble that as yet there is no prospect of obtaining any good results by any process of solution. But oxalate is so closely related to carbonate of lime, and this last is so rapidly soluble in any dilute acid, that hopes must be held firm that some day a process of oxidation may be carried on in the bladder to compensate for the want of that oxidising action in the blood and organs which leads to the formation of this kind of stone.—*Medical Times and Gazette, March* 25, *and April* 1, 1865, *pp.* 301 *and* 328.

30.—ON PHOSPHATIC GRAVEL AND CALCULI.

By Dr. George Harley, Professor in University College, London.

[Deposits of phosphates may take place in the kidney, giving rise to renal calculi, but they more commonly take place in the bladder. The pathology of the two is very different. Thus, renal phosphatic calculi arise in one of two ways,—either from the urine containing an excess of phosphates, in consequence of a vice in the tissues which contain them; or from the urine being alkaline at the moment of its secretion in consequence of being secreted from abnormally alkaline blood. In the case of vesical calculi, in addition to the causes just mentioned there exists a fruitful source of origin in the numerous diseased conditions of the bladder itself.]

As regards the first step in the formation of calculi, it may be laid down as a general law that *their formation in any particular part of the urinary system is due to some special local cause.* If the calculus begins in a urinal tube, it is on account of the tube getting blocked up with mucus, epithelium, or a little blood. Or it may also arise from the tube being temporarily constricted in some parts of its course in consequence of inflammatory action. Once, however, that the deposition of urinary sediment has begun, the original cause is soon lost sight of, and the calculus goes on forming round its nucleus quite independent of the local condition which first called it into existence.

When a phosphatic calculus, instead of beginning in a urine tube, originates in the pelvis of one of the kidneys, there may be no absolute obstruction to the flow of the urine, but merely a slight interruption, as from a fragment of mucus adhering to the side of the lining membrane of the pelvis. For when a predisposition to calculus formation exists it requires but a very trifling cause to call it into operation. Almost anything may form a nest round which the stone will gradually become deposited.

This is no fanciful description of the formation of renal calculi, as you may see by taking the trouble to examine the series of examples now on the table, each of which bears evidence of the truth of the statement. In fact, the assertion might be proved without any specimens at all, from the simple circumstance of the frequency with which we meet with renal calculi limited to one kidney in patients predisposed to stone. That fact of itself clearly shows that the deposition of the stone in any one particular spot must be entirely due to some local cause. Thus, for example, we have here a kidney, which contains no less than fifty-nine uric acid calculi, and yet neither in the bladder nor the opposite kidney of this gentleman was a single stone to be found. The deposition of all these calculi in one kidney must therefore have been entirely due to some local cause. At first sight, the cause does not appear very evident, nor is it apparently easy to account for the fact that although at least fifty of these calculi are not bigger than barleycorns, none of them have found their way into the ureter, and from thence onwards into the bladder. The reason of the last fact is, however, apparent when the specimen is carefully examined, and as it will be easier to explain the former proposition after *we have discussed* the latter fact, we shall consider the last

first. The largest stone, which, from its position and size, is evidently the one which was first deposited, is of a very peculiar conical shape, and the apex of the cone projects into the orifice of the ureter, and forms a movable plug, which is, in fact, a self-acting valve. No sooner does the pelvis of the kidney become filled with fluid, than its distended walls cease to embrace the stone, and consequently the fluid flows past the valve until the pelvic walls again come into contact with it. The separation of the walls from the valve, although being sufficient to allow liquid to pass, does not appear to be sufficient to admit of the escape of even the smallest of the stones. Now exactly the same thing as we have done artificially with water must have been daily done with the urine.

Gradually as the urine was secreted, it would distend the pelvis of the kidney and separate its walls from the valve-shaped calculus; but no sooner would this occur than the accumulated urine would make its escape. So that every now and again there would be little accumulations of urine, and during the period of these little accumulations not only would the smaller calculi form, but the larger one, the plug, would gradually increase in size. In this case, it is perfectly evident from the immense number of the calculi that the predisposition to stone existed; and from their being all limited to one kidney it is just as evident that it was some local cause which induced them to select the left instead of the right kidney. In Dr. Roome's case, again, it was precisely the reverse: all the calculi were found in the right kidney; not one existed in the left; which is additional evidence in favor of our views.

In my own case, moreover, it is further shown, by the absence of all calculi both from the right kidney and from the bladder, that, although the predisposition to stone existed, yet without some local cause no stone would have been deposited in the urinary passages of the patient—a most suggestive fact, as showing the importance of paying attention to the proper performance of the renal function. Who can tell what a trifle at first induced the deposition of the uric acid nucleus, which ultimately became the plug and caused the formation of all the rest?

The foregoing general principles are not alone applicable to the pathology of phosphatic, but to all varieties of renal calculi—oxalic, uric, xanthic, &c.

As regards the first step in the formation of vesical calculi, it may, with equal justice, be said that whenever the predisposition to stone exists, it requires but a very trivial cause to bring it into operation. For, as with renal calculi, almost anything may act the part of a nucleus round which the stone will become deposited. An insignificant particle of mucus, a microscopic portion of albumen, or an epithelial cell, is all that is requisite for the purpose. In the vast majority of vesical calculi, indeed, it is impossible to detect the primary point round which the urinary salts become adherent; but every now and then we come upon calculi with well marked nuclei, some of which are not only quite foreign to the urinary passages, but even foreign to the body itself. Thus, for, example, this large ammoniaco-magnesian stone (No. 3425 of Liston's collection in the museum of University College), which was extracted from the bladder of a man aged sixty, has a piece of beeswax for its centre; while three other small calculi (No. 3478), removed from the bladder of a man aged seventy, have barleycorns as their nuclei. It is perfectly clear these substances were not generated spontaneously in the urinary passages, and it is highly probable also that they did not find their way there quite accidentally; but with the mode of entrance we have at present nothing to do; so we may turn our attention to this other vesical calculus (No. 3515 of the same collection), which was removed from a woman's bladder. The nucleus of this stone consists of a piece of bone—a fœtal tibia, in fact—which found its way by ulceration into the bladder from the abdomen, the case having been one of extra-uterine fœtation. Any solid substance, indeed, may prove the starting-point of a vesical stone, just as a thread, piece of string, or anything else, may form the nucleus of a crystal *of sugar-candy.*

In connection with the origin of calculi, there is another point particularly

deserving of attention—namely, that local causes are the most potent in the production of stone, and that even where the predisposition to it exists, without some local cause no stone is likely to form.

Vesical phosphatic calculi are frequently induced by inflammation of the bladder. The inflammation gives rise to a copious secretion of mucus, which, acting as a ferment, causes the urea to decompose, while the urine still sojourns in the bladder. The decomposition of the urea leads to the evolution of ammonia, which, in its turn, deposits the earthy phosphates, and the gradual formation of a stone is the result.

Many phosphatic calculi have a uric acid nucleus, while some few have oxalate of lime for a centre. The deposition of phosphates, and consequent increase of the stone in such cases, is due to one of the four following causes:—

1st. Although the patient has recovered, either by time or by treatment, from the uric or oxalic acid diathesis, the stone that has already formed still remains in the bladder, and immediately, or at some future date, causes such an amount of irritation as to induce an excessive secretion of mucus: which mucus, acting as a ferment, brings about the decomposition of the urea, and consequent formation of the alkaline carbonate of ammonia, which, in its turn, induces, as in the preceding examples, a deposition of the earthy phosphates normally present in the urine.

In this case the deposition of the phosphatic calculus is not due to a vice in the general system, but the product of an entirely local cause.

2nd. A patient with a small uric or oxalic acid stone in the bladder, which induces no irritation whatever, is on some occasion or other seized with retention of urine. It may be from stricture, enlarged prostate, paralysis of the bladder, or any other cause liable to induce it. In consequence of this retention the urine becomes decomposed, and a deposition of phosphates takes place round the nucleus, the presence of which until now was of no importance whatever. The deposition of phosphates is, however, copious, and the stone rapidly increases in size, and becomes of itself a source of irritation. Here, then, neither the original nucleus nor the constitution of the patient is to blame, but a local cause quite independent of either.

3rd. A deposition of phosphates may take place round a nucleus of uric acid, oxalic acid, or anything else, not on account of its setting up irritation, not because of the urine being retained and decomposed, but from there being an excessive elimination of phosphates by the kidneys. In common language, on account of the patient laboring under the phosphatic diathesis.

Lastly. A phosphatic calculus may form round a uric acid or other nucleus without any of the already-mentioned conditions existing, but simply and solely from there being an absence of that normal acidity of the urine which is essential to the retention of the earthy phosphates in solution until its emission from the bladder.

The diagnosis of cases of phosphatic calculi, renal as well as vesical, is not only greatly aided by an examination of the urine, which may be said to contain, as a general rule, less phosphoric acid than normal, but even the differential diagnosis of the various causes inducing the calculi is also aided by an examination of that liquid.

1st. In cases arising from an excessive elimination of phosphates the reaction of the urine is normal, and it has no ammoniacal odour, nor does it deposit an excess of mucus.

2ndly. In those arising from an abnormal alkalinity of the blood the urine, although alkaline at the moment of its emission, either immediately after meals or during the whole day has no ammoniacal odour, nor does it deposit mucus in excess.

3rdly. In the first and second class of cases the urine is ammoniacal as well as alkaline when passed, and usually yields a copious deposit of mucus.

Lastly. The first and second class of cases are readily distinguished from each other by their histories.

Besides the triple or ammoniaco-magnesian phosphate, which is the most common kind of phosphatic calculus, there is also another variety composed

solely of the simple phosphate of lime. This is comparatively speaking a rare form of stone, and fortunately its appearance is so very peculiar that it can be readily recognised without an analysis. Its external surface is smooth, polished, and shining like glass. It is exceedingly brittle, and has the peculiar tendency to break into regular layers like an onion.

Treatment.—From what has been already said it must have been seen that the various classes and kinds of phosphatic calculi each demand a separate mode of treatment, and that the old method of treating all kinds of phosphatic calculi alike is radically wrong. The pathology is the guide to the treatment.

In the first class the stone itself has to be attacked; in the second class the cause of the retention is to be removed; in the third class the vice in the tissues has to be got rid of; while in the fourth the normal acidity of the urine is to be restored. One and all of these are more or less within our power.

First, as regards the class of cases where the deposition of phosphates depends upon a decomposition of the urine taking place in the bladder. That we have seen may originate in two ways, and, as we shall again have occasion in another lecture to refer to the mode of treating both of the causes above specified, we shall for the present take it for granted that the cause has been removed, and that all that has now to be done is to get quit of the gravel or stone that has already formed in the bladder.

The common way to remove a vesical stone is, of course, to cut or crush it; but many persons, and especially old people, in whom the phosphatic calculus is most common, object to either of these operations being performed. What, then, is to be done? Fortunately, in the case of phosphatic calculi, the physician has much in his power, for of all calculi the phosphatic are the most soluble, and if only gravel has as yet formed the chance of getting it removed by dissolution is very great. The simplest mode of treating such cases is to keep the urine constantly acid by the administration of the nitro-muriatic acid three times a day, after food, and to irrigate the bladder by means of the double irrigating catheter. This is a much easier operation than most persons who have not tried it imagine. The patient being placed on his back, the double catheter is passed into the bladder, the urine allowed to flow out, and then a caoutchouc tube, at least four or five feet long, is slid over the end of the catheter, while the free end of the tube is dipped into a can containing water acidulated with nitro-muriatic acid. The can having been previously placed at a convenient height above the patient—on the top of the bed-post, for example—the caoutchouc tube is to be so arranged that when the patient is ready the acidulated water will flow in a regular stream down through the tube, along the catheter, into the bladder, and from the bladder back again through the other division of the catheter, along another caoutchouc tube attached to its opening, into a pail placed beneath to receive it. If the acidulated water be kept at a temperature of about 90° F., the stream through the bladder may be kept up for a long time without causing the slightest discomfort to the patient. When the top pail gets empty it is refilled from the other as often as thought necessary. This treatment being continued for a certain time every day, the gravel is gradually dissolved without causing the patient any suffering.

In the third set of cases strict attention should be paid to the general health, and an effort made to improve it, and thereby restrain the elimination of phosphates from the system by a judicious selection of tonics. If this is not effectual and the daily waste excessive, superphosphate of lime is to be administered, for the reasons previously given.

In the fourth class of cases, where the vice lies in the abnormal alkalinity of the blood, the strong mineral acids are to be administered. Vegetable acids, on the other hand, ought to be avoided, from the fact of their being transformed into carbonic acid in the system, which only tends to increase the mischief. In general it is sufficient to give the acid after food, but in very bad cases it is sometimes necessary to administer acid tonics also *between the meals*. In many of these cases the patients perspire freely, and *the sweat is* excessively acid, which in some measure accounts for the dimi-

nution of acid in the blood. To this class of patients I find much benefit arises from the use of nitro-muriatic acid in conjunction with gentian after meals, and a dose of sulphuric acid, to diminish the perspiration, between them. The patient's diet must also be carefully regulated: the animal food increased, the vegetable food diminished. The reason of this is plain enough, if what was said in the first lecture regarding the influence of diet on the reaction of the urine be considered. Herbivorous animals have alkaline, carnivorous animals acid urine, so that the more the patient resembles the carnivorous animal in his diet the less alkaline his blood, and the more acid his urine will become.

In trying thus to check the formation of the gravel or stone, care ought at the same time to be taken not to run to the opposite extreme, and by increasing the acidity of the system too much produce a deposition of gravel or stone of another kind. Uric acid, for example. The safeguard against falling into this error is found in the condition of the urine. As soon as the normal acidity of the urine is permanently restored the acid treatment and animal diet is to be stopped.—*Medical Times and Gazette. Nov.* 5. 1864, *p.* 483.

31.—ON BRIGHT'S DISEASE.

By Dr. William Jenner, F.R.S., Physician in ordinary to H.M. the Queen and H.R.H. the Prince of Wales.

[Dr. Jenner commences with a consideration of the general facts in morbid anatomy which have an important bearing on the interpretation of the lesions of structure of the kidney in Bright's disease.]

During life, such portions of animal structures as are not absorbed when they cease to be duly nourished, either calcify (i.e., petrify), or they undergo, first granular disintegration; and then fatty metamorphosis, or degeneration as it is called. Chondrin and gelatin yielding structures more commonly calcify; protein compounds more commonly experience disintegration and fatty degeneration. In the last case there is a re-arrangement of the ultimate elements of the part, carbonate of ammonia and olein being the results. The protein compounds which experience these changes may be physiological or pathological structures or products. The cells of the kidney, the blood, the vessels, are all physiological structures, made up more or less of protein compounds; and it is matter of observation that, when any of these parts are so damaged as to cease to perform function, they undergo granular disintegration and fatty metamorphosis.

Lymph, a pathological product, when exuded into the kidney and not developed into tissue, undergoes the same disintegration and metamorphosis.

Varieties of lymph.—Under the word lymph are included various protein exudation matters.

One variety of lymph, quickly after it is exuded, disintegrates, forming innumerable molecular granules, which themselves rapidly undergo fatty degeneration.

Another variety of lymph has for its great characteristics rapid diminution of bulk, or contraction, forming itself, as it contracts, into imperfect fibres.

A third variety retains its transparency, homogeneousness, and bulk for a very long period.

And a fourth variety developes or is developed into those fibre-cells which Lebert so well described, and to which he gave the name of fibro-plastic corpuscles and fibres.

The exciting cause of the pathological lesion on which the exudation depends, the part into which the exudation occurs, the rapidity with which it takes place, and the constitutional state of the patient at the time it exudes, all appear to have an influence in determining the special variety of lymph that shall be exuded in any given case. And as several of these conditions vary during the same illness, you will understand why it is that although one or other of these forms of exudation may preponderate in any given case, the others in less *quantity are found* commingled with it.

For the sake of brevity, we may designate the

1st variety		Granular lymph.
2nd	,,	Contractile lymph.
3rd	,,	Albuminoid lymph.
4th	,,	Fibro-plastic lymph.

These varieties of lymph present another difference of great pathological importance—viz., that while the first and the third have considerable penetrating or transuding power, and therefore infiltrate every texture in their vicinity, the second and fourth have, like the blastema of tubercle, very little penetrating power, and are found near only to the part at which they have been exuded—they do not infiltrate the textures in their vicinity.

To determine during life the size, tenderness, &c., of the kidneys.—In a perfectly healthy but moderately thin person the kidneys may be readily felt by the hand. The left, partly from its being longer, is more easily handled than the right. Neither tenderness nor moderate enlargement can (except in the very fat) escape detection. To feel the left kidney you place the fingers of the right hand directly under the last rib, *just* outside the *mass* of lumbar muscles, and the points of the fingers of the left hand *just* inside the corresponding spot in front; the kidney is between the two. Depress the fingers of the left hand, and at the same time raise the points of the fingers of the right hand, and you will have no difficulty in feeling the kidney, in defining its outline, its mobility, &c. This mode of palpation is far more practically useful than is percussion for determining the size of the kidneys.

Special pathological conditions of the kidney which may lead to Bright's disease.—I proceed to pass in review those lesions of structure which affect every part of both kidneys, and which, if not rapidly cured by nature or by art, are accompanied by albumen in the urine, by anasarca, and by retention of the urinary elements in the blood.

Congestion.—Although now and then some tumour, as the gravid uterus, presses on the renal veins, and so mechanically causes over-filling of the veins of the kidney, still by far the most common cause of this form of passive congestion is impediment to the circulation through the heart and lungs.

When the blood is unable to escape freely from the veins of any organ, the first vessels to yield to the pressure of the blood on their inner surface are those which have the thinnest coats and are the least supported by the textures in which they are imbedded. The capillaries of the Malpighian bodies are at once the thinnest coated and the least supported of any part of the vascular system of the kidney.

Incomplete impediment to the return of blood from the kidney has, then, as its first perceptible result, over-filling of the capillaries of the Malpighian bodies; if the impediment continue, then the stretched walls of the vessels permit serosity poor in albumen to escape. The impediment being continued, the next result is the escape of serosity containing more albumen and some fibrin in solution—i. e., of liquor sanguinis poor in fibrin; and the impediment continuing, the final result is rupture of the over-stretched walls of the vessel.

If the impediment be considerable and rapidly formed, then rupture of the over-filled vessel is the almost immediate result; the rule being that the more gradually the impediment is formed the less likely is rupture to occur. The state of the blood has a decided influence in determining the character of the effusion; for if the patient be very anæmic (or to use the last-devised name for what we formerly called anæmic—oligocrythro-cythæmic and hydræmic), then the escape of serosity is considerable, the serosity of watery blood, poor in red corpuscles, readily escaping through the walls of the capillaries. On the other hand, if the blood be rich in red corpuscles, then rupture of the capillaries is one of the earliest consequences of a very moderate amount of impediment.

Casts of tubes; blood-casts.—When blood is extravasated into the tubules of the kidney, some portion at least coagulates before its escape into the pelvis of the organ; coagulates, that is to say, in the tubules. The fluid poured

out by the vessels of the Malpighian bodies washes out the coagula so formed, and the expelled coagula are to be detected with the aid of the microscope in the urine. As these little coagula are moulded to the inside of the tubules, they are of the shape and diameter of the tubules in which they are formed. They are therefore really casts of the tubules. Their diameter will vary according as the tubules of which they are casts are denuded of their epithelial lining or not, and according as they are formed in tubules of the cortex or of the pyramids.

Their number will vary with the quantity of the fibrin or blood effused, the number of the Malpighian bodies from which it has escaped, and the rapidity with which the blood coagulates, and with the pressure exerted on them by the fluid poured out from the vessels of the Malpighian bodies.

If the fibrin adhere, as it sometimes does, to the cells lining the tubules, then those cells may come away with the coagulum, and constitute a great portion of the cast. If a little serosity escape from the portal capillaries, and then pass, as it must, through the basement membrane of the tubules, it will loosen the attachment of the cells to that membrane and to each other, and so favour their expulsion. Again, after serosity has passed for some time through the tubules, the cells will be to some extent infiltrated by it, their structure will be damaged, and their function impaired.

The coagula retained in the tubules, the cells damaged by albumen passing into them, the cells the attachment of which has been loosened, and the basement membrane itself injured by the passage of the albuminous fluid, will all, in accordance with general laws, undergo granular disintegration and fatty degeneration.

Before leaving the subject of passive congestion, let me point out and impress on you that chronic impediment to the flow of blood through the lungs and heart is always intermitting in degree—now almost none, then extreme, then little, and again extreme; and the congestion dependent on this impediment must also intermit or remit.

Acute passive congestion of the kidney is not unfrequently the result of feeble action of the heart, gravitation, and a state of blood unfavourable to its passage through capillary tubes. In typhus fever, in typhoid fever, and in diseases of typhoid type generally, congestion of the kidney from these causes is not of uncommon occurrence. The kidneys are not the only parts which suffer congestion under the conditions mentioned; the posterior part of the lungs, and the skin of the back generally, suffer first. In the skin we see all the changes in the capillary circulation; a faint bluish tint of the surface, with a dusky hue of that of the back; then an almost purple colour of the skin of the back; and finally the latter is studded with petechiæ, or little extravasations of blood. If we press the fingers on the skin of the patient, we remove the blood from the compressed part, and cannot fail to be struck, not only with the change of colour, but also with the time that elapses before the blood re-enters the capillaries.

In these cases of passive congestion of the kidney blood is usually present in the urine, and albumen out of proportion to the quantity of blood. As the blood in diseases of very low type—e. g., so-called malignant typhus fever and erysipelas—is little disposed to coagulate, casts of tubes may be absent, or present in small numbers only.

Passive congestion of the kidney, especially the form last described, is very often followed by reaction and the establishment of active congestion, and finally of the exudative stage of inflammation. The lymph exuded is the granular variety. Nutrition being interfered with, granular disintegration of the contents of the cells occurs. When reaction occurs, a febrile disturbance of low type is the result of the state of the kidney and of the resulting uræmia, and the primary affection is apparently prolonged beyond the date at which it should have terminated. The reaction which ensues on this form of passive congestion, and the low-typed inflammation that follows, have long been studied as they occur in the most depending part of the lungs.

Primary active congestion of the kidney—i.e., the first stage of inflammation—that stage which precedes exudation—is a common consequence of exposure to cold, and of the state of the blood in scarlet fever, and sometimes, though

more rarely, of the state of the blood in other acute specific diseases—pyogenic fever, pyæmia, septicæmia, &c.

In congestion the result of direct mechanical impediment to the return of blood from the kidney—e.g., from diseases of heart or lungs—the Malpighian vessels, as we have seen, are the first to become over-distended, and by permitting the escape of serosity, of liquor sanguinis, or of blood, may retard, moderate, or altogether prevent the over-distension of the vessels of the portal system—i.e., of those veins and capillaries which lie between and around the tubules. In the passive congestion that occurs in typhoid diseases, and still more in active congestion, there is no such exemption of the renal portal system; nay, it seems probable that the intertubular vessels are the special seat of active congestion.

If active congestion subside before lymph has been exuded—i.e., if serosity only has been poured out, then, although a considerable loss of epithelium will have been necessitated (as the cells are renewable), no decided permanent damage to the structure of the kidney will have resulted. On the subsidence of the active congestion the patient will be said to be well. The loss of epithelium will have been necessitated because, the vessels between the tubules being affected, serosity will have been effused there, transuded the basement membrane, and loosened the attachment of the cells to it and to each other. The patient, I say, will be *called* well, because, although parts once damaged are never permanently restored, still they may be so far restored that we are unable to detect any inability in them under ordinary conditions to perform function. Still forget not that these little injuries do remain, and when some extra strain on the damaged organ comes, they tell on the result. There can be no doubt that these trifling "damages" to structure are one reason why certain diseases are so fatal to those of advanced life. Now this organ, and now that, is damaged to a trifling degree, until at last although the man is "in health," none of his organs are sound.

I have described or defined active congestion to be the stage of inflammation preceding exudation; but such a stage is of most limited duration, and it is probable that in the serosity exuded almost if not quite from the first there is some fibrin in solution. Still, from a practical point of view, it is important to separate the two stages. For when exudation in quantity has occurred, then the evidences of disease are manifest and the patient becomes the subject of chronic Bright's disease.—*Lancet, Jan.* 14, 1865, *p.* 29.

32.—ON THE PRINCIPLES OF TREATMENT OF BRIGHT'S DISEASE.

By Dr. Arthur Hill Hassall, Senior Physician to the Royal Free Hospital.

I have to make a few remarks relative to the principles upon which Bright's disease should be treated. What these principles are we shall experience but little difficulty in determining.

We must remember that in two cases related by me there has been a very large drain of albumen; that the blood has consequently become impoverished; that its serum is so deficient in albumen that, in obedience to the law of endosmosis, it readily transudes from the containing vessels, and makes its way into the surrounding tissues and cavities, giving rise to the anasarca which was present to so large an extent in these cases on their first admission; that the system, therefore, is permeated, and the various functions of the body impeded and deranged by the presence of this excess of water.

That coincident with this loss of albumen there is also diminution of the red blood-corpuscles, as shown by the analysis given, and probably also of the fibrin. Of this destruction of the red blood-corpuscles the peculiarly pallid aspect of the face in both patients alone affords a sufficient proof.

That in the case of G—— the kidneys are in a state of fatty degeneration; and that, owing to the structural changes which have taken place in those *organs, there* is diminished elimination; and that consequently the dangers *of uræmic* poisoning and convulsions are to be apprehended.

I reiterate, therefore, that when these particulars are duly impressed upon the mind we can be at but little loss to comprehend the true principles on which such cases should be treated.

Thus it is obvious that our first endeavour should be to relieve the system of the surplus water which pervades the tissues, more especially the cellular tissue; and it is also obvious that, in consequence of the great loss of albumen, this object should be effected with as little additional loss of that principle as possible. This purpose is commonly sought to be attained by the free use of diuretics, and these sometimes of a very irritating character. Now nothing can be worse than this practice as a system, for it is one fraught with danger to the structure of the kidney itself, and one which, by increasing the elimination of the albumen, contributes to the reduction of that strength which is already disappearing with such rapidity. Of course diuretics are of great value in the treatment of some cases of Bright's disease, but then they must not be indiscriminately used, and should not be relied upon as the chief means employed for the removal of the dropsy.

Another means whereby an endeavour is made to disperse the dropsy is by purgatives. These are also objectionable, though less so than the use of diuretics, since by them something more than water is abstracted, and they tend consequently to increase the weakness. Valuable, therefore, as purgatives are in many cases, they likewise should not be too much relied upon for the removal of the effused water.

The means of all others which should be resorted to is the frequent use of the vapour-bath. This is both powerful and safe, and admirably adapted to the object in view. A vast surface is brought into action, and little besides water is eliminated. The effects of the vapour-bath are usually most marked, and so grateful to the patient, that, as was particularly the case with Edward G., he often asked to be allowed to have it repeated at shorter intervals than had been ordered.

Next, remembering the enormous drain of albumen from the system, and the consequent impoverishment of the blood and weakness thus produced, it is obviously most desirable that this loss should, as far as possible, be replaced. The best idea that can be given of the extent of the drain upon the system through the loss of albumen is furnished by reference to the quantity of blood represented by the amount of albumen daily discharged. In the case of Francis S., the loss of dried albumen when his malady was at its height amounted to 70·4 grains in the twenty-four hours; being the quantity contained in 1000 grains or 2¼ ounces of blood. In Edward G.'s case the mean daily loss of dried albumen was in the first series of analyses found to be 310·8 grains, equal to 4400 grains or ten ounces of blood. Now supposing this drain to continue daily, in forty-seven days and seven hours he would have lost a quantity of albumen equal to that of the whole of the blood contained in the body. The diet therefore should be highly albuminous, and should consist of eggs (especially the white portions) uncooked and of plenty of juicy and underdone meat. Unfortunately our wishes in this respect are often frustrated by the want of appetite of the patient; thus while Francis S. took freely all the eggs and meat ordered, Edward G. did not consume nearly the quantities of meat apportioned to him.

The third indication which must be fulfilled to still further improve the condition of the blood consists in the administration of such tonic remedies as act especially on the red corpuscles, as some of the preparations of iron. This indication may probably be promoted by the oxygenation of the system as far as possible by pure air and the use of an aqueous saturated solution of oxygen.

Lastly, we must endeavour to reduce the amount of albumen discharged by the administration of appropriate astringent remedies, such as gallic acid.

Of course in some cases there are other indications which require to be attended to, and which may possibly arise hereafter in that of Edward G. Thus coma or convulsions may supervene. Upon the consideration of the appropriate treatment for these conditions it is not necessary that I should at present enter, as my object is to dwell chiefly on the treatment of Bright's disease only so far as *this* is elucidated by the two cases the histories of

which I have given in the foregoing remarks. We should not forget, however, that Edward G.'s is a case of fatty degeneration, and that therefore his diet has been regulated accordingly, and fatty and oily matters prohibited.—*Lancet, Dec.* 17, 1864, *p.* 684.

33.—ON THE ACTION OF TINCTURE OF PERCHLORIDE OF IRON IN THE CURE OF RENAL AND URINARY AFFECTIONS.

By Dr. Arthur Hill Hassall, Senior Physician to the Royal Free Hospital.

There are few remedies more frequently prescribed in the treatment of renal and urinary affections than is the tincture of perchloride of iron, formerly called muriated tincture of iron. The value of that remedy in a variety of such cases is undoubted, as in the albuminuria of Bright's disease, in hemorrhage from the kidney, bladder, or urethra, in spasmodic stricture, &c.

The tincture of the perchloride of iron consists of two atoms of iron in combination with three of chlorine, dissolved in water to which rectified spirit has been added; and it possesses the properties of an astringent, tonic, and styptic, coagulating blood or albumen with which it is brought into contact, and constringing the vessels and tissues to which it is applied.

Now, this astringent property is just that which a remedy ought to possess to be useful in the cases above referred to, and by it, it is usually supposed, it exerts its beneficial action; and certainly nothing would appear to be more plausible and more natural than this explanation.

Having, after the administration of this remedy, repeatedly tested the urine for the purpose of detecting in it the presence of iron, and having failed to discover the faintest trace of the metal, I was led to doubt the correctness of this view, and was induced to institute some experiments, in order to put the matter to the proof.

To a patient, T. L., labouring under an habitual urinary discharge, I administered for the period of more than a week a drachm of the tincture thrice daily; but, although I tested the urine on several occasions, in no instance could I detect the smallest trace of iron, notwithstanding that a pint of the urine was evaporated to a small bulk before being examined.

To a patient now in the Royal Free Hospital, Charles S., who is suffering from an extravasation of blood, three drachms of the tincture were administered on two consecutive days. The whole of the urine passed in the twenty-four hours of each day was collected, a pint of each sample evaporated to a small bulk, and tested as before, but with a similar negative result.

Lastly, I myself took in the course of a day three drachms of the tincture; the urine passed in the twenty-four hours being collected and analyzed, not only on the day on which the medicine was taken, but on the preceding and succeeding days. Still no iron was found.

I could enumerate several other instances in which iron had been taken and the urine analyzed, without even traces of the metal being subsequently discovered. The examples, however, I have quoted are sufficient to show that the tincture of perchloride of iron does not produce its beneficial effects, as generally supposed, in restraining the amount of albumen or of blood discharged from the kidney or other portion of the genito-urinary mucous track by coming in contact with the seat of the lesion and by its action as an astringent.

How, then, does this remedy act? That much of the iron contained in the sesquichloride does not find its way into the circulation at all, but escapes from the system with the undigested portions of the food, is certain; the black discoloration of the fæces under the use of this tincture, and indeed, I believe, under all the preparations of iron, is well known, the colour being due to a combination of the iron with a portion of the sulphur of the food—*sulphuret of* iron being thus formed It might therefore be very plausibly *presumed* that while the greater part of the iron is thus thrown off by the

bowels without having been absorbed at all, the hydrochloric acid, being set free, enters the circulation, is eliminated by the kidneys, and so comes in contact with the seat of the lesion; and that it is to the acid, and not to the iron, that the benefit is to be attributed. But if this view be correct, it is capable of being substantiated by experiment; and with this object I administered to two persons drachm doses, repeated thrice daily, of the perchloride; the urine of the twenty-four hours being collected and analyzed before, during, and after the administration of the ferruginous preparation. The results will be seen in the following tables:—

C. S——.

	Nov. 26th. Before.	27th. Before.	28th. During.	29th. During.
Quantity ..	62½ oz. ..	70½ oz. ..	49 oz. ..	46 oz.
Acidity ..	30·35 ..	30·18 ..	33 gr. ..	30·14
Chlorine ..	115·0 ..	137·0 ..	90·0 ..	140·9

A. H——.

	Nov. 17th. Before.	18th. During.	19th. After.	20th. After.
Quantity ..	56 oz. ..	57 oz. ..	33 oz. ..	53 oz.
Reaction	Faintly alkaline.	Faintly alkaline.	Faintly alkaline.	Faintly alkaline.
Chlorine ..	95·0 ..	84·57 ..	76·0 ..	44·98

The above figures show, 1st, that there was no increase in the acidity of the urine consequent upon taking the remedy; 2nd, that there was no increase of chlorine, and that therefore the hydrochloric acid of the perchloride was not eliminated by the kidneys either in the free or combined state: thus proving that the second view mentioned of the action of the remedy is also entirely unfounded.

These results are not a little remarkable; and we have, therefore, still to enquire, in what way does this medicine act in the cure of disease? Its effects are too rapid to allow it to be supposed that its operation is due to its influence in improving the condition of the blood by its action on the red corpuscles. We appear, therefore, driven to the conclusion that the perchloride of iron acts by its stimulating influence on the nervous system.

These observations are interesting, not alone as concerns this one preparation of iron; they also probably apply more or less to most of the other medicinal preparations of that metal, since it is at least certain that by far the greater part of the iron contained in them is not absorbed but escapes from the system by the bowels like that of the perchloride.

The particulars herein recorded are suggestive of further experiments calculated to throw additional light upon the subject, and which hereafter I may have the opportunity of instituting.—*Lancet, Dec.* 31, 1864, *p.* 740.

34.—ON DIABETES AND ITS TREATMENT.

By Dr. H. Bence Jones, A.M., F.R.S.

Each grain of starch that is taken as food, when acted on by the saliva or pancreatic fluid, is converted into sugar and becomes a source of fuel or power and ultimately passes out of the body as carbonic acid and water.

In diabetes the starch is still changed into sugar; but in the circulation and in the extravascular or parenchymatous structures the further change is partly or wholly stopped; more or less of the sugar remains as sugar, part reaches the kidneys and is thrown out in the urine, part exists in the structures unchanged; and thus the system is deprived of one part of that conversion of latent into active force on which the nutrition and power of the organs depends.

In the body or out of the body, when once a series of chemical action is set up, the series tends to become continuous; it propagates itself, unless a change of conditions occurs. For example, grape juice will remain as grape juice, if kept from the action of other substances. Under well-known con-

ditions it will ferment, and continue fermenting, as long as sugar, and ferment, and the proper temperature are present. If the conditions are slightly varied, variations in the products will be observed. The series of actions will not pass rapidly from grape juice to alcohol, to vinegar, or to putrefaction; but according to the conditions one action or another action will be set up or will continue as long as that action is possible.

In health and in diabetes the sugar is subjected to different conditions. In health the chemical changes are propagated and continued to their full extent; whilst in diabetes the conditions necessary for perfect change are wholly or partly wanting. These conditions, on which the changes depend, are chiefly three. 1st. The proper temperature; 2d. The presence of a not immoderate quantity of sugar; 3d. The presence and activity of a ferment.

On each of these conditions a few words are necessary.

First, the internal temperature of the body, under all circumstances, is subject to so small a variation, that at present there is no proof that such a reduction can occur as can check the chemical actions in the human body, though under certain circumstances small animals can be exposed to cold so that sugar may be made to appear in the urine.—(*Proceedings of the Royal Society, December* 15, 1864.)

Secondly, by the most careful investigation, I have satisfied myself that Professor Brücke is right in saying that healthy urine contains sugar—that is, the amount of sugar derived from the food, or from the changes going on in the textures, is at all times more than can be entirely changed in the circulation and in the tissues. If, then, a considerable amount more is thrown into the circulation, some of it will pass out in the urine. Thus an injection of sugar into the rectum when absorbed causes temporary diabetes; so, also, large quantities of sugar, fruit, and farinaceous food tend to produce diabetes. Thus, also, if Bernard's amyloid or glycogenic substance were produced in excess in the liver, diabetes would result.

Thirdly, the ferment, the prime cause of change, has not been yet insulated. Whether it be the albuminous substance of the saliva or pancreatic fluid, or some other of the many albuminous substances in the blood, it requires for its action heat, the presence of alkali, and an undisturbed circulation of the changing fluid.

With regard to the effect of alkalies, something will be said when I speak of the treatment of diabetes: but concerning the influence of unobstructed chemical action some remarkable physiological experiments must here be mentioned, because they not only furnish the most striking illustrations of the relations of mechanical to chemical disease, but because they also help to account for the effect of sudden mental shocks and nervous disturbances, which do so much harm to the diabetic patient.

Bernard made the great discovery that the mechanical injury of the floor of the fourth ventricle alters the chemical actions going on in the body so as to cause sugar in excess to appear in the urine. This is a most remarkable demonstration of the relation which subsists between mechanical and chemical actions in the living body.

M. Schiff, in his "Untersuchung uber Zuckerbildung" (1859), states that this diabetes results from injury of the vaso-motor nerves of the abdominal organs which arise from the thalami optici and the crura cerebri—*partent des couches optique et des pedoncules cérébraux;* and he also shows that by injury of other parts of the nervous system temporary or permanent diabetes is produced.

Thus cutting the posterior roots of the nerves arising from the cervical portion of the cord, leaving the anterior roots untouched, causes temporary appearance of sugar (irritative diabetes): whilst by division of the anterior roots on a level with or above the fourth cervical vertebra, permanent (or paralytic) diabetes results. These injuries, he says, cause an irritation or a paralysis of the vaso-motor nerves. The vessels of the liver become dilated and distended; and as a consequence there is an excessive secretion of sugar by the liver.

The permanent diabetes may be thus occasioned: but it is far more probable that the temporary diabetes is owing to the mechanical injury of the

nervous system so affecting the vessels that the chemical changes are interrupted. The same phenomenon is seen in every fermentation. The chemical action is temporarily stopped when the fermenting fluid is disturbed by any sudden change.

These conditions of change may in diabetes be affected singly or all simultaneously. When the affection reaches to the extent of stopping the chemical change in the vegetable or animal sugar in the body, diabetes, intermittent or permanent, is the result.

That the disease is only a little way distant from health is shown by the existence of sugar in the urine in the healthy state; so that a small quantity of sugar in the urine is no proof of disease; and here, as elsewhere, there is no defined limit where health ends and disease begins.

Diabetes is the arrest of a healthy state; an increased quantity of sugar appearing in the urine, because the actions that constitute health are stopped.

In perhaps half the cases of diabetes the arrest of change in the food-sugar constitutes the complaint. This is proved by the fact that when a strict antifarinaceous diet is observed the abnormal amount of sugar ceases, and the patient is well as long as he keeps to the strict diet. When this fact was published in 1806 by Dupuytren and Thenard, they used these words:—"Que le traitment qui consiste surtout dans un régime purement animal a le même degré d'efficacité que le quinquina dans lès fièvres intermittants."—(*Annales de Chemie, vol.* 59, p. 45.)

But it was soon found that cases occurred in which a strictly antifarinaceous diet, consisting of animal food and water, did not stop the sugar in the urine. For days and weeks not a grain of vegetable starch or sugar may be taken as food, and yet sugar in excess will exist in the urine. Whence does this sugar come? It must either be taken in the animal food or it must be produced in the body.

Previous to Bernard's discoveries, this production of sugar when no vegetable food was taken admitted of no explanation. Now, the discovery of the amyloid substance in the liver, and of inosite in the muscles, lungs, brain, and other organs, gives a full solution of the difficulty. In the chemical changes going on in the body, animal starch is formed in the liver, and in other organs as in the prostate gland, and sugar in the muscles; not as in vegetables, by the fixation of carbon and the decomposition of water by the action of light, but more probably by the gradual splitting up of the higher organic compounds of the different organs whilst performing their functions.

The good effect of an antifarinaceous diet in some cases, and its want of effect in others, marks the two great divisions of the disease—the two stages in diabetes. In the first stage vegetable sugar alone ceases to go through the healthy chemical changes, whilst the animal sugar is entirely changed; in the second stage, animal sugar as well as vegetable sugar are more or less unchanged. It may be concluded that animal sugar is more readily changed than vegetable sugar; and when in diabetes an antifarinaceous diet has no marked effect, then the conditions of change of the sugar in the body are furthest from the healthy state.

There is sufficient evidence that diabetes does not always progress from one stage to the other. The disease may halt anywhere in its progress, and remain stationary for years, or get better or worse. A gentleman has consulted me for the last four years occasionally; he is now fifty-seven. When I last saw him the specific gravity of the urine was 1020, about three pints in twenty-four hours, containing about four grains of sugar to the ounce. He had been diabetic for twenty-nine years, during which time he married, and has now healthy children grown up. When he takes care about his diet, the sugar and symptoms vanish, but when careless, the symptoms return, and the sugar can be found in the urine.

Diabetes, then, in its mildest or first form, is the loss of power to change the sugar of the food; in its more advanced or more intense form, it is the loss of power to change the sugar produced in the organs and textures of the body, as well as in the food. The changes in the animal as well as vegetable sugar are arrested.

[Almost every *remedy in* the Pharmacopœia has been tried with the view

of arresting the excretion of sugar, but none has any constant effect. Meanwhile, the effect of diet is far beyond that of any known remedy.]

An antifarinaceous, or, in other words, an antisaccharine diet will remove the sugar from the urine, and stop all the symptoms of the complaint in all those cases in which the power of consuming the animal sugar remains unaffected.

Even when the consumption of the animal sugar is imperfect or impossible, an antisaccharine diet will lessen the thirst, the flow of water, the dryness of the mouth, and even the constipation, and check, though it may not stop, the waste.

The simplest formula for the diet may be thus stated. All animal produce, including fish, flesh, fowl, game, eggs, cream, and meat soup should be taken; and all vegetable food that contains starch, dextrin, and sugar should be avoided.

As generally it is of the utmost importance to shun the forbidden food, I shall dwell upon it first.

The vegetable substances that contain most starch, dextrin, and sugar are rice, maize, arrowroot, sago, potatoes, oatmeal, peas, beans, bread, biscuit, toast, maccaroni, vermacelli, and all confectionery.

Fruits are even worse than vegetables. Apricots, plums, peaches, cherries, pears, gooseberries, are nearly as bad, and some worse, than rice and maize. Stout, porter, and ale, cider, port, Madeira, champagne, and sherry are more or less highly saccharine; cocoa and chocolate contain near 20 per cent. of starch and dextrin naturally, and more is often added.

The harm of each of these substances may be determined by the amount of starch, dextrin, and sugar they contain.

The following table, in which the fruits and farinaceous vegetables are taken as perfectly dry, will answer many questions regarding the diet of a diabetic patient:—

	Amount of Starch, Dextrin, or Sugar.
Ripe dry Apricots	about 93 per cent.
,, Plums	,, 92 ,,
,, Peaches	,, 86 ,,
,, Cherries	,, 85 ,,
,, Pears	,, 84 ,,
,, Figs	,, 79 ,,
,, Gooseberries	,, 37 ,,
Dry Rice	,, 90 ,,
,, Maize	,, 88 ,,
,, Arrowroot	,, 77 ,,
,, Potatoes	,, 76 ,,
,, Oatmeal	,, 70 ,,
,, Peas	,, 67 ,,
,, Beans	,, 67 ,,
,, Bread	,, 61 ,,
,, Milk	,, 21 ,,

Stout	about 45 to 64 grs. of sugar per ounce.
Porter	,, 23 ,, 40 ,, ,,
Ale	,, 12 ,, 45 ,, ,,
Sweet cider	,, 18 ,, 44 ,, ,,
Port	,, 16 ,, 34 ,, ,,
Madeira	,, 6 ,, 66 ,, ,,
Champagne	,, 6 ,, 28 ,, ,,
Sherry	,, 0 ,, 12 ,, ,,

If dry rice contains 90 per cent. of starch, dextrin, and sugar, and potatoes contain 76 per cent. of starch and dextrin, and if all the starch and dextrin pass off in the urine as sugar, it is evident that to forbid potatoes and to allow instead an equal quantity of rice, is simply ordering the quantity of sugar from this source in the urine to be increased 14 per cent., thus adding to the thirst *and waste*. Or if half-a-pint of port wine is forbidden containing from 128 to *272 grs. of sugar*, and if a pint of porter or stout is ordered which contains

from 368 to 960 grs. of sugar, it is clear that the quantity of sugar in the urine will thus be increased from half-an-ounce to an ounce and a-half daily.

Before passing to the best diabetic diet, there are two substances—bread and milk—which require to be further mentioned here.

Ordinary bread contains water, salts, starch, dextrin, sugar, and gluten.

If the salts, starch, dextrin, and sugar are washed away, the gluten remains, which, in a chemical point of view, is as unobjectionable as meat.

In making the different kinds of gluten bread this washing is more or less perfectly performed.

In the following analysis the water, starch, dextrin, and sugar were determined, the residue or difference was taken as gluten :—

	Water.	Starch and Dextrin.	Sugar.	Gluten.
Ordinary bread .	36	49	1	23 per cent.
Aerated bread .	37	42	2	19 ,,
Gluten bread from Toulouse . .	2	16 to 44	0	82 to 54 ,,
Dried bread .	2	60	1	37 ,,

The best washed gluten bread contains less starch than bran cakes or any brown bread; Dr. Pavey's almond bread is free from all starch, but the almond flour must be well washed to remove the sugar and dextrin, of which 10 per cent. are present.

Badly-washed gluten may be made into dry bread containing bulk for bulk more starch than ordinary undried bread; thus an excess of gluten bread may keep up the amount of sugar in the urine, and prevent an improvement in the symptoms.

With regard to milk, one hundred parts may be taken to contain three parts of lactine, or about half-an-ounce of lactine exists in a pint of milk. If all of this animal sugar was incapable of being consumed in the system, milk would be nearly as injurious as an equal quantity of many wines, and the best sweet ale; but experiment shows that this sugar is often partly or entirely consumed.

A diabetic patient lived upon butchers' meat alone for two days. The quantity of urine passed was forty ounces the first, and forty-two ounces the second day; specific gravity, 1029·0. No trace of sugar could be found. He then took milk for two days; the first day eighty-eight ounces, the second day ninety-nine ounces. The urine was forty-five and a-half ounces the first day; specific gravity, 1024·1: and the second day sixty-nine ounces; specific gravity, 1011·9. Sixty-seven grains only of sugar were passed the first day, and twenty-three the second day.

At a more advanced period of the disease, when strictly animal diet did not cause the sugar to disappear from the urine, milk alone was again taken.

The first day 138 ounces of milk were drank; the urine was 61 ounces; specific gravity, 1030·5. The second day 88 ounces of milk were taken; the urine was 34½ ounces; specific gravity, 1027·8. The quantity of sugar in the urine was 854 grains the first day, and 414 grains the second day. At this time, on animal diet alone, one day 280 grains of sugar were passed, and the next day 600 grains.

Thus milk is more or less injurious according to the stage of the complaint. When animal sugar can be consumed milk is comparatively harmless.

In the advanced stages of the complaint curds or sour milk, with the acid nearly neutralized by potass, soda, or ammonia, would be as unobjectionable as gluten bread.

There are two great ends to be gained by the use of medicines in diabetes.

Of these the first and most important is to promote the oxidation of the sugar; or, failing this, to compensate the system for the loss of saccharine fuel, and the consequent loss of power and nutrition by promoting the supply and oxidation of the oleaginous fuel.

Of all the medicines that can be given for the promotion of the oxidation, whether of sugar or fat, in the body, iron and alkalies are the most energetic; and hence, beyond all other remedies, iron or ammonio-citrate of iron with excess of ammonia, or with other alkalies, are the best medicines for diabetes.

The iron may be given in potass or Vichy or in Fachingen water, and that preparation which confines the bowels least is most to be preferred. Hence the potassio-tartrate and Griffiths' mixture are often useful.

Alkalies without iron promote oxidation. This is very evident in the copper test for sugar. M. Miahle even has stated that alkalies are the specific for diabetes; without doubt they are of importance in promoting oxidation. Soda or potass may be given in the caustic state or as carbonates. Carbonate of ammonia in ten, fifteen, or even twenty-grain doses thrice daily in any gaseous mineral lessens the thirst.

Long since Professor Graham tried the phosphate of soda, with three equivalents of soda, on the ground that the blood required this substance and could not get it in the animal food. I have not found any great advantage from its use.

Vichy water, more particularly the Celestin, is recommended by M. Bouchardat. It contains between eighty and ninety grains of carbonated alkali and alkaline earths in a quart of water. The Hospital spring contains about ninety-six grains, the Grand Grille ninety-two, the Hautrive eighty-nine, and the Celestin eighty-five grains to a quart of water.

Carlsbad Sprudel water and the more aperient Muhl spring is highly praised by Dr. J. Seegen. It contains about one-fourth or less of the alkali of Vichy water, but half a drachm or more of sulphate of soda in each quart gives an aperient action which the common salt of Vichy water rarely possesses. Marienbad Kreutzbrun water is twice as aperient, but rather less alkaline than Carlsbad Sprudel. Fachingen water has one-third or more of the alkaline power of Vichy. Seltzer water one-sixth.

Besides alkalies some animal substances are thought to promote change in the sugar in diabetes. Of these rennet and pepsine may be mentioned. In 1852 Dr. James Gray published some remarkable results in the 'Edinburgh Monthly Medical Journal,' p. 396, but I am not satisfied that rennet is very useful in diabetes. It is an albuminous substance in a state of change, and, therefore, it exactly fulfils the conditions required in the undiscovered specific for diabetes. It should be well washed with water to remove the adhering sugar and dextrin, and it should be given on an empty stomach, so as to enable it, if possible, to act on the sugar in the blood rather than on the sugar in the food.

Pepsine is another albuminous substance in a state of change. Its action in the stomach is to help the solution of the albuminous food, but when it passes into the blood it might be the animal diastase that carries on the change in the sugar. With this idea it has been given in diabetes, but with no satisfactory result; although many patients have said they were better whilst taking this remedy, yet I have never found the sugar diminish under its use. It usually helps the action of the bowels.

Lately oxygen gas has been tried by Dr. Richardson in diabetes. It would be spendidly simple if this were the specific for diabetes. This view implies that a deficiency of oxygen is the cause of diabetes, but the chemistry of the disease does not admit of this explanation. There must be a much more complex chemical error than a want of oxygen, and it is far more probable that no deficiency of oxygen exists, but that it does not act because the proper animal diastase is not present.

Vegetable and animal oils and fats constitute important remedies in diabetes. Of all these, cod-liver oil and cream are most frequently used. The following case may be taken as an instance of the amount of cod-liver oil that can be given:—

A man, aged twenty-four years, was admitted into St. George's Hospital, having lost two stone in weight during eight months. He passed seven quarts of urine daily. He remained under treatment for a month, during which time he was on animal diet and cod-liver oil. He began with half an ounce daily, and this was gradually increased up to eight ounces. The quantity of urine fell to two pints and a-half, specific gravity 1030, and he increased in weight from 8 st. 8 lb. to 9 st. 1 lb.

Cream may be given in any quantity until the tongue begins to be coated, *then it soon* disagrees, and the stomach refuses to take it, or rejects it when *taken.*

Fats, combined with alkalies, as soaps, are more ready to undergo oxidation than when the glycerine is unseparated. Pure glycerine is, however, often very useful as a substitute for sugar in tea and in other liquids.

To lessen the thirst and the craving for food opium is very useful. The alkaloids of opium diffusing out of the blood in contact with the nerves of the blood-vessels cause contraction of the capillaries; this affects secretions, so that the quantity of urine, saliva, bile, and intestinal secretion is greatly diminished. The drain of urine and the desire for food are thus checked, but the increased constipation and dryness of the mouth almost, and in some instances quite, counterbalance the gain obtained by checking the flow of water. By the use of very small quantities of opium, as five or ten grains of Dover's powder, or five or ten drops of laudanum once or twice daily, the thirst and excessive flow of urine may be stopped, and the constipation may not be excessively increased.

The second great object in the treatment of diabetes is to remove the constipation.

The excessive determination of water to the kidneys causes increased dryness elsewhere, hence the mucous membrane and the skin are harsh and dry; probably also the production of acid being greatly diminished by arrest of change in the sugar, the healthy secretion by the intestines and skin cannot take place.

Notwithstanding the amount of food eaten, the action of the bowels usually is very difficult. All saline aperients increase the thirst and pass off by the urine. Magnesia from the absence of acidity is usually inactive. Castor oil is by far the best aperient, when it does not nauseate, then capsules containing castor oil with minute quantities of croton oil are most efficacious. Compound extract of colocynth with jalapine, scammony, or gamboge, or podophilline will act when oil cannot be taken. Mercury when soluble combines with the albuminous matters with which it comes in contact and sets up increased chemical action. The nutrition of the parts to which it is directly applied, or carried by absorption and diffusion, is so altered that increased action, inflammation, and ulceration are produced. This altered nutrition has no influence on the oxidation of the sugar or fat, and hence diabetes is not benefited by mercury. Calomel may be used as an aperient, but it has not any advantage over other chemical or mechanical irritants to the mucous membrane of the bowels, and promoting a more rapid change in the albuminous structures of the textures causes a feebler nutrition to take place.

In extreme cases the constipation of the bowels becomes the most serious symptom. The chemical disease leads to a mechanical difficulty amounting almost to an obstruction of the bowels. Strong chemical irritants are required to excite the muscular action, sometimes even "croton oil" is necessary. When the bowels do act the prostration of the strength is sometimes alarming: so little power seems to be set free in the body that any extra expenditure seems to deprive the heart of the force necessary to carry out the circulation, in the same way as a fatiguing journey to London for advice in extreme mental anxiety will bring a prostration of strength from which with difficulty recovery takes place.—*Med. Times and Gazette, Jan.* 21, 28, *and Feb.* 4, 1865, *pp.* 55, 81, 109.

SURGERY.

DISEASES OF THE BONES AND JOINTS, ETC.

35.—ON THE IMMEDIATE TREATMENT OF FRACTURE BY FIXED APPARATUS.

By JOLLIFFE TUFNELL, Esq., Surgeon to the City of Dublin Hospital.

[Mr. Tufnell gives a table showing the practice in ninety of the principal hospitals of Great Britian. He constructed it from information supplied by the house surgeons of the respective places. An analysis of this table gives the following result.]

The various materials employed for the purpose of making fixed apparatus for the treatment of fracture of the leg are—starch—starch combined with pasteboard or gutta-percha—gum and chalk—plaster of Paris—plaster of Paris and gum—gum only—dextrine and glue; and the relative degree of frequency with which each one is employed will be exemplified by the following detail:—

Starch, with or without side splints, in	57	Hospitals
Gum and chalk, in	18	"
Plaster of Paris, alone, in	6	"
Plaster of Paris and Gum, in	1	"
Gum, only, in	4	"
Dextrine, in	2	"
Glue, in	2	"
Total,	—90	"

In this long list of hospitals the period usually adopted for the employment of the starch or other fixed material has been stated in the majority of cases; and, yet in seven instances only out of the whole number do I find the custom of early putting up of the fracture by it resorted to. Good reasons must necessarily exist for this line of practice to be so general and uniform, and the danger or risk from constriction of the limb by the starch bandage, as ordinarily employed, must, I consider, be regarded as the cause of its primary rejection, and employment only after swelling has subsided, or partial union of the bones taken place. I have, myself, for many years in hospital and private practice, but especially in the latter, been in the habit of using a modification of the fixed apparatus, such as is not, I believe, described in any work, or otherwise generally known; for, out of all the replies which I have received, I find allusion to it made only by one gentleman, viz., Dr. Turner, Senior House Surgeon of Guy's. I purpose, therefore, briefly alluding to the mode of procedure here; and, I trust that I shall be enabled to make the plan of action intelligible to all. The material required for forming these splints is very generally available, consisting only of lint or old linen for the inner lining; strips of the same, torn three inches in width, and long enough to reach from the head of the tibia to the sole of the foot. To make the splint itself:—The white of eight eggs and half-a-pound of flour for the fixing substance. These are all that are required; each and every one of which is to be procured in almost the poorest home —*and as such*, this splint is readily available to the surgeon in country *practice*.

The main feature of difference between this mode of setting fractures and every other kind of fixed apparatus, is the construction of the splint in two halves, and the applying of the bandage, which is to form the same lengthwise, instead of circularly, thus avoiding all possible sources of constriction of the limb.

Supposing, then, that the fracture has occurred in a city, or wherever else the most desirable materials are to be procured, the surgeon sends for the following, and places them beside him before interfering with the patient, further than to strip him of his clothes and lay him upon a properly prepared bed, upon the side opposite to the fractured limb—the leg itself being supported easily on a pillow—and the fracture, as far as possible, reduced. The articles required are—a table for spreading the bandages upon, a wash-hand and small sharp-edged basin, eight eggs, and half-a-pound of flour, as before stated; a large iron or silver spoon, a large knife, three calico roller bandages, half-a-yard of Taylor's lint, and some hot water. The lint is first thrown upon the limb, from the knee to the sole of the foot, and cut roughly into the outline of the limb, of a size sufficient to tuck under the sole at the bottom, and from side to side of the leg.

This lint is placed in the large basin, and hot water poured upon it, so as to saturate it completely, and whilst dripping, and without being wrung, it is lifted out and put upon the outer side of the limb of the patient who is lying with the leg bent, and exactly in the same position as recommended by Pott, with this difference that the limb is on the inner instead of its outer side, by this measure insuring subsequent inversion instead of eversion of the foot. Extension and counter-extension are now made by the hands of two assistants, and the fracture set by the surgeon, who models the wet lint on to the limb with his hands, so that it is as closely applied as a stocking would be; any starting or spasm of the muscles is prevented by the assistants just laying their hands steadily upon the limb. The surgeon now breaks the eggs upon the sharp edge of the small basin, drops the yolks into it, and the albumen into the other, having first thrown out the hot water which was previously in it for wetting the lint; he then takes sufficient flour, according to the size of the eggs, and with the spoon beats it and the albumen into a homogeneous mass of the consistence of pancake. He next measures with the end of a roller the length of the limb, from the knee to the centre of the sole, and placing the forefinger of his left hand, pointing upwards, and the forefinger of an assistant opposite to him at the required distance, he winds the whole of the three roller bandages, one after the other, around the fingers, and then with a scissors divides them at either end; thus in a few moments obtaining the strips necessary for the splint, all of exactly the same and proper length. Each of these tails is now spread thickly with the egg and flour upon one side, and as spread, folded double, with the plastered surfaces opposed and laid aside. The object of this is to keep them moist, and in hot weather from drying; as soon as they are all done the surgeon commences, and having them handed to him one by one, he lays them on the limb, moulding them to it, layer after layer, from the innermost portion of the tibia within to the farthest point of the calf of the leg that can be reached behind, covering accurately the instep, the toes, sole, and heel, and not leaving a wrinkle or crease. The large knife which has previously been used for spreading the bandages with egg and flour, is now employed as a spatula, and taking a few of the tails of bandages which have been left unspread, these are passed beneath the limb, and one after the other brought around it so as to confine the wet tails to the leg, and not allow them to be raised from off it as they otherwise would be by the expansion of the heated air which becomes rarified within.

The limb is now *left uncovered* till next day; if covered the splint will not dry, but retains a kind of pasty condition, whilst if exposed to the air for twenty-four hours it will be as firm and as light as cardboard on the limb. Upon the succeeding day the patient turns over upon his opposite hip, and the same proceeding is repeated exactly as before, taking care that the lint lining goes well over the limb, so as to be in advance everywhere of the plastered strips, *which if allowed to* come in contact with the other portion

of the splint would intimately adhere to it, and cause difficulty in separation of the halves; this too is allowed to dry. Upon the succeeding morning the two half splints, or only the second half, as the surgeon may think fit, are removed, the edges trimmed neatly with scissors, the second half overlapping the other by at least half-an-inch, ensuring correctness, whilst all pinching of the integument is prevented; when the roller bandage is subsequently put on, the two portions of the splint, in fact, should glide, as it were, the one over the other. Nothing more remains to be done; a couple of fillets to confine them to the limb, or a lightly applied roller bandage are sufficient, and the patient may now lie in bed with his leg straight or bent, as he wishes, raised up upon a pillow, or slung, whichever way he fancies or feels it easiest; and after a few days may get up, supporting the extremity by a sling round his neck.

Subsequently, when the patients come to exercise and move about, and the size of the limb to decrease, the splints may be brushed over inside with hot paste (as also round the edges), and a piece of chamois leather, previously cut to the size and shape, moulded in to form a softer and additional lining.

These splints are very light, weighing, when dry, but a few ounces, yet from their extreme accuracy in fitting the limb, and evenness of pressure, most surprisingly strong.

The same material I also employ in the immediate treatment of fracture of the fibula, and, at a later period, in fracture of the thigh, used in the circular form; put on, however, in tails, imbricated one over the other, and not as a roller bandage. Thus applied it is, of course, necessary to slit it up prior to removal, and for this purpose I employ a blunt gorget with the handle reversed, using it as an ordinary director, and cutting down upon the grooved steel with any sharp-pointed knife. Soutin's pliers and all other kinds of scissors and dividers I have tried and found objectionable in use, causing more or less annoyance and pain to the patient from the pressure exercised upon the soft parts in making the division of the harder external case, whereas the gorget slips along upon the skin, whilst its broad, round, and polished surface, is cut down upon without the slightest motion or pressure, and is not noticed hardly by the individual.

Such is the mode which I myself now employ, and such the form of proceeding in reference to fixed apparatus that I think will, upon experience, be found to be the most satisfactory to the practising surgeon; but, as so many other modes of putting up fractures in starch, gum and chalk, gum, &c., &c., have been referred to in the table of hospital practice here given, I would add a few words in reference to each, and my experience as to the advantages and disadvantages of the several plans.

The condition of one individual with a fracture of the leg, put up in a properly fitting and well applied fixed apparatus is so incomparably superior to that of another with a similar fracture, treated in any other kind of splint, that whatever can tend to the removal of objections, needs, I feel assured, no apology. The sufferer with fracture of both bones of the leg, set in tin, wooden, or other ordinary splints, is, for a considerable time after the receipt of the injury, dependent, and unable to do anything for himself; the other is comparatively independent; and fractures treated in the way described, of the tibia only, of the fibula only, and both bones together, have turned out so satisfactory that I cannot but speak strongly in its favour. The union has been firmer, in the same number of weeks, than under any other plan. Why it should be so is evident from two reasons—firstly, the mobility of the fractured ends of the bone is reduced to nil, and the circulation of the limb is normally maintained—no circular constriction existing to produce venous engorgement, and interfere with arterial supply. The egg and flour splint, made as described, has this great advantage also, and whilst being considerably firmer and lighter than the starch or other material, as ordinarily applied, it requires no padding for the protection of osseous prominences and projections. In the common mode of applying a starch bandage every point must be well protected by tow or cotton, or some other soft defence, and the *heel, tendo Achillis*, or instep, the metatarsal bone of the little toe and the *ball of the great*, the spine of the [illegible]ta tuberosity, the head of the fibula,

the patella, and condyles of the femur, all require padding to prevent ulceration, whilst the longitudinal splint receives its own indentations so accurately as to press unduly on no spot, and retains its position *in situ* so completely as to maintain its exact position when once applied, thus obviating or rather preventing the restlessness which accrues from pain caused by the pressure of a splint upon a projecting point of bone, and which so often irritates the patient and causes him to undo and loosen the fracture for the purpose of obtaining relief. Its stiffness upon drying, too, is such as to obviate the necessity of resorting to any supplemental support from pasteboard, gutta percha, whalebone, or any other of the stiffening matters usually employed as adjuncts to starch, and which all, more or less, press upon the limb. White of egg and flour, therefore, used as directed, are the substances that, in combination, seem to be most desirable in practice—they are everywhere obtainable, and at a very trifling cost.

Gum and chalk are the next best materials—indeed, as for stiffening purposes, they will equally answer as the former. They are daily becoming more and more used, and have superseded starch in very many cases; whereas, in no instance have I found starch to have replaced gum and chalk, when once introduced.

The general mode of using this combination is to rub up equal parts of powdered gum Arabic and prepared chalk, or one part of gum to two of chalk, with cold water, to the consistence of thick cream, or white paint, at the time of use, and at the setting of the fracture; whilst others keep the mucilage of gum Arabic ready made, and when wanted pour it upon finely powdered prepared chalk, and rub it up to the consistence desired. The general directions given for its employment I subjoin, and the special plans adopted at some of the hospitals also annex.

GUM AND CHALK.

Mode of Applying.—The mixture should be made of equal parts of prepared chalk and gum Arabic in powder, rubbed up together, with sufficient water to make it as thick as good cream, for unless made tolerably thick it will not answer well.

The limb is now to be evenly surrounded with wadding, or cotton wool, and then firmly bandaged with an ordinary roller from the toes to the knee. This bandage is then to be well smeared over with the mixture, and next a bandage, well soaked in the mixture and then rolled, is to be applied; this is again smeared over with the gum and chalk, and the whole left open to dry.

If it is desired to make the casing more than usually strong, another dry bandage is put over the last, which at every turn is smeared over with the mixture; when dry the bandage ought to look quite smooth and white.

This bandage looks better and keeps cleaner than if a simple dry bandage is placed externally. The bandages should be applied very evenly.

In saturating the bandage with gum and chalk, it is a good plan not only to soak the roller bandage in the mixture, but also to brush it into the bandage as it is rerolled, with a stiff shaving brush.

By immersion of the limb in warm water the whole can be removed if desired.

The following are the plans adopted in the undermentioned hospitals, viz.:—

Birmingham.—Equal parts of gum and chalk rubbed down into water, to a proper consistence.

Leicester.—A simple roller is first applied, then strips of stiff brown paper, about two inches wide, smeared with a solution of gum and chalk in equal parts, made to sufficient consistence, placed both longitudinally and circularly over the fractured parts, and outside these a roller bandage saturated with the same.

This bandage is used at once in recent simple fractures of one or both bones, if transverse, but not if oblique.

West Liverpool.—*Put on a dry roller*, then mould pasteboard side splints;

a dry bandage is placed over these to fashion them to the limb. The gum and chalk (of the consistence of thick cream), is then well rubbed into the bandage with the hand, and allowed to dry.

If extra firmness be required, another bandage is applied over this, and treated in the same manner, the heel being covered with the bandage, as it prevents all complaints of soreness and existence of swelling.

Middlesex.—Cover the limb with a thick layer of cotton wool; over this a simple bandage, next a mixture of gum and chalk, with a few strips of bandage extra, smeared in the mixture, placed over the seat of fracture.

STARCH.

Starch is, of all the materials used for fixed apparatus, that most commonly employed; but only so, I consider, from the two previously mentioned not having been tried. It is, in my opinion, far inferior to either gum and chalk, or white of egg and flour; but, as being in such general use, I append it with the modes of application in several institutions.

Aberdeen.—Soak a roller in boiled starch the thickness of common mucilage; apply it from the extremity up to prevent swelling, in the same manner as any ordinary bandage—two or three layers; next apply two softened pasteboard splints, one on each side, confined by a dry roller; these splints are to be kept on for a few days, then removed, and the fracture left in the starch only.

Birkenhead.—A common roller is first applied to the limb, then five or six pieces of pasteboard, from six to nine inches long, and one inch and a half in width, soaked in hot water, are to be accurately moulded to the limb, around the fractured part; a thin layer of cotton wool, or a piece of lint, being placed under the ends of the pasteboard to prevent any undue pressure and possible ulceration of the skin.

Over these strips of pasteboard a wet roller is placed, formed of cheese cloth or open canvas, its porous nature absorbing more readily, and in greater quantity, the starch, which is gently rubbed into its pores, the starch being as hot as the hand can bear it, and as stiff as it can be well used.

Canterbury.—Apply the starch bandage in strips, enclosing the heel; then a common roller, and again starch; then bandage, and leave it to dry.

Chester.—Well soak a bandage in thick starch; then roll up for use; apply the pasteboard splints one on either side, and bandage over all.

Dorset.—A bandage is unrolled and wetted, then soaked in warm starch, and applied as an ordinary roller.

Essex.—Pad the bony prominences with raw cotton; apply a common dry roller; next softened pasteboard splints on either side, and over all a well-starched roller bandage.

Glasgow.—Wrap the limb in a layer of cotton wadding, or a dry roller; then draw a bandage through starch, so as to saturate it; apply this as a roller over pasteboard side splints, and a dry roller over all.

Hereford.—Bandage the limb and foot to above the knee; smear over with starch; pad the projections of the knee, tibia, and ankle, with tow; three wide strips of pasteboard, well softened in hot water, are to be applied; the posterior passing over the heel and terminating in the sole of the foot: the whole is then firmly bandaged and starched, a second layer being applied if necessary.

Portsmouth.—The bandage should be only two inches wide, making no reverse turns.

St. Bartholomew's.—First apply a flannel bandage to the leg; next a dry calico roller, and then lay on the starch with a brush.

Staffordshire.—First put on a bandage wet; then rub this over with starch; next a calico roller soaked in starch; rub this well with starch on the surface, and leave to dry.

University College.—Apply a roller evenly as far as the knee; next two splints of coarse strong pasteboard, and shaped to the foot and leg, so as to grasp it, with their edges carefully bevelled—these having been first well *soaked in hot* water. Starch of medium consistence is rubbed on the outer *side of these splints*, which are then moulded to the limb by the hands of an

assistant. Whilst the bandage is being applied starch is well rubbed into each turn of the roller, and afterwards plenty of starch is rubbed into the whole case.

If much strength is required a second bandage is similarly applied; when quite dry the case is cut open along the line of junction of the side splints, and, if necessary, the edges are pared, and then firmly brought together by a dry roller.

The bandage next the skin is dispensed with by Mr. Erichsen, who substitutes a small layer of cotton wool, so that there is less risk of strangulation taking place; and it also allows a fracture to be put up earlier than it could otherwise be done.

The objection to it is, that in very oblique fractures there is less certainty of obtaining an accurate coaptation of the parts.

Westminster.—Roll a bandage through a moderately thick solution of hot starch; let it stand an hour; then apply it to the leg; next smear a thick solution of starch evenly over, and allow it to dry. If not stiff enough to satisfy, another bandage is to be similarly placed over it when *quite dry*, but not before.

Chichester.—A dry roller; several splints of stout mill-board, softened in boiling water; then a bandage, thoroughly starched; and over this a dry roller.

Northampton.—First encase the limb in a thick layer of cotton; next a gutta percha splint, softened in boiling water, is to be moulded upon each side; a roller bandage is then to be put on in the ordinary way, rubbing starch in between each fold, made as thick as possible, and over the surface, so as to permeate it thoroughly.

PLASTER OF PARIS.

Plaster of Paris comes after gum and chalk in frequency of use. It is firm, and sets quickly; but is not so easily removed from the limb, and is very liable to crack. In using it the calico or other material employed for the bandage should be of very open texture, so that the plaster can be well worked into the meshes of the stuff, and not merely lie applied to its surface. It may be rubbed dry into the bandage, which is then rolled up, and wetted as it is applied to the limb; or the whole bandage be dipped entire into cold water, saturated, and then put on. Other surgeons use it thus:—They make a mixture, with cold water, of finely powdered good, dry, and *fresh* plaster of Paris, to the thickness of cream, in a basin; then unroll the bandage from a basin of cold water into the basin in which the plaster of Paris is contained; roll the bandage again in the solution of plaster of Paris, and apply it quickly to the limb. The names of some of the hospitals who adopt it, and the modes which they adopt, are subjoined.

Devon and Exeter.—First put a single dry roller bandage on the leg, and then another bandage previously rolled into the dry finely powdered plaster, well rubbed into it, and soaked in water for a short time before being applied; after being put on a moderate coating of wet plaster is to be smeared over all.

Lincoln.—The bandage, with the plaster well rubbed into its meshes, is to be applied to the limb dry, and then wetted through, more dry plaster being rubbed on the bandage, and more water; then a tolerably thick layer of plaster is to be put on, and allowed to dry.

Liverpool.—Dr. Armstrong, of the Northern Hospital, Liverpool, puts up oblique fractures of the tibia at once, as follows:—A competent assistant holding the limb in accurate position, the surgeon takes a piece of muslin long enough to reach from above the knee behind round the heel to the toes; and broad enough to enclose the limb, with the exception of an inch and a half over the line of the tibia.

Having first tested the plaster to see how long it is setting (and if longer than five minutes, hot water, or a little salt, must be added), mix a good quantity of plaster, rather thin; soak the muslin in it, and apply it accurately to the limb, coating it well with the plaster of Paris behind and all over.

The assistant *must hold the limb* until the plaster sets. In this splint

there is vent for any swelling; and, as the plaster is as firm as stone, no change of position can take place.

Compound fracture, when the wound is in front, can also be treated in this way; its cheapness is also a recommendation.

South Devon.—The dry powder is firmly and carefully rubbed into a roller of proper texture, with open pores, and of sufficient length; this is then accurately applied to the limb, each turn of the bandage being damped by cold water; and, subsequently, a layer of the powder is sprinkled over all and moistened.

Taunton.—Mix the plaster in a basin, and pass the bandage through it; apply the roller, rubbing more plaster into the pores of the bandage.

GUM.

Gum and dextrine are but scantily employed, as compared with the substances already spoken of. Glue, too, is occasionally used. Gum seems to be generally rejected in consequence of the number of coatings necessary to give to the splint the required consistence. Dextrine, because of its being difficult to procure, and more expensive than the others; and glue from its being less clean in its application. The hospitals in which the latter is occasionally employed are St. Vincent's and Dr. Steevens' in Dublin, and by Dr. Pentland in the Infirmary at Drogheda. Gum, when resorted to, is thus directed to be used.

Bedford.—Envelope the limb in a layer of lint; then bandage firmly; next apply a thick solution of gum Arabic; allow it to dry, and then put on a second and third coat of gum solution.

Reading.—Place a layer of cotton wool round the ankle; then apply a washed bandage from the foot to the knee, and coat with gum; then take some pieces of lint, about two inches broad, doubled three times; gum them well, and place them on each side of the leg for two-thirds of its length; then put on another bandage from the foot to the knee, and gum it well and leave it to dry; the gum will have to be so applied about three times before it is hard enough.

St. George's.—A dry roller is first applied; then pasteboard splints; and, lastly, a gum bandage outside all.

St. Thomas's.—Three or four strips of lint, about an inch and a half broad, are dipped in thick mucilage of gum and applied longitudinally along the leg; over this a wet roller bandage is placed, and at each turn a little mucilage is rubbed in; over the lint a second bandage is placed to give sufficient strength.

DEXTRINE.

Charing Cross.—Put a roller bandage on the leg; then, with a brush, plaster on hot dextrine, only liquid enough to soak through the calico.

To sum up; the principal points in connexion with the treatment of a recent fracture by the fixed apparatus are:—

1st. To adopt longitudinal strips instead of a circular bandage.

2nd. To make the splints in two portions, excepting in cases of fracture of a single bone.

3rd. If making the splint in one piece, to do so by imbricating the bandage in tails, and not using it in the roller form, as the turns in doing so cannot but more or less constrict the limb; besides, grating of the fractured ends of the bones cannot be avoided in the rolling up of the limb.

4th. In no case should the circular form of fixed bandage be adopted when the surgeon cannot inspect the limb within a few hours of its application.

5th. If constriction follow, and the limb requires to be freed, the splint should be slit up upon a broad director, such as a paper-knife, if better be not at hand, cutting down upon it with the point of a sharp penknife or bistoury.

6th. Never to use scissors or pliers when the last-mentioned mode can be adopted; and if any idea of using scissors be entertained, then to employ the longitudinal tape recommended by Séutin when forming the splint.

7th. To use gum and chalk of the consistence of thick paint, or flour and *white of egg* of the firmr of pancake, in preference to starch or

gum, as drying more rapidly and being lighter, and not needing pasteboard or other additional support.

8th. If starch should be employed, to use it hot and as stiff as it can be worked.

9th. To pad well all bony projections when starch is used.

10th. To *tear* the pasteboard employed as side splints so as to bevel the edges, and not cut the splints out with a knife, in order to prevent the risk of ulceration, as laid down by Gamgee.

11th. To select pasteboard of thick and porous kind, which will readily absorb the starch and water, and be easily moulded to the limb.

12th. If the pasteboard at hand be too thin, then to use two or three layers, smearing each of them, and applying them together in two or three thicknesses.

13th. To use open-textured material for the bandages, more especially when plaster of Paris is employed.

14th. To see that the plaster of Paris is fresh, finely-powdered, and dry, and if so, employ cold water in its mixing; but, if the plaster is not fresh, then to use tepid or warm water.

15th. To steady the limb with side splints if necessary, or sand bags, for the first few hours after the fracture is set, or until the first half of the splint (if made in two portions) is dry.

16th. If an opening for the dressing of a wound (as in compound fracture, or ulcer in disease of the joints, or for the application of leeches), be required, then to form it in the method recommended by Dr. Browne, of Galway, or the next mentioned, and not in the ordinary way of roughly cutting the portion out.

"Dr. Browne's plan" is as follows, viz. :—To place a piece of the thinnest zinc, the exact size of the opening intended to be made, over the site of the wound, or required surface, and over this to form the splint. When dry and hard to cut down upon the centre of this piece of zinc, with a sharp knife; and then, having come down upon it, to cut cautiously with a pair of scissors round and round, until the piece of zinc, which has acted as a shield to the soft parts, is set free. The next plan, or that adopted by myself, is to leave the opening in the formation of the splint thus :—After encasing the limb or joint with lint, to trace out, with the point of a pen and ink, a circle larger than the opening required; then, as each layer of bandage comes over this spot, to cut out with sharp scissors the requisite portion, and lay the cut edge nicely down to the margin of the space needed to be open; to continue to do this until the splint is completed, leaving the aperture with the lint lining at the bottom. This lint is now to be pinched up *in the centre* with a pair of dissecting forceps, and punctured like a hernial layer. The point of the scissors is then to be entered here, and the lint divided from the centre to the circumference, in a crucial form, reflecting the four angular flaps, which are then turned to the back, and fastened, with a little of the material used in forming the splint, to the margin of the aperture or foramen.

The opening made in this way is more evenly bevelled at its edges, and there is no secondary disturbance of the limb; but Dr. Browne's plan has proved most successful in his own hands, and I have to thank Dr. Browne for the suggestion, which may prove useful to the readers of this paper.—*Dublin Quarterly Journal, Feb.* 1865, *p.* 149.

36.—ON EXCISION OF THE KNEE-JOINT.

By Dr. John K. Barton, Surgeon to the Adelaide Hospital, and Lecturer on Surgery to the Ledwich School of Medicine.

[If the patient has reached forty years of age Dr. Barton considers that amputation should generally be preferred to excision, because a very great deal of reparative power is necessary to render a limb, after excision, a perfectly firm support. In a markedly strumous constitution, although no symptoms of *visceral disease* can be detected, excision should never be per-

formed, for inflammation in such constitutions, instead of ending with effusion of plastic lymph, usually furnishes a seropurulent fluid. In cases where the patients have been hard drinkers inflammation generally runs an unhealthy course.]

Turning now to the local conditions which indicate or contra-indicate excision of the knee, it is evident that any disease of the joint which has produced suppuration, is just to the extent to which suppuration has gone, unfavourable for excision. Not only does abscess, in or about the joint, indicate a constitution unfavourable to excision, but it is generally the result of extensively diseased bone; and when we have evidence of the ends of the bones being extensively diseased, I believe amputation is the only operation advisable, for two reasons—first, excision would most likely fail to produce a sound or useful limb—and, secondly, if solid union was obtained between the bones, the amount of shortening would be so great that the limb would be of little use, its questionable advantages (being five or six inches shorter than the other) by no means compensating for the six months additional time spent in recovery. Necrosis of part of the head of the tibia or abscess in this bone, do not constitute states suitable for excision, as in order to get healthy bone for union, two to three inches of the tibia must be removed, with which the piece necessarily removed from the femur, would make the limb too short.

Of the three kinds of diseases of joints as described by Brodie—viz., that which originates in the bone, that which originates in the synovial membrane, and, finally, that beginning in the articular cartilages—the latter is the form of disease which offers the most suitable conditions for excision, for here the bone is but secondarily and to a small extent engaged, and there is very little tendency to suppuration. In acute inflammation of the joint, with suppuration, arising either from accident or otherwise, incision into the joint, to evacuate the purulent contents, will be the only operation required, and will probably be followed by anchylosis. In severe injuries to the knee, either compound fractures, implicating the joint, or severe lacerated wounds opening the joint, when it may be thought impossible to save the limb complete, it does not appear to me that excision will be at all applicable—for if the nature and extent of the injuries render the recovery of the limb hopeless, they would seem to me necessarily to forbid excision, the shock of which must be at least as great as amputation of the thigh. The experience of military surgeons who have attempted this operation for injuries in the field, is not encouraging. It should be borne in mind, when considering the question of excision of the knee, that any case which does not result in giving the patient a perfectly *firm*, *straight*, and *healthy* limb, must be considered a failure. It is totally unlike amputation in this respect, which is always undertaken as a last resort, and is successful if the patient's life is preserved and the stump heals soundly. Viewing the matter in this light, it must be admitted, if the remarks I have made are well-founded, that the cases suitable for the operation of excision of the knee are rare. They will require—first, youth or a vigorous middle age; secondly, no constitutional disease, or signs of deteriorated blood; thirdly, the disease of the joint to be non-suppurating, and confined, or nearly so, to the articular surfaces. If from the cases which combine these conditions we subtract those of them in which a firm anchylosis can be obtained by rest and other suitable means, we have but few remaining in which the operation is called for; in these few, however, we have in excision the means by which we may not merely save the patient's life, but with his life give him back his limb; and it must be admitted that in doing this the operation well merits the encomiums it has received of being the most brilliant achievement of modern conservative surgery. The details of the operation itself, and the important after-treatment, seem now to be nearly perfected. All that we require to raise the per-centage of successful cases to three times its present ratio is a judicious selection of cases.—*Dublin Quarterly Journal, Feb.* 1865, *p.* 5.

37.—ON THE EXPOSURE OF ARTICULAR CARTILAGES IN WOUNDS.

By T. Holmes, Esq., M.A., Cantab., Surgeon to the Hospital for Sick Children, and Assistant-Surgeon to St. George's Hospital.

Some surgeons believe that to leave articular cartilage exposed in a wound is dangerous. This idea appears to be derived from the false analogy of wounds of the knee-joint, and of the symptoms attributed to inflammation of its cartilages. It is remarkable that the every-day experience of amputations performed through joints should not have shown that it is an error. A wound almost surrounded by exposed articular cartilage will heal without any bad symptom. Thus, in a case in which I excised the astragalus and scaphoid bones, leaving exposed in the wound the whole cartilaginous surface of the tibia and fibula above, the cartilaginous facets of the os calcis below, and those of the three cuneiform bones in front, all of these cartilages being natural, no unusual amount of inflammation took place, and the healing process, though it went on slowly, met with no impediment. In another case I had an opportunity of examining the stump of an amputation through the knee-joint, in a child between two and three years of age. The child died from constitutional causes forty days after the amputation; but the stump had healed seventeen days after the operation, with remarkably little inflammation; and the patella was found as freely movable in the stump as in the natural joint, nor was any trace of inflammatory degeneration of the cartilages noticed. Again, although the cases on record are too few to form the basis of a decisive opinion, it may be reasonably alleged as probable that a wound in which no bony tissue is exposed is more likely to heal more kindly and soundly, and is less liable to pyæmic infection, than one where a large surface of cancellous bone is laid bare, with all its cells cut open. How often we see such wounds sluggish and refusing to unite. How often the divided bone participates in the inflammation going on around it, softens, and finally becomes carious; or dies, and has to exfoliate. And if this is the case with healthy bone, when exposed and divided in operation, it is still more often the case with bone already softened by low inflammation.—*Lancet*, *Jan.* 21, 1865, *p.* 50.

38.—ON PENETRATING WOUNDS OF THE KNEE-JOINT AND THEIR TREATMENT BY IRRIGATION.

By Dr. William Newman, St. Martin's, Stamford.

[Many years ago Dr. Newman drew attention to this subject, narrating five cases of penetrating wound of the knee-joint, followed in every instance by speedy recovery. The following cases have occurred since that time.]

Case 1.—In January 1862, I was asked to see a groom (a man over fifty years of age), who had, twelve days previously, fallen from a haystack upon the cutting-knife, and had laid open, by a slanting incision on the outside, his right knee-joint. The man had been kept quiet in bed, and the injury had been treated by poultices, leeches, &c.; but he was suffering considerable pain, had had very bad nights in spite of frequent opiates, while the joint was swollen, the integuments around puffy and infiltrated, and a gaping wound was freely discharging pus and synovia.

Irrigation was recommended, and continued steadily for twenty-four hours; so securing to the patient a good night's sleep, the first since the accident happened. After that time, from disturbing causes, other treatment was determined on, and employed. A week afterwards (nineteen days from the infliction of the injury), irrigation was again resorted to, at the patient's own request; and the progress to recovery was steady and satisfactory. Sedatives were no longer necessary; the nights were good, and the wound healed. Constantly used at first, the water was, after some time, only allowed to drop on the knee when the joint became, even in a slight degree, hot and painful.

Case 2.—In May 1862, I saw a labourer, who had, two days before, sus-

tained a punctured wound of the knee. The left knee-joint was distended by effusion, and painful in some measure; but the symptoms were not of the same urgency as those usually met with. Possibly, the extreme *nonchalance* and indifference which the man strongly showed existed physically as well as mentally; and his nervous system may have been singularly insusceptible of external impressions.

Irrigation was suggested, and carried out for six or seven days. At the end of that time, all joint-irritation had subsided; the swelling, also, had quite disappeared.

Case 3.—The details of this I propose to give at length, since some of the main points are well illustrated.

W. P., aged forty, agricultural labourer, came to my house on August 24th, 1864, having walked a mile from his own house to a side railway-station, and thence travelled by rail; the transit had given him a good deal of pain.

On the preceding day, August 23rd, he had sustained a punctured wound of the right knee-joint from a hay-fork. The point entered on the inner side, half an inch below the level of the mid-line of the patella, and passed upwards and outwards directly into the joint. He had walked a mile or more to his house after the accident. The joint was soon swollen, and became very painful, so as effectually to prevent his having any sleep through the night.

At midday on the 24th, when I saw him, the knee was much swollen and the joint tense with effusion. Pain was constant, and increased by pressure; and there was a blush of redness on the skin. He was told to return home, to go to bed, and to keep up constant irrigation of cold water.

August 25th. He had had some sleep in the night, and said he had much less pain. The knee was still hot and swollen. The treatment was continued.

August 26th. He was doing well. He had a threatening of shivering in the evening, which was stopped at once by taking some ammonia; no feverishness succeeded. The joint was easy, but still swollen.

The irrigation was continued constantly until the 28th.

August 28th. The knee was less swollen, not painful, not hot. He slept fairly well. He was ordered to discontinue the dropping for an hour or two at a time; resuming it at once if any pain or heat should occur about the joint.

August 30th. The knee was much smaller; the patella a little thrown forward; otherwise, the external conditions differed very slightly from those of the uninjured joint. There was no pain on pressure; and very faint evidence of fluctuation. He was ordered to continue irrigation for three or four hours each day, with longer intervals between the applications.

Sept. 1. He was going on well. The swelling was even less than when he was seen two days previously. There was no heat, nor pain. He was told that he might cease the water-dropping unless pain or heat should come on; and also that he might sit up a little, supporting the limb in its whole length. (For this he had special and clear directions.)

Sept. 4th. He had a relapse; was not nearly so well. The knee was more swollen, and was distinctly hot to the touch. This was easily explained. He had been up daily, and had not used any irrigation since the 2nd instant; but had been seated (contrary to orders), with the limb extended, and resting by the heel only, on a low stool, so having no support behind the seat of injury. Further, as the limb, from this new position, began to ache, and the knee became hot, he had been applying for the past twelve hours cloths wet with cold water, *propriâ manu;* and to do this had, on his own showing, moved his trunk forwards every five or ten minutes to reach the basin of water. I put a firm splint behind the knee, retaining it in place by a figure-of-eight bandage, sent the man to bed, and requested him to resume and continue irrigation.

Sept. 5th. He was better; was not in so much pain. The knee was still hot.

For the next seven or eight days, rest and continuous irrigation were insisted on; and, so much was the patient in fear that mischief in the joint *might return*, that he used the water even too thoroughly by voluntarily *increasing the number of points* from which the water dropped. Hence,

he induced a heat of reaction, transitory, however, and not accompanied either by pain or swelling.

Sept. 15th. He was ordered to discontinue the use of water entirely, and to move very cautiously, still wearing the back-splint.

Sept. 26th. The knee was normal in the size and appearance. He could bear a little weight on it, and was now beginning to walk a little.

Oct. 12th. He walked well; had nothing to complain of, and would go to work the next day.

I would remark on this case that the first error was one of distinct omission on my own part. I ought to have applied a splint behind the knee in the first instance, and so provided against the possibility of any ill result from thoughtless movement. On this omission, in a measure, followed the relapse of Sept. 14th. More especially, however, was the recurrence of joint-irritation, due to want of care on the part of the patient. He failed to give to the limb, on first rising from bed, the support which he had been requested to insure; and hence the terminal points, hip and heel, alone having a resting place, the articular surfaces of the femur and tibia were in constant and close apposition at the knee, as the central point; these surfaces having been the seat of subacute inflammation but a few days previously. Again, too, the continual movement of the trunk necessitated by the choice of wet cloths in lieu of irrigation, destroyed the chance of rest to the injured part, and kept the articular surfaces of the bones constantly rubbing one upon the other.

No sooner, however, was complete rest insisted on, and cold thoroughly applied, than the threatening symptoms began to subside, and the progress to recovery was uninterrupted, though slow. There was still, on Oct. 12th, a shade of general thickening around and about the joint; but the movements were not impaired, nor was pain induced by extreme flexion or extension.

If cold be the appropriate remedy for any surgical affection, it is but a bare truism to maintain that it should be applied effectually. If this, then, is to be done in the case of some acute inflammation—of a joint, for example—the constant presence of an attendant will be imperative, who may apply thin coverings moistened with some frigorific or evaporating lotion to the part affected, and change these coverings at very frequent intervals, as they become either warm or dry. But to speak of such procedures as available or possible in everyday surgical practice would be absurd. How is cold then applied? Usually, a lotion is prescribed, and some material soaked in this is applied to the seat of injury. More frequently than not, and despite distinct orders, some three or four folds of material are used; thus more lotion is taken up, and the attendant's trouble is proportionately diminished. But the result is precisely opposite to that desired by the surgeon—the surface of the cloth very speedily steams with moist vapour, while the deeper layers are reservoirs of moist heat; practically, indeed, a poultice is concocted, without the efficiency of that time-honoured compound, and, this, too, employed under conditions where warmth and moisture are not desired.

This may be an extreme view; but I, nevertheless, claim for irrigation, or water-dropping, advantages which do not attend on the most careful employment of cold in the more ordinary forms. It is easily arranged; when once put in order, it is self-acting, even for hours; it is cleanly and effective. The absence or stupidity of an attendant will not interfere with the application; and for its employment, no appliances are needed other than may be found even in the poorest cottage.

A reservoir of water (iced, if possible), a jug, or some other convenient vessel, may be hung to some hook in the ceiling; or tied to the frame of a bedstead.

The distance, from the injured part, of the water supply, should not be less than three or four feet. If time be a great object in the treatment, or the symptoms of local injury be severe, a more speedy effect will result on the placing the jug at a greater distance above the bed on which the patient lies.

A few threads of lamp-cotton or Berlin wool, a narrow shred of flannel, or a strip of lint from the pocket-case, may do duty as a syphon. First well soaked in water, the shred must so be placed that one end lies at the bottom

of the jar, while the other one hangs outside at a lower level. A continual dripping will so be kept up until the cistern is exhausted.

The injured part should be kept immoveable; the knee-joint should be fixed by a back splint sufficiently long and well padded, this being kept in place by a figure-of-8 bandage. The turns of the bandage must not be too tight, for, when wetted, the material shrinks very perceptibly; nor is it necessary to cover the joint itself with the folds. From hip to ankle the limb should be thoroughly supported; and, to insure this, the patient should be kept in bed. Waterproof sheeting, or an oil-cloth covering from a table, should be stretched beneath the limb, so as to prevent the wetting of the mattress; and, by adjusting this covering, a channel may readily be made, so as to direct the waste water into some dish placed on the floor.

Next, as to the time during which this mode of treatment may be carried on. I have continued the dropping uninterruptedly for eight or ten days more than once, and without ill result; then, intermitting the flow for an hour or two at a time, I have continued the same plan for a second similar period of time—three weeks in the whole. The time usually employed is about seven or eight days; and this may be extended or shortened according to the special requirements of each case.—*British Medical Journal, Dec.* 3, 1864, *p.* 623.

39.—ON THE TREATMENT OF CARIES OF SINGLE BONES OF THE TARSUS AND METATARSUS BY EXCISION OF THE ENTIRE BONE.

By T. Holmes, Esq., M.A., Cantab., Surgeon to the Hospital for Sick Children, and Assistant-Surgeon to St. George's Hospital.

Several objections occur to the proposal of removing tarsal bones entire instead of piecemeal. They are as follows:—

1. That the operation is a very much more severe one, and involves a more protracted convalescence. 2. That it is a very difficult surgical proceeding, and cannot be effected without injury to important vessels, nerves, and tendons. 3. That it leaves the foot in a less useful condition than the stump after amputation at the ankle. 4. That the same results can be procured by what is termed subperiosteal resection; that is to say, by scooping the whole bone away piecemeal.

I will examine these objections seriatim. First as to the severity of the operation. It cannot be denied that the total extirpation of any of the tarsal bones, and particularly of the os calcis or astragalus, leaves a very large gap to be filled up, and requires very extensive incisions; while, on the other hand, a carious cavity can be gouged out through a simple cut of moderate length. If, however, the latter operation is to turn out useless, its simplicity will be a poor argument for adopting it. These excisions in the tarsus do, indeed, involve a long convalescence; the foot cannot, in ordinary cases, be placed on the ground in less than a quarter of a year; even if everything goes on as favourably as possible: but they are not liable to be followed by much constitutional disturbance, and in healthy children recovery may be confidently expected. Besides, during the latter period of the treatment the patient need not be confined to bed, nor even prevented from walking about on a wooden leg. Therefore the difference of a few weeks in the time which the respective cavities take to granulate is of little or no moment, if it be true that in the partial operations sinuses are usually left communicating with diseased bone, and that the disease will spread again after the child has been running about for a little while, or after slight injury, while after the radical operation a sound cicatrix will be formed which will remain sound and healthy during the rest of life.

The second objection is to the difficulty of the operation and the importance of the parts necessarily injured. It is true that, in order to remove entire one of the larger bones of the tarsus, large flaps have to be made, and a somewhat formidable dissection encountered; but in children, at any rate, *the operation need* not last very long, and is not very bloody. I do not think *that in any of the cases* referred to below of excision of the os calcis or astrag-

alas the child was under chloroform longer than a quarter of an hour; and in none of them did more than a single artery require to be tied. As to the parts which have to be divided, if the os calcis be removed by the operation described by Mr. Guthrie and devised by Mr. Hancock—*i. e.*, by making a flap out of the heel like the posterior flap of Syme's amputation—then the posterior tibial vessels and nerves and all the tendons which pass into the sole of the foot will necessarily be cut through, and much loss of power in the foot will perhaps result, if not some loss of nutrition also. But none of these parts need be injured. The incisions by which the os calcis was removed in the three cases noticed below do not involve the injury of any considerable vessel or nerve whatever. The posterior tibial artery need not be wounded, and was not wounded in any of those cases; nor was it, in fact, usually necessary to use any ligature at all. Excepting the tendo Achillis, no tendon is cut but those of the peronei, and even these might possibly be spared. The operation is perfectly simple even when the calcaneo-astragaloid joints remain entire, and when these are destroyed by the disease it becomes of course proportionally easier. But it leaves a large bag in which pus collects after the operation, and usually requires other incisions for its release. Possibly, passing a drainage-tube through the cavity at the time of the operation would be a useful precaution; I have found much convenience from the use of such a tube in the after-treatment.

The operation for removing the astragalus when diseased is a more difficult one than that for excising the os calcis, and involves an incision across the dorsum of the foot, in which the anterior tibial vessels and nerve must necessarily be divided, and most of the extensor tendons. I have only once performed this operation (and then, I must allow, the operation was of doubtful necessity); but in that instance, although the thickening over the bones was very great, I found it possible to spare one at least of the tendons —viz., that of the peroneus tertius, and there was slight power of moving the foot after the child's recovery. In this operation also there is a very extensive gap to fill up, and a long period of treatment to be gone through; but the experience of those numerous cases in which this bone has been dislocated, and afterwards removed, shows that after recovery a very useful foot may be expected. It was the accidental occurrence of a case of this kind, under my own care at the time, which emboldened me to undertake the removal of the whole astragalus from its bed in this instance. I found the operation not very difficult by a curved incision, extending nearly from one malleolus to the other, and reaching forwards as far as the front of the scaphoid bone. The chief caution required in the operation is to divide all the ligaments which hold the bone in place, and dissect it clean on all other parts before meddling with its posterior surface, where the groove exists for the flexor longus pollicis tendon, near which the posterior tibial vessels and nerve lie. In this operation also I found no vessel, except the anterior tibial, which required a ligature. None of the other tarsal bones present any difficulty, unless it should be desired to excise the middle or external cuneiform bone by themselves. But I am not aware that such cases ever occur. These bones, situated in the centre of the tarsus, are well protected from injury, and only suffer in affections of the bones around them.

The third objection which I have heard made to these operations is, that they leave the foot a less useful member than the stump of an amputation at the ankle. This is so perfectly the reverse of the fact that I should not have noticed the idea, had I not heard the objection from a surgeon for whom I have the highest respect, and did I not know that it influences the conduct of some justly celebrated operators—for instance, those of Mr. Syme's school. I have no hesitation whatever in saying that the foot, after the successful removal of the os calcis, or of the astragalus, and *a fortiori* of the smaller tarsal bones, differs far more for the better from the result of an amputation at the ankle, than it does for the worse from the natural healthy foot. In fact, in many cases of excision of the calcaneum there is very little difference from the healthy foot, except an elevation of the heel to the extent of about half an inch, and a consequent slight limp—not very perceptible if *the heel of the boot* be correspondingly thickened. The removal of

the astragalus is a more dangerous proceeding, yet most surgeons have had an opportunity of seeing that after the removal of this bone, in dislocation, the foot remains perfectly useful. Another very important consideration, on which too little stress is ordinarily laid, is that no apparatus is required after these operations. In a discussion which recently took place at the Pathological Society, on the subject of amputation of the foot at the ankle-joint, I heard, with great surprise, one of the most distinguished practical surgeons present advocate the removal of one or two inches from the bones of the leg; thus converting the operation into an amputation of the lower part of the leg, with no distinguishing feature except that the stump (being furnished by the skin of the heel) might be more than ordinarily able to support the pressure of the wooden leg. But the main recommendation of Syme's amputation would be lost in such an operation. For in these amputations at the ankle, although some special form of boot is needed for continuous exercise, the great convenience is obtained that the patient can walk about the house with no apparatus at all—can start out of his bed and attend to any sudden emergency as well, almost, as if he were unmutilated. To a far greater extent is this the case after excision of the ankle, and still more after excision of the tarsal bones. Beyond a raised sole nothing is required; and even without this raised sole the foot is often quite useful enough to enable the patient to run nimbly. Therefore, I need hardly say that I consider this third objection to arise simply from want of experience of the operation.

That the same result can be obtained by subperiosteal resection, or by piecemeal scooping away of the bone, may or may not be true; but this consideration does not, I think, argue against this operation which it is the object of this paper to recommend. I do not desire to advise the removal of whole tarsal bones in any but those cases where a very large portion, almost the whole, of the bone is hopelessly disintegrated, and would seldom perform such an extirpation, except after a previous trial of partial operation (as gouging, &c.) Now in such cases, where all that remains of healthy or firm bone is only a thin shell of its outer part, I have no hesitation in saying that whether the same result could or could not be obtained by scooping the bone out, no better result could possibly be obtained from that measure than from total extirpation. Besides, the scooping process is at best uncertain; the extirpation, if the cartilaginous surfaces exposed be healthy, is almost sure to succeed. An impression prevails that the original bone is reproduced after the diseased bone has been scooped out and nothing left except the periosteum; but I cannot discover what support this opinion has in recorded cases. The possibility of scooping away the whole bone and leaving the periosteum has not yet been satisfactorily demonstrated; and if there is any case in which the bone has been proved by dissection to have been reproduced, I have failed to find it, though I have hunted diligently in English and foreign surgical literature for it. But after the extirpation of one of the tarsal bones, the resulting cicatrix is firm enough to support the weight of the body, and to give attachment and leverage to the muscles, and we should require very cogent demonstration to convince us that any bone which could be reproduced from the shreds of periosteum that would be left after the scooping process could replace the bone to any better purpose. The idea that after the scooping out of the diseased bone, a perfect case of periosteum, of the same shape and extent, is to be left behind, and that from this case the original bone is to be produced afresh, seems to me almost absurd. In the first place, could the operation be so performed? Would it not rather result in leaving behind a shreddy mass, consisting only in part of periosteum, of which again a large portion would perish by the resulting inflammation? Nothing could be produced from such a mass except, perhaps, a few detached osseous nodules, and these would contribute nothing to the firmness of the cicatrix or the usefulness of the limb. On the other hand, is there not great risk, in these scooping or piecemeal operations, of leaving some of the bone which ought to have been removed? I pass over the greater loss of time and loss of blood which such operations entail, since this is really a subordi*nate part of the question.* More blood is generally lost in scraping away the *inflamed bone* than in exposing its surface by turning down flaps to remove

it; but this would be of little consequence if the result were better. On the whole, however, I believe the attempt to scoop away the diseased bone, and leave behind the periosteum, will be found very liable to end in disappointment, and to allow of, or even cause, the extension of the disease to neighbouring parts.—*Lancet, Feb.* 18, 1865, *p.* 173.

40.—ON THE RESTORATION OF MOTION IN CASE OF PARTIAL ANCHYLOSIS, OR STIFF JOINT, BY FORCIBLE EXTENSION UNDER CHLOROFORM.

By William Adams, Esq., Surgeon to the Royal Orthopædic and Great Northern Hospitals.

[The interest attaching to this subject very much arises from its novelty, as it is only since the introduction of chloroform that we have been enabled to carry out this mode of procedure in an efficient manner.]

The operation of *forcible extension*, or *forced rupture*, as it is sometimes called, essentially consists in tearing through by forcible movements such bands of adhesion, whether internal or external to the joint, as may limit or prevent its motion; and in these operations we must necessarily tear through some of the proper ligamentous structures of the joint itself, especially the capsular ligament, which, being thickened and retracted by chronic inflammatory effusion, must be to a greater or less extent torn through before free motion of the joint can be obtained.

Bands of adhesion, thickened tissues, and ligaments are therefore necessarily torn through in these operations, and it would naturally be expected that such a proceeding would necessarily be followed by severe inflammation and serious risk to the limb or life of the patient; and undoubtedly such results have followed, perhaps even in many cases, and I shall allude to some such results in my own practice. But, on the other hand, I can adduce sufficient evidence to prove that when appropriate cases are selected, having reference especially to the constitutional condition of the patient and the cause of the anchylosis, and when the operation is done with proper precautions, and with less violence than is generally used, there appears to be no risk whatever of inflammatory mischief following the operation. Frequently, indeed, there is scarcely any heat or swelling, nor is the pain following it sufficient to interfere with the sleep of the patient.

The absence of inflammation, and more especially of suppurative inflammation, as a result of the operation, is in a great measure to be explained by the subcutaneous nature of the injury; and in this respect it may be classed in the same category with dislocations and simple fractures, which we know are seldom, if ever, followed by suppurative inflammation; the great law of subcutaneous surgery being, that all wounds and injuries not exposed to the air, such as simple fractures, dislocations, &c., are not followed by suppurative inflammation; whilst open wounds—*i. e.*, wounds to which the air is admitted—such as compound fractures and wounds from accidents, are, as a rule, followed by suppurative inflammation. The truth of this great surgical law, first pointed out by Hunter, has long been recognized, and is known to be the basis of the practice of subcutaneous surgery, and the explanation of its success.

In the operation of forcible extension, the amount of injury inflicted upon the joint is undoubtedly much less than the joints sustain in many dislocations, especially of the hip and shoulder, in which not only are the capsular ligaments and other ligaments of the joints torn through, but the muscles surrounding the joints are often extensively lacerated.

The mode of procedure at the time of making the forcible extension is very simple and easily accomplished. The patient must be brought thoroughly under the influence of chloroform, and this must be carried to the extent of complete muscular relaxation. If the knee-joint is to be operated upon, an assistant must hold the thigh firmly, and the operator must first flex the *joint, i. e., bend it still* further in its deformed position. It is sel-

dom that much force is required to do this, and a sudden sharp jerk is much better than a steady gradual force.

When the joint has yielded to the flexion movement, the adhesions generally give way with a loud noise, which may even astonish the operator on the first occasion. The joint must then be forcibly extended or straightened, and this is often accomplished less perfectly, and with more difficulty, than the flexion movement. If the yielding of the joint is not satisfactory, a few more sudden jerks may be given, and the flexion and extension movement repeated; but at the first operation the surgeon had better avoid doing too much, as the amount of force used by some surgeons, in the expectation of obtaining the full amount of motion at the first operation, has undoubtedly been the cause of serious inflammation.

The joint should be firmly bandaged after the operation, and the limb fixed in a good position on a splint. The bandage may be damped by a spirit lotion. If swelling and heat follow on the next day, I resort to hot fomentations; but in a large number of cases even this is not required. The patient usually sleeps well without an opiate, and no swelling or heat occurs. If slight inflammation should follow, it generally disappears about the third day, and then passive motion should be commenced. In private practice I generally obtain the assistance of a professed rubber, or shampooer, but a good deal may be done by the patient as well as the surgeon; and there are various mechanical contrivances, such as those suggested by M. Bonnet, by means of which the patient can exercise the joint.

In the case of a lady whose knee-joint I broke down recently, no swelling, heat, or pain followed. She walked into my house four days after the operation, without even a bandage on the joint, and I at once recommended her to be shampooed by Mr. Mahomed, of Somerset Street, Portman Square. Since then I have seen this lady, and the shampooing will be regularly continued. The operation has generally to be repeated several times, and this I do at intervals of about a fortnight.

One of the most successful cases I have had occurred in a man æt. twenty-seven, F. J., admitted into the Royal Orthopœdic Hospital, June 1, 1859. The patient had anchylosis of both knee-joints in the straight position after gonorrhœal rheumatism; the operation of forcibly flexing the joints was repeated fourteen times, generally one leg only being done at a time. This man left the hospital in June, 1860, now five years since the operation, can move both knee-joints freely, and kneel to work at his trade—that of a carpenter. He can flex one knee, so that the heel moves through a space of eighteen inches, and the other fifteen inches.

Now, with regard to the applicability of the operation to different classes of cases, I will offer a few observations; and for our present purpose will arrange all cases of anchylosis, or stiff joint, in three classes, viz.—1st. The Strumous; 2nd. The Rheumatic; and, 3rd. The Traumatic.

With regard to the first class, the strumous, the result of my experience has been to prove that they are decidedly the most unfavourable for the treatment by forcible extension; and I have determined never to repeat the operation in any case belonging to this class, unless it should be of an exceptional character, and present some unusually favourable conditions, especially such as lead to the conclusion that the articular cartilages have not been the seat of ulceration.

In any scrofulous disease of a joint, where there is reason to believe that the articular cartilages and other textures of the joint have been much damaged—and be it remembered that in scrofulous diseases the articular cartilages are, as a general rule, destroyed to a greater or less extent—a stiff joint is certainly the best possible result for the patient; and to obtain this, therefore, should be the object of the surgeon, every care being taken to obtain anchylosis with the limb in the most useful position. Any attempt to recover the lost mobility can only be made at the risk of producing serious inflammation; and if free motion should be obtained under chloroform, and no serious inflammation follow the procedure, the motion will not be *permanently retained*, owing to the damaged condition of the joint. Stiffening *will gradually* return, and the case must at least be considered a failure.

In one case belonging to this class in which I operated against my inclination, but at the request of the patient, after telling him the risk, suppuration of the knee-joint followed, and amputation was obliged to be resorted to.

In another case in which forcible extension was applied to the hip-joint, in a young lady, suppuration followed, but the patient recovered.

In some of these cases, occurring as they generally do in young subjects, there is a risk of separating the shaft of the bone from the epiphysis, which has repeatedly occurred, and sometimes even the bone may be fractured. It occurred to me on one occasion to break the thigh-bone in a young lady. The fracture occurred in the upper third, and as it was in a case of extreme contraction and deformity of the limb, I was thus enabled to produce a great improvement, by setting the fracture with the limb in a good position, so that the general result of the case was satisfactory, and I did not expect to obtain useful motion. The operation was undertaken to improve the form and position of the limb, and this was accomplished, though not in the way proposed.

I believe it to be in the strumous class of cases that the largest number of accidents and unfavourable results have occurred; and as a general rule of practice, therefore, I consider that the operation of forcible extension is not applicable to this class of cases.

With respect to the *second class of cases*—the Rheumatic—the result of my experience has been just as favourable as it has been unfavourable in the strumous class. In a large proportion of cases of stiff joint, or anchylosis, occurring in young adults after rheumatic fever, or as the result of that severe form which occurs during the progress of gonorrhœa, and hence we call gonorrhœal rheumatism, free and useful motion may be restored by forcible rupture of the adhesions and thickened ligamentous tissues.

There is one great peculiarity in the rheumatic form of inflammation, in whatever organ or structure it may occur—viz., a remarkable indisposition to suppuration; and this is in the highest degree favourable to the operation of forcible extension of joints. The tendency of rheumatic inflammation is to the adhesive form, so that whether it occurs in a joint, in the pericardium, pleura, or in the eye, where we can best watch its progress and results, adhesions are formed, but suppuration does not occur.

I have never seen suppuration occur as the result of rheumatic inflammation in any organ or tissue of the body, and such an event must be extremely rare. Where it does occur, in all probability the case is one of a mixed character, and not purely rheumatic.

I have already alluded to the man in the hospital with anchylosis of both knees in the straight position, and in whose case a complete cure resulted, after fourteen times breaking down adhesions, and freely moving the joints under chloroform. This case belonged to the most favourable class, and the treatment was commenced about three or four months after the stiffening had taken place, and steadily persevered in for twelve months.

At the Great Northern Hospital, I have recently had under my care a young woman, whose right hip-joint had been anchylosed in the straight position for a year, after an attack of rheumatic fever, in which both the knee-joints had also suffered. When fully under the influence of chloroform, the hip-joint was examined by several of my colleagues, and not the least movement in the joint existed. After several unsuccessful attempts to flex the thigh by a sudden jerk, more force than usual was applied, the pelvis being firmly fixed, and the anchylosis then yielded suddenly and completely, with a loud noise. Motion at the joint was at once free in every direction. No bandage or splint was applied, slight local inflammation followed, with a little increase of heat and swelling, all of which disappeared on the third or fourth day, cold lotion being applied. Pain, however, was considerable, especially when the slightest movement was attempted.

In a fortnight the operation was repeated, and the joint moved freely, without any force being applied. The sensation conveyed to the touch was that of one cartilage surface moving on the other, the surfaces, however, not being smooth. No inconvenience followed this operation, and the joint

could afterwards be moved a little without chloroform, but any attempts at voluntary motion were always painful.

In another fortnight the operation was repeated, and the joint not only moved freely, but the cartilage surfaces appeared to be perfectly smooth and natural; nothing could be more satisfactory. No swelling or local symptoms followed. Passive motion was employed by a variety of means, especially by flexing the body upon the thigh, in the standing position, when the foot was raised and placed on a low chair, with the knee bent. She could do this very well, and Mr. Willett, the house-surgeon, used frequently to flex the thigh when the woman was lying on the bed. But still all attempts at walking were painful.

Under chloroform, which was again employed, the motion was as free as in a healthy hip-joint, and I never saw a case which promised a better result; but the woman could not be persuaded to persevere in the treatment, as she had a great dread of the chloroform, and each time suffered from its effects for several days. She was a highly hysterical subject, and the pain in the attempt to walk seemed partly due to this cause. The muscles, however, had not time to recover from the effects of a year's inaction. This patient, therefore, left the hospital with directions to persevere in the passive motion; and if shampooing, hot and cold douches, &c., could have been added, as in such a case they would have been in private practice, muscular power might have been restored more quickly. The head sister of the hospital, however, tells me that there was a considerable amount of deception practised by this patient, and that she had sometimes been seen to walk about very well, and on one occasion even was said to have danced. It is supposed that this woman wished to remain an object of sympathy and charitable support, which had been extended to her by a wealthy and benevolent lady. Of this it was difficult for me to judge, but the treatment appeared to be eminently successful, and there seemed no reason why the voluntary motion should not be more free.

In many of these cases movement of the joint may be restored by passive motion alone, without chloroform, if commenced at a sufficiently early period, *i.e.*, soon after the inflammation has subsided; and no fear need be entertained of again setting up inflammatory mischief, which deterred the older surgeons from adopting the pratice, and consigned many patients to the consequences of a permanently anchylosed joint. Here the distinction is of the utmost importance between the rheumatic and strumous affections, as in the latter class the practice would often be as dangerous as it is safe in the former.

In July, 1858, a gentleman came under my care, who had previously been attended for some months by the late Sir B. Brodie for acute rheumatic inflammation of the right knee-joint which had followed an attack of gonorrhœa. All inflamamation had subsided when I saw him, but the knee had remained in a bent position and immovable. This gentleman arranged with much ingenuity several pulleys fixed to the wall in his library; and by means of a number of ropes, straps, and pads, so placed as to fix the thigh and make flexion and extension of the knee-joint, set himself to the task of obtaining motion, whilst he was seated on the floor. With great perseverance he continued this plan for three or four months, sometimes repeating the movements two or three times a day, and the result was completely successful. He recovered the full range of motion in his knee-joint, and is now able to ride or go through a hard day's shooting without inconvenience. In this case sulphur taken internally appeared to be useful in removing occasional rheumatic pains, and after steaming the joint, a turpentine or camphor liniment with friction was also employed.

It is not my intention to refer to individual cases, but if space permitted I could adduce a large number of instances in which the treatment has been to a greater or less extent successful in this, the second class of cases, in which anchylosis has been the result of acute rheumatism in young adults.

From my experience, it would seem that in this class of cases we may *always* depend upon finding the articular cartilages in a healthy, or nearly *healthy*, condition—a point both of pathological and practical importance. *The anchylosis* in such cases evidently depends upon inflammatory thicken-

ing and retraction of the ligaments of the joints and other fibrous structures surrounding the articulations. I doubt the frequent existence of intra-capsular adhesions, so often described by my colleague, Mr. Broadhurst, as existing in the rheumatic as well as in the scrofulous cases of anchylosis. In cases of fibrous anchylosis after scrofulous disease, by which the articular cartilages have been more or less destroyed, bands of fibrous tissue are always seen passing between and connecting opposed bone surfaces, and all such cases are essentially unfavourable for treatment; but adhesions passing between and connecting articular cartilages are rarely, if ever, met with independently of the ulcerative process; and I am not aware that the ulcerative process ever occurs as the result of acute rheumatic inflammation, either in the joints or other textures.—*Medical Mirror, April*, 1865, *p.* 193.

ORGANS OF CIRCULATION.

41.—ON THE COMBINATION OF DISTAL WITH PROXIMAL COMPRESSION IN CERTAIN CASES OF ANEURISM.

By Dr. J. M. O'Ferrall, Chief Surgeon to St. Vincent's Hospital, Dublin. Reported by D. B. Hewitt, Esq.

I have long since been of opinion that compression of an artery on the distal side of an aneurismal sac should precede or accompany that on the cardiac side of the aneurism. I have often observed arrest of pulsation easily accomplished, attended at the same time with a flaccid state of the sac; but I have also remarked in such cases that the moment the pressure ceased the sac filled and throbbed as before. The formation of the coagulum is thus desirable as the first step to consolidation. A half empty sac is the ready recipient for the slightest thready current that can follow the cessation of the compressing force on the cardiac side. It, therefore, appeared to me advisable to interrupt the current only when the sac was full of blood.

These considerations, together with the knowledge of the fact that ligature of the artery on the distal side has sometimes cured an aneurism when the upper or cardiac portion of the vessel could not be reached, made me resolve to try this expedient whenever a fair amount of pressure was not followed by success.

From what I have observed, I am inclined to think that cessation of pulse in a sac which suddenly diminishes in size and becomes flaccid is less likely to be followed by a permanent cure, whatever time may have been occupied in the compression, and that a sac which retains its volume, and is, moreover, full of coagulum, is less likely to be refilled, however short the duration of the compressing force.

When the supply is completely cut off by ligature of the trunk, I believe that the danger of relapse from refilling of the sac by collateral sources is more likely to occur when the sac collapses and becomes flaccid at the moment of deligation than when its dimensions are unchanged. This firmness of the parietes of the sac is always considered favourable to the success of the operation, as implying the presence of fibrinous deposits, whether we attribute them to stasis of the blood or to inflammatory exudation, as suggested by the researches of that distinguished surgeon, the late Dr. Abraham Colles. I have no doubt, however, that whether a sac be filled by fibrinous deposits of some duration, or by coagulum recently formed, that a full sac is very influential in preventing the ingress of blood from a compressed artery above, or collateral branches from below; the compression below the sac need not, in some cases, last more than a few minutes before the current above is stopped. If, on making the pressure above and arresting the pulsation the sac remains full, the object is attained, and time is merely required to allow the blood then liquid to coagulate in the sac. In other cases, it may be prudent to continue both compressions for a longer time.

Three cases have already been treated on this plan with success. The first was published by me in the Dublin Quarterly Journal for November, 1846.—*Dublin Medical Press, March* 15, 1865.

ALIMENTARY CANAL.

42.—ON EXCISION OF THE TONGUE.

By James Syme, Esq., Surgeon in Ordinary to the Queen in Scotland, Professor of Clinical Surgery in the University of Edinburgh.

[Mr. Syme had on previous occasions removed the tongue by operation: both cases, however, proving fatal, he determined not again to repeat the operation. In the early part of November last, however, he was consulted by a gentleman aged fifty-two, on account of a painful affection of the tongue, which rendered deglutition impossible, and death seemed pending from starvation. He determined to remove the tongue as the patient urgently desired it.]

Being thus as it were compelled to make another trial of excision, I carefully considered all the circumstances concerned that might tend to interfere with its successful performance. Of these the one which most prominently presented itself was the prevention of voluntary deglutition that must result from depriving the os hyoides of the power by which it is drawn forwards. In the common cases of cut-throat, where a large transverse wound is made into the pharynx, although the suicide rarely accomplishes his object in the first instance, he still more rarely escapes the fatal effect of pulmonary inflammation induced by irritation propagated from the larynx; and I did not forget that both the patients on whom I had performed the operation in question died from purulent effusion into the lungs. Instead, therefore, of cutting through all the muscles of the os hyoides, as had been done in the former cases, I resolved to retain the mylo-hyodei and genio-hyodei entire, and divided merely the attachment of the genio-hyoglossi. I also thought it would be better to perform the operation without chloroform, since the patient, instead of lying horizontally, might thus be seated on a chair, so as to let the blood run out of his mouth and not pass backwards into the pharynx.

The operation was performed on the 29th, with the assistance of Mr. Annandale, Dr. Sewell, and Mr. Cheyne, to the first of whom I am especially indebted for his able co-operation. Having extracted one of the front incisors, I cut through the middle of the lip and continued the incision down to the os hyoides, then sawed through the jaw in the same line, and, insinuating my finger under the tongue as a guide to the knife, divided the mucous lining of the mouth, together with the attachment of the genio-hyoglossi. While the two halves of the bone were held apart I dissected backwards and cut through the hyoglossi along with the mucous membrane covering them, so as to allow the tongue to be pulled forward and bring into view the situation of the lingual arteries, which were cut and tied, first on one side and then on the other. The process might now have been at once completed had I not feared that the epiglottis might be implicated in the disease, which extended beyond the reach of my finger, and thus suffer injury from the knife if used without a guide. I therefore cut away about two-thirds of the tongue, and then, being able to reach the os hyoides with my finger, retained it there while the remaining attachments were divided by the knife in my other hand close to the bone. Some small arterial branches having been tied, the edges of the wound were brought together and retained by silver sutures, except at the lowest part, where the ligatures were allowed to maintain a drain for the discharge of fluids from the cavity.

Next day I visited the patient, and finding him in all respects comfortable, inquired if he could swallow. In reply he pointed to a drinking-cup containing milk, and intimated that he wished it to be filled; then placing the spout between his lips, while his head was bent backwards, he drank the whole without any cough or sputtering. Having seen this, I felt assured that the result would be satisfactory, and was not disappointed, as everything went on well afterwards. The only inconvenience experienced was from the edges of the jaw being occasionally displaced; but this was easily remedied by an ingenious contrivance of Mr. Wilson, the dentist, who, finding that a *silver* cap inclosing the teeth, was not sufficient for the purpose, fashioned *a shield of gutta-percha*, embracing the chin on each side, and secured to the *metal* plate by a wire.

Under an ample supply of nourishment by milk, soup, and soft solid food, there was a rapid return of strength, so that an improvement in this respect was almost daily observable, and before the end of three weeks the patient declared that he had never felt better in his life. He returned to Manchester on the 23d of January.

Excision of the tongue has thus afforded complete relief in a case of the most formidable and distressing disease. How far the relief thus obtained may prove permanent, and how far it may admit of being extended to cases of similar kind, are questions that can be determined only by experience. But the frequency of malignant growth affecting the tongue in an otherwise sound state of the system, urgently requires the truth to be ascertained in regard to the value of a remedial measure; and if the operation is now, as I trust it has been, freed from the chief danger attending its performance, facts sufficient for the purpose will probably ere long be accumulated.—*Lancet, Feb.* 4, 1865, *p.* 115.

43.—ON A NEW MODE OF TREATING DISEASES OF THE CAVITY OF THE NOSE.

By Dr. J. L. W. Thudichum, Lettsomian Professor of Medicine of the Medical Society of London.

[The treatment of diseases of the nasal cavity has hitherto been attended with very great difficulties, partly owing to its being inaccessible to topical applications.]

All difficulties are removed at one stroke by the discovery of Professor Weber, of Halle; that when one side of the nasal cavity is entirely filled through one nostril with fluid by hydrostatic pressure, while the patient is breathing through the mouth, the soft palate completely closes the choanæ, and does not permit any fluid to pass into the pharynx (a physiological fact thus far already discovered by E. H. Weber, of Leipzig, before 1847, and published in Müller's Achiv, 1847, pp. 351–354); while the fluid easily passes into the other cavity, mostly round and over the posterior edge of the septum narium, in some persons also through the frontal sinuses, and escapes from the other open nostril, after having touched every part of the first half of the cavity of the nose, and a great part, certainly the lower and median canal, of the second half. By means of the application of this principle to the treatment of diseases of the nose, it is possible easily and frequently to wash the nasal cavity, to disinfect and deodorize it, to remove the sordes which accumulate so easily in it, and to apply to its surface a great number of beneficial medicinal substances, so as to prevent acute affections from extending, and to incline them towards a speedy recovery; to stop hemorrhages, allay irritations, and subdue in a remarkable manner chronic affections of the Schneiderian membrane, so as to re-establish a perfectly healthy surface and normal condition of the organ of smell.

The apparatus.—A rod of iron or brass, thirty inches in length, is fastened upright into a heavily loaded foot, so as to form a firm stand. On this rod slides a nut, which can be fixed at any height by means of a screw, and carries an arm and ring, in which is cemented a high cylindrical glass vessel of a capacity of from one to two pints. The glass vessel is open above, and its cavity contracts within the ring in which it is fastened, here directly to pass into a small-bore muzzle, to which a suitably sized flexible india-rubber tube, thirty-six to forty inches in length, is fastened. To the other end of the india-rubber tube, a stop-cock is fixed; upon this a little cup-shaped collar, and upon this the cylindrical perforated nozzle of gutta-percha or of prepared india-rubber. If now the glass vessel is filled with fluid, and the little stop-cock immediately underneath the nozzle is opened, the fluid will escape at the fine openings of the nozzle; and if the nozzle accurately fits the nostril, and the fluid is allowed to flow, the fluid will enter and fill the cavity of the nose, as will be more specially described hereafter.

Great care must be taken to ensure an accurate fitting of the nozzle to the nostril of the person who is to be operated upon, as if fluid escapes by the side of the nozzle it makes the operation difficult and troublesome. It is

therefore necessary to have several sizes of nozzles to be fixed upon the stop-cock at will—for adults, sizes of diameters corresponding to the sizes 5, 6, and 7 of the thick probangs of instrument-makers; for young persons and children, very fine and more conical nozzles of india-rubber or horn. These latter small nozzles have but one central aperture; but the large gutta-percha ones are provided with four openings on the convex part, for reasons which a little practice will show as cogent; for as the nozzle has to be held in an oblique position, one or two openings are pressed against the membranous septum of the nose and closed, while at least one will be open and sufficient to afford a good stream of fluid, two or three giving a considerable current.

In order to avoid all possible chances of infection and ensure cleanliness, I lay it down as a desideratum that every person using the apparatus should have his or her own nozzle, to be used exclusively by that person. In dispensaries and hospitals, where this cannot be so easily effected as in private practice, the utmost care should be exercised to clean the nozzles, and particularly the little openings, from any semi-solid matter, which easily becomes firmly adherent to them. As the current is always directed outwards through the openings, there is hardly any chance of the interior of the nozzle becoming unclean or infectious. Yet it will be well to give to each patient, particularly if he be the subject of specific disease, his own apparatus. Even the suspicion that a patient might by accident blow into the tube and endanger his successor will thus be avoided.

The apparatus described in the foregoing has been constructed for me by Messrs. Weiss, with their accustomed skill and accuracy.

Of the fluids to be employed for rinsing the nose.—Pure warm water, when introduced into the nose by means of the apparatus, causes in most persons a very disagreeable sensation, ending in lachrymation and sternutation (or tears and sneezing), with subsequent copious discharge of watery mucus from the nose. If the quantity of water run through the nose be large, the "cold" produced thereby, including the change in the sound of the voice, may last for some hours. To avoid this objectionable symptom, it is best to employ solutions of common salt, or other salts, of sugar or milk, for rinsing the nose. In the course of practice, cases will arise in which all these solutions offer advantages. For general use, a solution containing one ounce of common salt in a pint of water is satisfactory. Some persons will bear less salt; others will tolerate more. Of this solution—having a temperature rather lower than that of the blood—from one to four, or, if desired, any number of pints may be allowed to flow through the cavities of the nose. It does not easily produce sneezing, rarely lachrymation, and hardly ever any subsequent symptom of cold in the head.

The saline solutions which, next to common salt, offer the greatest advantages, are those of the common phosphate of soda and phosphate of ammonia, and soda. They can be used by themselves, or mixed with the common salt. Their alkalinity has a beneficial effect upon the irritated Schneiderian membrane, and dissolves or loosens any deposits of mucus or pus, which so frequently dry and harden upon the surfaces of the nasal cavity. When these solutions are made with the common hot water of kitchen boilers, they are a little turbid from phosphate of lime. The presence of this slight precipitate is no objection to its use. On the contrary, it is a convenient means of distinguishing this from other uncoloured solutions which may be used at the same time.

Of the fluids to be employed for deodorizing the nasal cavity.—For this purpose I have employed dilute solutions of permanganate of potash. This agent had done me such excellent service in removing the fetor of the mouth in cases of typhus fever, that I was induced to apply it for the removal of the fetor of ozæna, and with the most striking and immediate success. A solution of from one grain to ten grains in a pint of water is a good proportion, according to the severity of the case. The solution tastes alkaline, and acts as a feeble escharotic upon healthy, and particularly upon vascular or erythematous, parts. When the margin of the nostrils is excoriated, the permanganate colours the excoriated part brownish; but the effect of this

is rather beneficial than otherwise, as the excoriated and coloured part dries easily, and after the shedding of the faint brownish pellicle, appears healthy.

Mode of applying these and other fluids to the nose by means of the apparatus.—The fluid, of the proper composition and temperature, is filled into the glass vessel, which is lowered for that purpose. All air in the india-rubber tube is now replaced by fluid, the escape of the air upwards being facilitated by gentle manipulation. The glass vessel is raised and fixed in the position which will give the desired pressure. A little fluid is now allowed to escape from the nozzle, to make sure that all air is expelled. The patient (or healthy person, if it is only desired to show the physiological experiment) is seated in front of a basin, with his head and face slightly bent over it, the apparatus standing by his side. He is told to breathe through his mouth exclusively, and to abstain from swallowing. The nozzle previously selected as of proper size, and connected with the apparatus and filled as described, is now inserted into one of the nostrils, and held there by the patient's hand of the same side. The little stop-cock is now opened, and after a few seconds a continuous and rapid stream of fluid is seen to flow from the opposite nostril into the basin below. Persons who have control over themselves will always bear the experiment as here described; but young persons, nervous females, and children become confused, begin to cry, or to swallow and breathe through the nose. In such cases the level of the fluid in the glass should be very little above the level of the introitus into the external ear, so that the fluid runs very slowly, or only drops out of the free nostril. The hand of the operator should be upon the india-rubber tube, to close it by compression the moment he sees bubbles come through the nostril, or perceives that the patient swallows or becomes confused.

It is always well to let the fluid pass at first under slight pressure, in order to allow sordes within the nose to be loosened and crusts of dried matter to be softened. When this has been effected, it is useful suddenly to raise the glass vessel, and produce a rapid stream, which will then scour the impurities away. In some cases I have done this repeatedly with success. The loosening of crusts and lumps of inspissated mucus is always attended with some irritation, and also with retardation and diminution of the current of fluid. The sudden increase of the pressure is the surest means of causing the least inconvenience to the patient, and effecting in the quickest manner the purpose of the operator.

It is also well to reverse the current now and then, as sordes are much better detached in that manner. If only one nostril is diseased, or the principal seat of the disease, I allow the fluid to enter by the opposite side, and to leave by the affected nostril. I then change the current, and filling the affected nostril, allow the current to leave by the healthy one. Thus half a dozen or a dozen changes may be usefully instituted. This reversal has sometimes the effect of throwing large lumps of inspissated mucus and pus upon the upper side of the soft palate; and as they are too large to be carried round the septum narium into and through the nasal canal by which the fluid leaves, they are taken into the pharnyx, and immediately ejected by the patient through the mouth. This presence of lumps upon the soft palate is, therefore, sometimes a cause of a sudden interruption of the operation. After the removal of these lumps, the operation may be continued as before. It is really surprising what an amount of sordes will sometimes be removed from the nose by this rinsing process. Anyone who has seen it once will easily conceive the manner in which, by means of these constant accumulations, nasal diseases become chronic, incurable, and lead to fearful suffering and death.

The shape of the basin in which the fluid which leaves the nose is received, is a matter of some importance. A common washhand-basin will suffice in many cases in which the patients are not disturbed by the splattering of the fluid as it falls into the accumulating fluid in the basin. The splattering can be mostly avoided by so placing the patient's head that the current from his nostril runs along the side of the basin. But there remains the objection that the sordes, mucus, and dirty fluid are constantly before the patient's eyes. All these *evils can be* avoided by using a washhand-basin provided

with a funnel-shaped tin cover, similar in construction to, but larger in size than, a common spittoon. It is still better to use a high cylindrical vessel of white china—say a large jug of a capacity of from one to two gallons—for the reception of the fluid from the nostril, as the funnel-shaped top can be made more inclined, thus ensuring a quieter flow of the water. For the consulting room it is most convenient to have the washhand-basin sunk into the stand, with the tube leading from the lowest point into a hidden receptacle. If it is desired to collect any of the crusts and secretions for microscopical examination, the aperture in the basin may be plugged, so long as the rinsing process continues. But even this I find unnecessary when the bottom of the basin is not too rapidly inclined, but rather flat. The mucous and inspissated discharges then adhere to the basin, the current from the nostril being insufficient to carry them down into the tube, and can be inspected, or removed with a forceps into a little white china dish, and further examined. The inspection of these discharges is of some importance in a diagnostic point of view; for the nature and amount of the epithelium which they contain show us the special part of the nasal cavity from which they come. The upper regions of the nasal cavity are covered by a cylindrical epithelium, which mostly shows two or three layers of cells upon each other. The lower regions of the nasal cavity are lined with a ciliated cylindrical epithelium, the action of which removes particles of dust and fluid towards the nares. When the discharges are purulent, they consist of pus-corpuscles only, with hardly any admixture of cylindrical epithelium. This characterizes chronic ulcerations. When chronic ulceration becomes extended, the pus is mixed with cylindrical epithelium which is thus being shed. When the affection of the nose is quite recent, no pus, but only cylindric epithelia are perceived in the secretion. In cases of old follicular ozæna, the crusts have on their soft surface a villous appearance, being condensed mucus, indicating in some slight manner the distribution, size, and shape of the mucous follicles of the Schneiderian membrane. In cases of ulceration with rapid loss of substance, as in specific disease, elements of the deeper tissues, particularly fibrous structures, can be perceived in the discharges. When the nasal cavity is the seat of villous or other descriptions of ulcerated cancer, the elements of these tumours are found in the blackish-brown, thick, and extremely fetid discharges.

Of the medicinal solutions which may be applied to the cavity of the nose.—Although the solutions before enumerated act in a measure as alteratives, resolvents, and escharotics, and, therefore, rarely constitute a sufficient medical application by themselves, yet they are more frequently used for preparing the nose for the application of more energetic and specifically acting solutions. To this latter class belong the solutions of alum, sulphate of zinc, and sulphate of copper—the best astringents; the solutions of nitrate of silver and bichloride of mercury—the most suitable alteratives; and the solutions of chloride of calcium, in which suboxide or oxide of mercury is suspended in a finely subdivided state, together with the bichloride solutions—the best specifics. Of stimulating solutions, a mixture of eau de Cologne with water or salt-water is sometimes useful.

The probable concentration of these solutions can be surmised from the circumstance that the sensibility of the healthy nasal cavity stands about midway between that of the eye and the mouth. When the nasal cavity is completely filled with fluid, the specific sense of smell cannot any longer be exercised; even the solution of eau de Cologne is not perceived to be such when it once fills the nose. The sense of smell being thus entirely obliterated by the fluid contained in the nose, the reflex effects which substances may exercise by means of this sense are entirely absent; and the only impingement which the fluids can produce is upon the filaments of sensitive nerves coming from the fifth pair. It is owing partly to this circumstance that comparatively strong medicinal solutions are borne by the nasal cavity without great secretion. Another circumstance favouring the application of stronger solutions is the ready manner in which the healthy surface of the *nose defends itself* against irritating, chemically-impinging substances by *means of a copious* flow of mucus. Excoriated or ulcerated parts lack this

power of rapid secretion; and hence they are affected by medicinal solutions much more than the healthy parts of the surface of the nasal cavity. What is here stated is the general result of experience and experiment; but, at the same time, I must insist that the application of medicinal solutions in each case should be begun with the greatest caution, as individuals differ greatly in point of irritability of the nasal cavity. In the beginning, therefore, very dilute solutions of medicinal substances should be used, and their strength be increased gradually, after their effect has been well exhausted by the use of greater quantities applied by a quick flow, or the use of smaller quantities in a slow current distributed over a longer time of contact.

Solution of alum.—Half an ounce of roughly powdered crystallized alum is dissolved in a small quantity of hot water, and the solution made up to one quart by means of cold and tepid water in such a manner as to insure that the temperature of the solution should be below, but near to blood-heat. In superficial ulceration or blenorrhagic conditions this solution is well borne. Ulcerated parts, which before its application were red, mostly appear as white patches after its application, thus showing that the effect of the alum on the ulcerated surface has been considerable. When I was desirous to manage with smaller quantities of solutions, I have sometimes mixed a little permanganate solution with that of alum.

Solution of sulphate of zinc.—From a scruple to a drachm of the sulphate of zinc, dissolved in a quart of warm water, together with half an ounce or an ounce of sulphate of soda, or sulphate of magnesia, gives a suitable fluid.

Solution of sulphate of copper.—Of this sulphate also from a scruple to a drachm, mixed with half an ounce or an ounce of soda sulphate or magnesia sulphate, can be dissolved in a quart of warm water.

Solution of acetate of lead.—Of this crystallized acetate from a drachm to two drachms, together with half an ounce or an ounce of crystallized acetate of soda, may be dissolved in the quart of warm water.

Solution of nitrate of silver.—Of this salt not more than from half a grain to a grain should be dissolved in each ounce of water. A quart of water, therefore, in which previously from half an ounce to an ounce of nitrate of soda has been dissolved, may receive from sixteen to thirty-two grains of the nitrate. In particular cases the solution may be made stronger. The nitrate of potash is not so good as the nitrate of soda, because it has slightly irritating qualities. When it is necessary to use it in an emergency, when soda nitrate cannot be had, the solution should be more diluted.

Solution of bichloride of mercury.—The greatest caution is necessary in the use of this agent, as it has a tendency to produce excoriations on healthy surfaces. The first solution to be employed should be one containing five grains of corrosive sublimate in a quart of water, in which an ounce of common salt is also dissolved.

Solution of chloride of calcium with suspended oxide or suboxide of mercury.—These fluids are the common phagedænic waters, or black and yellow wash, to which common salt has been added. Two drachms of calomel, twelve fluid ounces of lime water, one ounce of common salt, and twenty ounces of warm water, yield the black solution. One scruple of corrosive sublimate, one ounce of common salt, twelve fluid ounces of lime-water, and twenty fluid ounces of common warm water, yield the yellow wash. These mixtures must be well agitated in the glass vessel while being allowed to run through the nasal cavity.

Sedative solutions.—Of prussic acid forty minims to the quart of warm salt water, of tincture of opium two drachms, may be taken. These drugs may be added to some of the above solutions of metallic salts. But if this is desired, it is better to substitute a solution of morphia for tincture of opium. The prussic acid is incompatible with the copper, silver, and precipitated mercury solutions; it goes conveniently with the alum and common salt solutions.

Styptic or hæmostatic solutions.—Amongst these, ice-cold salt-water, containing an ounce of salt to the pint of ice-water, takes the first place. When this, after having been continued for a considerable time, is insufficient to

stop the hemorrhage, a fluid ounce of the tincture of the sesquichloride of iron may be added to each pint of ice-cold salt-water.

Stimulating solution.—One ounce of eau de Cologne upon ten ounces of salt-water is a useful stimulant. Strong spirit of wine may be taken in place of the eau de Cologne.

I have now fully, and for some readers perhaps somewhat too explicitly, described a number of medicinal solutions which may with advantage be applied to the treatment of nasal diseases by the method in question. I was desirous to impress upon the memory of the reader the fact that I recommend only such solutions as are brought up to a certain specific gravity by salts which do not decompose the medicinal agents. There may be cases in which it is desirable to swell the Schneiderian membrane by watery fluid and produce endosmosis, and others in which highly concentrated solutions may beneficially be used to effect exosmosis and shrivel Schneider's membrane. These adaptations, and the various accommodations of the fluids and their degrees of concentration, I must leave to the skill and ingenuity of those who make use of this method. They will also probably multiply the resources of the rhinotherapeutic pharmacy, and thereby add to the success and certainty of this interesting method of treatment.—*Lancet, Nov.* 26, *and Dec.* 3, 1864, *pp.* 600, 628.

44.—A NEW MODE OF TREATING DISEASES OF THE CAVITY OF THE NOSE.

By Dr. Thomas Skinner, Liverpool.

In March, 1859, a lady patient consulted me about her waitress, an orphan, in whom she took great interest, and whose only fault was an exceedingly fetid breath. On making an examination, I found that she was the subject of chronic ozæna. One morning, when syringing her left nostril, an enormous plug of congealed muco-pus came out of the right nostril, followed by a copious stream of water. The conclusion I then came to was inevitable, so I changed the nozzle of my instrument to the right side, and the stream came down the left nostril.

Since that time I have had many similar cases to treat, and I have always followed the same plan, and with marvellous success. I quite agree with Dr. Thudichum when he says—"It is really surprising what an amount of sordes will sometimes be removed from the nose by this rinsing process."

The means which I have always adopted have consisted of a pint jug of tepid water (my consulting-room water-stand with hot and cold water and waste-pipe) and one of Higginson's invaluable syringes. Occasionally I have had to add a deodorant, and I know none better than the following tincture, a teaspoonful of it being added to a pint of tepid water:—Tincture of camphor, tincture of myrrh, of each three drachms; soap liniment, two drachms; glacial acetic acid, twenty minims; oil of pitch, one drachm.

I have never had any difficulty with any of my patients when I have taken time and moderated the force of the injection to what they seemed able to bear, always commencing with great gentleness, and inserting the nozzle gradually whilst the injection is proceeding. I have never required in the worst of cases to medicate the injections further than stated. Constitutional treatment has accomplished all else.

In conclusion, allow me to direct the attention of Dr. Thudichum and your readers to the *surgical* aspect of this simple discovery.

In June, 1860, the child of one of my patients was brought to me, a little girl two years of age, who had pushed an Indian bead up the right nostril. The mother had made vain attempts to pull it down, and succeeded in pushing it entirely out of sight, accompanied with profuse bleeding and terrific squalling. While the mother held the child's face over my basin, I forcibly injected some tepid water by means of Higginson's syringe up the left nostril, when the bead with one single compression of the elastic cylinder made its appearance in the basin.

There is a Yankee method of treating such cases, which, though inelegant,

is still very ingenious. It is as follows:—An emetic is first administered to the patient; a pocket-handkerchief is then tied tightly over his mouth, and held there until the usual medicinal effect takes place. It is not difficult to "calculate" the result.—*Lancet, Dec.* 17, 1864, *p.* 708.

45.—ON THE TREATMENT OF CONGENITAL CLEFT PALATE.

By Dr. Norman W. Kingsley, New York, U.S.A.

[The physical comfort of an adult patient with congenital fissure of the palate is usually in no way seriously impaired, for he obtains such control over the remaining organs of deglutition as never to experience any great discomfort from the defect. But it is quite another thing in regard to the mechanical function of making articulate sounds.]

So far as I have read, it seems to me too little credit has been given to the velum by physiologists as an organ of speech. I would, therefore, claim for it that it exercises a more important office in the modulation of sound than any other organ, except the tongue. By it, in conjunction with the pharynx, the voice is directed entirely through the oral cavity; by it, in conjunction with the tongue, the voice may be directed entirely through the nasal cavity, and by it the voice may be divided and directed through both channels. On it depend the tongue, teeth, lips, and other organs of the oral cavity for their current of voice, out of which to make articulate language.

It is essential to the purity of any spoken language with which I am acquainted, that, to produce certain sounds, the passage from the pharynx to the posterior nares should at times be entirely closed, while for other sounds it should be entirely open, permitting the whole voice to pass in that direction.

And in this connexion let me not be misunderstood as confounding the uvula with the velum. So far as my observations extend, the uvula exercises no control over the voice; and so far as any injury to articulation might follow, the uvula may be entirely removed, providing the velum and the pharynx have the power of closing the posterior nares. With congenital fissure of the palate it is, therefore, physically impossible for a person to speak the same language of those with perfect organs; and in this way only, in many cases, is the deformity betrayed to the society in which the person moves. It becomes us, therefore, not only as a matter of professional pride, but on far higher and more noble grounds, those of sympathy with a class of unfortunates, in many cases as intelligent, as refined, and as sensitive as their more fortunate neighbours, though deprived by nature of one of the most delightful acquirements of man,—it becomes us to use every means in our power which shall give hope of their restoration to society. It is not uncommonly the case that a practice is continued because it has grown venerable with age, and will not yield to a better method through jealousy or pride.

In the treatment of fissured palate, the operation of staphyloraphy has long held its rank as the treatment *par excellence* for this deformity, and although, during all the earlier years of its history, it was accompanied with varying success, it has nevertheless been put in practice so often, that in a surgical sense little else can be desired or hoped for. Thanks to such men as Warren, Fergusson, Pollock, and others, who by their consummate skill have carried this operation to such a state of perfection, that they have proved all that can ever be claimed for it. Whatever may be the future of staphyloraphy, their operations will form a brilliant page in the history of surgery That this should seem at first the most desirable treatment for fissured palate all must admit. What more natural than the suggestion to pare the edges and unite them by suture, and thus form a perfect septum in place of an impaired one? Thanks to the gentlemen before alluded to, their skill has proved in this country what cannot be said of my own: they obtain a union more or less complete in nearly every case. I have dwelt at some length in the beginning of this paper on the only object of any treatment of congenital cleft palate being with a view of improving articulation. How will staphyloraphy stand *the test? Has it*, as a rule, enabled the patient to conceal the

evidence of the deformity from society? Or in other words, has it proved in any number of cases that it can be relied upon to enable a patient to articulate so clearly and distinctly as not to continually betray the defect? That it does improve articulation in many cases somewhat, and perhaps all more or less, all candid minds must admit, from the abundant statements to that effect. But there is no serious difficulty experienced from a fissured palate in deglutition, and the only discomfort worthy of notice is the mortification arising from unintelligible or defective articulation, how far does limited improvement in speech answer the end desired and hoped for, and how far does it justify a tedious and painful operation, so long as the deformity is still patent to all? In this connexion let me quote from the highest living authority.

"The grand practical object of this operation is to improve the voice and articulation. Defective deglutition from this malformation is what attracts the mother's or nurse's attention in early life. The cries of infancy are in nowise peculiar in tone; but when definite articulation commences, or rather should commence, the value of an entire palate is then appreciated. The air and sound, in passing outward from the larynx, escape in part through the nostrils by the split in the palate. A nasal twang is the result, and articulation as in the normal state is impossible. Immediately after the operation the modification on the voice can be at once detected. .... Improved articulation, however, comes slowly. Years, many years, are required for distinct articulation; and after the most successful operation for cleft palate, months and years are required to alter defective sounds. Voice and speech have to be modified anew. With some the changes come slowly and sluggishly; but with others they are so rapid and perfect that within a few years the original defect cannot be detected except by a practised ear."

Out of the large number of surgical successes in this country, is it a rule that the patient is recompensed for the suffering undergone by the result obtained? I must confess I have no data from which to answer these inquiries; but I *can* speak knowingly of results in the American States, and while I have seen many cases where there was a good union throughout the entire length of the former fissure, I have never seen one where the speech was not defective, nor where the friends considered that it was *very materially* improved. And the opinions now entertained in France may be said to perfectly coincide with those held in my own country. In the 'Lancet' of Nov. 19th, 1864, I find a review of an article furnished by Mr. Pollock on Staphyloraphy for the fourth volume of "Holmes's System of Surgery," from which I make the following extract:—

"It is, we believe, a fact that the success which French surgeons have met with in this operation is so indifferent that it is practically abandoned in the French hospitals, notwithstanding the early trials by which Roux connected his illustrious reputation with staphyloraphy. The French surgeons now entertain the idea that, on the whole, the defect is best left to mechanicians..... But it is a very doubtful question whether surgery is exalted by thus surrendering its triumphs, and calling in the aid of a mechanist to supply defects which may be remedied by their surgical obliteration. Surgeons undertaking staphyloraphy should, however, always bear in mind one circumstance connected with the results of the operation. The most complete success in restoring the palate by union of the cleft by no means implies restoration of the articulation to a natural or intelligible standard. Long after, or even for ever after the success of the operation, in a surgical sense, the patient may continue to speak in a manner hardly less unintelligible and disagreeable than before. The use of the new palate can only be acquired by careful and intelligent practice in the mechanism of articulation. Much perseverance and careful tuition are constantly necessary to attain this end."

I can readily conceive, however, of rare presentations where, with abundance of material, a long uvula, and a good union, without much injury to the muscles, clear and distinct articulation ought to follow; but I think I am justified in *saying* that in a large majority of cases such a result is impossible. My reasons for such a sweeping assertion are as follows:—The newly-formed septum is too short at the boundary at the fauces, and generally somewhat more de-

pressed than it would have been if it had united in its formation, thus contracting the passage between the tongue and the velum; the power of the levator muscle has been much weakened by the knife; the newly-formed septum is either taut or paralysed: and from this combination of causes the passage to the nares remains permanently open, thus permitting a large volume of voice to escape without the power of the tongue to exercise any control over or modify it. The ability to articulate any spoken language correctly must remain an impossibility so long as the velum and pharynx are unable to meet, and at will shut off that passage. I apprehend that on this point alone rest all the failures of whatever treatment; for the difficulties to be overcome at any other point are comparatively slight.

If staphyloraphy, then, comes so far short of accomplishing, in a majority of cases, all that is desired, we are very naturally led to inquire, Is there any hope from any other source?

Obturators have for a long time been resorted to for this object; and when we consider that probably the earlier applications of this instrument were for perforations of the palate induced by accident or disease, and that their good offices were made apparent immediately, it is not to be wondered at that much was expected from their use in congenital cases, and especially so when the true principle upon which an instrument should be adapted was so little understood. It is hard to reconcile the statements which have passed into history of the perfect results obtained by their use in congenital fissure. An obturator properly adapted as a bridge to span a simple perforation of the hard palate, from whatever cause induced, will accomplish all that any treatment can, but there its mission virtually ends. To claim that, with a fissured velum, the cap can be bridged over, or the posterior nares plugged, and perfect articulation follow, is to claim an absurdity—a total impossibility. A great deal of ingenuity has been wasted in their construction, and the claims for them must have been made in advance, without waiting for the proof.

In the present state of science and art, our only remaining hope of relief is the substitution of an elastic artificial appliance to fill up the gap. This simple idea is not a new one, neither is the application of it of recent date. Elastic artificial vela have been made, or attempted to be made, of some form or other, almost ever since the use of india-rubber for mechanical purposes, and, for ought I know, of other materials long before. One thing, however, is certain: the application of suitable elastic material, in skilful hands, for the treatment of these cases, is capable of producing far more satisfactory results than have heretotore been derived from any other source. The indications to be fulfilled by such an appliance are: an obturator, or covering for the cleft, which shall be perfectly adapted to the muscles against which it is to lie; shall be flexible—susceptible of all the motions of the velum or soft palate itself; shall be durable; easily detached and replaced by the patient; and in a word, shall be so under the control of the surrounding muscles when *in situ* that the patient shall have the power, which in a normal state he would possess, of directing the voice at will either through the mouth or the nasal cavity, or both, as desired.

Such an instrument as that described has been brought to such a state of perfection that I have no hesitation in saying that it can be adapted to any case of congenital fissure of the velum that is usually seen, whether complicated with a fissure of the maxillæ or not. It can be made so as to be retained *in situ*, without danger of misplacement; can be worn all the time from the first hour without discomfort; is capable of being raised and depressed and contracted upon itself by the muscles embracing it; and is so simple that a child cannot disarrange it to its detriment. Such an appliance renders it perfectly possible for the patient to learn to speak well.

The physical and mechanical difficulties to be overcome in the adaptation of this artificial velum are a serious obstacle to the operator, for on the nice adjustment of the instrument to all the parts surrounding the fissure depends entirely the comfort with which the patient wears it, and the consequent use he will make of it.

The movements produced by the superior constrictor muscle of the pharynx upon the *remnant of the* velum in the act of deglutition will be

remembered. In the proper adaptation of an appliance arrangement should be made for this movement. If not provided for, the act of deglutition would compel it to fold upon itself, and thus interfere with that function; or if too unyielding, it would irritate the surrounding tissues until it could not be borne. To accomplish that object, the instrument is made so that its parts shall slide over each other like scales when the muscles contract the fissure. It is also essential that it should be accurately adapted to the superior or hidden part of the cavity, as well as to the inferior or more exposed. To secure this adjustment, impressions of the whole cavity are taken in plaster of Paris; these impressions reveal the conformation of all those parts hidden from the eye, including the floor of the nares, the inferior turbinated bones, the vomer, the chamber, and the walls of the pharynx as far down as the fauces. From these impressions plaster models are made in the usual manner, to which models the artificial velum is adapted.

The material of which the velum is made is elastic vulcanized rubber, prepared with special reference to this object, and possessing sufficient flexibility to be carried by the muscles in any direction they act upon it; also sufficient elasticity to regain, and firmness to keep, its original position when the muscles are relaxed. It is so delicate in its structure that I have never known a single instance of irritation or inflammation of the tissue in contact with it when properly adapted.

It is supported *in situ* by resting on the superior surface of the palatine or maxillary bone in the vicinity of the apex of the fissure. It is retained by a very simple attachment of gold connected with it near its apex, and reaching to one or two of the teeth, with a sufficient hold around the tooth to prevent its slipping off. But even the presence of natural teeth is not essential to retain it firmly and properly in its position, as in one case which I had under treatment the patient had not a natural tooth in her mouth, and an entire upper and under set of artificial teeth was adapted, and to the upper set of teeth was attached the artificial palate, which was worn with as much satisfaction as any case that has come under my observation.

There are many points of physiological importance developed by this experience which would be most interesting to dwell upon, did not limited space prevent my presenting them in full. To some of them, however, I must briefly refer.

The intellectual capacity of the patient exercises a greater control over the rapidity and amount in improvement than the peculiar physical conformation of the defect. A musical ear, cultivated to a nice distinction of sounds, is of material benefit in making the most of this appliance. The age should also be taken into consideration, and as early an age as the patient would take an interest in developing its benefit would undoubtedly be preferable. The improper position in which some of the organs of speech are placed in the efforts of the patient to articulate distinctly becomes so habitual as to be almost impossible to overcome, and manifestly the earlier the age at which this is attempted before these habits become firmly fixed the better. I have, however, in one instance, adapted an instrument for a patient over thirty years of age, and in another for one over forty years of age, both of whom derived very marked benefit from its use within a few months.

Again, the sensitiveness of the individual to the defect, the mortification experienced in the exposure by their speech of this deformity, will prove a powerful incentive to their practice and the consequent rapidity of their improvement.

It is astonishing with what entire freedom from discomfort or annoyance the velum is worn immediately on its introduction. I have never had a patient where there was any irritation or inflammation in consequence of wearing it, and only in rare cases have they ever experienced a lameness of the surrounding muscles.

In conclusion, it will naturally be asked, What has been the result of this treatment in any number of cases? I can state most confidently, a decided *improvement* in speech within a very few weeks—a clearness and distinctness *of utterance* which the patient never showed before. In some cases this *progress* has been so rapid that within a period of six months after its first in-

troduction the wearer would not, from his speech, be suspected of possessing such a deformity; and in *every case* within a few months the speech has improved so much as to render it perfectly intelligible to strangers, without repetition. It must not be supposed from these statements that there is any marked change in articulation immediately. There is almost always an immediate change in the tone of the voice, which is much less disagreeable than formerly, and this change is often mistaken for an improvement in articulation. These malformations are manifested much more in the attempt to articulate some languages than others. There is a most interesting field of inquiry for the physiologist growing out of this subject, which I have neither the time nor the ability to do justice to. I can only glance at it. In the articulation of the Anglo-Saxon tongue the compressor-nasi muscle is very seldom, if ever, called into requisition. In the cases of deformity which we have been considering, the escape of voice through the nostril is so great that the individual very soon forms the habit of using that muscle constantly. The result is, they acquire the power of making such sounds as do not belong to our language, but which nevertheless do form a very prominent part of other cultivated tongues. That muscle is much more frequently used in speaking the French language than our own. It will be readily understood, then, that these persons (other things being equal) can much sooner learn to speak the French language correctly than our own. With that language that habit assists them; with our own it must be broken up.—*Lancet*, *Dec.* 17, 1864, *p.* 686.

46.—AN INSTRUMENT FOR CONTROLLING HEMORRHAGE IN THE OPERATION FOR HARE-LIP.

By Thos. Smith, Esq., Assistant-Surgeon to St. Bartholomew's and the Children's Hospital.

The object of the instrument delineated in the adjoining woodcut is to facilitate the operation for hare-lip, by giving the operator a firm hold on the soft and yielding textures of the face, thereby rendering their division more easy; and, again, by controlling the hemorrhage, and thus enabling the surgeon to give his undivided and deliberate attention to the accurate and artistic adaptation of the denuded edges of the lip.

The instrument consists of a pair of forceps, of the shape indicated in the drawing; the blades are deeply grooved to hold the lip firmly, and one handle is fitted with a sliding catch and button to admit of the forceps being fixed, and the degree of compression regulated at will. It is used in the following manner:—The upper lip being separated from its adhesion to the gum to the required extent, one blade of the forceps is pushed up between the lip and gum, and the lip being grasped, the forceps are closed and fixed with the button; so that the fissure in the soft parts is surrounded, except at its upper angle, the coronary and other branches of the facial artery being thereby compressed. The operator, as is usual, should stand behind the patient, and having fixed the instrument, holding it by the handle, with one hand he may raise the lip from the jaw, while with the other he practises the necessary incisions. The edge of the fissure being pared, the sutures may be introduced, and the lip having been released from the gripe of the forceps, the sides of the cleft can be drawn together.

In some cases *it is desirable* to release the lip from the forceps before pass-

ing the sutures, in order that each suture may be passed through exactly opposite and corresponding points of the fissure with more certainty.

The above-described instrument, or one very similar, may be used for controlling hemorrhage in many operations on the lip or cheek, such as the removal of cancers or small tumours; it is also available in operations on nævus of these parts, to arrest the circulation in the growth while the disease is either excised or ligatured. Especially I would venture to recommend its use when practicable for nævus of the face, treated by the injection of solution of perchloride of iron or other coagulating fluid, as I believe that thereby all risk of that rare though most fatal accident would be obviated—namely, the admixture of the injected fluid with the general stream of venous blood.

With the view of meeting the requirements of other operations on the face than hare-lip, Mr. Fergusson has been good enough to make an instrument similar to the one figured, but having movable arms, which expand so as to admit of adjustment to the extent of tissue requiring to be included within their grasp.—*Lancet*, *Nov.* 12, 1864, *p.* 545.

47.—ON CLEFT PALATE.

By Maurice H. Collis, Esq., Surgeon to the Meath Hospital.

[In *Retrospect*, Part xxxiv., p. 138, will be found a previous paper on this subject by Mr. Collis.]

I will first give a short abstract of the progress of staphyloraphy and uranoplasty from the year 1819, when Roux operated on Stephenson, then a student in Paris, and afterwards Professor of Anatomy at Montreal. Prior to that date we have no records in connexion with actual operation, save that we know that in 1767 Lemonnier had operated for fissure of the soft palate with success, and that Græfe, of Berlin, had revived, or re-invented, the operation in 1816, but without a successful issue at that time. Roux's description of his first case reads like a romance, and is told with characteristic energy in his *Forty Years of Surgery*. Ignorant of Græfe's work in this direction, he was seized with the idea of the operation on the moment of first witnessing the deformity, and within two days he carried out his design. But ten days more elapsed when the patient, already cured, read a full account of his own case before the Academy of Science; and shortly afterwards, on his return to Canada, was appointed Public Lecturer on Anatomy—a satisfactory proof of the permanent benefit derived from the operation.

Roux's operations, with trifling exceptions, were confined to the soft palate; he was singularly methodical, and seems, in all cases, to have followed the same steps with unvarying regularity. First, he inserted, from behind forwards, the needles, armed with flat sutures of well waxed thread. Secondly, he pared the edges; and finally, he closed the fissure, if possible, by tying the inferior, superior, and middle sutures in the order I have given. If the edges would not come together, from scantiness of soft parts, he made incisions, through the velum, along the posterior margin of the palate bone, leaving the aperture either to granulate or be closed by an obturator.

Dieffenbach, in 1834, improved on these incisions by making them through the soft palate, parallel to the edge of the fissure; and unquestionably this was a most important step in the right direction.

Up to 1842 all patients who had submitted to this wearying and weakening operation (as it then stood) were enjoined absolute quiet for many days, and were not allowed to swallow anything, not even their own saliva, until the edges of the fissure had united. Sir Philip Crampton was the first to break through this supposed necessity, and to feed his patients liberally with an abundance of soft and nutritious food; this was a most valuable improvement, as tending to bring about union by first intention.

In 1843 Mr. J. Mason Warren, of Boston, Mass., published in the 'New England Medical and Surgical Journal' the results of fourteen cases of soft and *hard palate*, treated by a new operation. Hitherto fissures in the hard palate had *been closed* by an obturator, or, in rare favourable instances, by the rude

transfer of flaps, from one or both sides of the fissure. I shall quote Mr. Warren's description of one of his operations, as I have not been able to get a copy of his earlier paper:—

"With a long spear-shaped knife, curved on its flat side, and cutting for half an inch on both edges, the mucous membrane was dissected from both sides of the palate, as far forward as the anterior angle of the fissure, and backwards to where the soft palate joins the ossa palati. A pair of sharp-pointed scissors, curved like a knife on the flat side, were now carried behind the soft palate, and the attachments of the latter to the ossa palati cut away, leaving the mucous membrane in front intact, thus affording a continuous flap. The uvula was now seized on either side, and the parts being made tense, the posterior pillar of the palate was freely divided, and the dissection continued backwards until the flap expanded, and not the least resistance was offered to any manipulations. Six sutures were rapidly introduced without difficulty."

In this extract Mr. Warren's two great points of improvement are shadowed out. First, the separation of the muco-fibrous periosteum of the hard palate from the bone, along the whole line of fissure, and outwards, on each side, as far as may be needed, even to the alveolar processes; and, secondly, the division of the muscular fibres of the palato-pharyngeus comprised in the posterior pillar of the palate. By this latter process the soft palate is generally rendered capable of easy approximation, as, without doubt, these palato-pharyngeal fibres are the very strongest antagonists to this action. They draw the divided palate outward, and cause its edges to gape most widely, and are the special muscles for rendering the perfect velum tense. The levatores palati are less concerned in this divaricating action, inasmuch as their use is to raise the velum, and, in combination with the superior constrictor, to close off the passage into the posterior nares.

Warren's proposal to close the aperture in the bony fissure by a systematic dissection of the soft covering of the bones was a decided novelty, and a vast step in advance. It was based on the perception of the fact, that the imperfection in the vault of the mouth was due less to a loss of substance than to a misplacement; that, in fact, the coverings of the high but broken arch of bone might be brought down to a lower level, and there united without strain; that, to carry out the simile, a false ceiling might be put to the bony palate—a conclusion at which Mr. L'Estrange has independently arrived in this country. Warren's operations have been most successful; they form the groundwork from which all later improvements have sprung, if, perhaps, I except Mr. Fergusson's well-known and important one of dividing the levator palati muscles from behind. In one particular it is possible that we, who have since operated, have departed from Warren's proceeding without due consideration. It will be seen that he advises the completion of the operation at one sitting, and that to effect this he prolongs the dissection of the muco-periosteum backwards to the edge of the palate bones; and, then, with a curved scissors, divides the attachment of the soft parts to the posterior margin of these bones, leaving the mucous membrane in front intact. Now, without going into the question whether the hard and soft palate should always be attempted at the same sitting, it is manifest to all who have done the operation that Warren's method is the easiest to effect this object; the great point of difficulty is invariably at the spot where hard and soft palates meet. There is here, as a rule, such deficiency of the bone, from the angles being rounded off, as to make it almost impossible to complete the closing of the fissure when the operation is taken at two times. At least one may esteem himself fortunate, if there is not a pin-hole or something larger at this spot, which will only close after repeated touches of caustic or tincture of cantharides. Mr. Warren's method of dissection offers a better prospect of success, although I cannot say that the record of his cases is by any means as good on this point as I should have expected.

Mr. Fergusson, the author of the next improvement, founded it upon some dissections of a cleft palate, a rare advantage, and one that justly gives his views the greatest weight, apart from his eminent position as a practical surgeon. He divides *the levator* palati, with a stroke of a rectangular knife,

from behind forwards, cutting it above the level of the velum, where its belly is thickest, before it has spread out upon the velum. This incision, made with a knife whose point is out of sight, requires nerve and anatomical knowledge, and should not be attempted by any who do not feel themselves masters of the situation. To those who have studied the anatomy of the parts accurately, and who take the trouble of measuring, with their eye, the alterations which the bony points of the palate have undergone, or rather their deviations from a proper outline, there is no difficulty and no danger worth mentioning. I have frequently used Mr. Fergusson's knife and method for the right side of the palate, and Mr. Pollock's for the left, as much for the purpose of illustration as for convenience. Mr. Pollock, for the reasons I have given above, uses a knife either straight or slightly bent on the flat, cutting on both edges; this he plunges through the soft palate, close to the hamular process, and then raising the handle of the knife, without increasing his wound in front, he divides the spreading fibres of the levator on the back of the velum and any fibres of the tensor that may be in the way. His knife has the advantage that it suffices for all the steps of the operation in the soft palate.

I now come to Langenbeck's modification of Warren's operation. He uses very clumsy but useful round-edged chisels to separate the muco-periosteum from the bone. His plan of proceeding is good; his first incision is near the alveolar process, somewhat parallel to the fissure, but at as great a distance as practicable from it. This incision may be made with a common scalpel; his chisels are pushed into this incision down to the bone, and the muco-periosteum is forcibly separated towards the fissure. I have tried this method, and find it act more rapidly than Warren's plan of cutting outwards from the margin of the fissure towards the alveoli. Both may be combined with advantage in some cases. A further aid in shortening the operation (a very material point) may be had by using Mr. Philip Smyly's knife through the nostril. It is a small, square knife, set on the side of a long handle, and capable of being inserted by the nostril, and brought to work along the free margin of the bony fissure. It commences the dissection of the soft parts in this situation best of all knives I have used: and, in one instance, shortened the operation by at least ten minutes. Such are the principal means by which the difficulties of this operation have been gradually lessened, until it has become, in practised hands, one that presents no formidable difficulties, and that does not now necessitate any very lengthened endurance upon the part of the patient. The following cases exemplify the various steps of the operation in a practical manner, according as the malformation extends through the entire vault of the mouth, or is less extensive.

Case 3.—Dooley, a farmer, from Queen's County, aged twenty-seven, came to me, in 1856, with a huge fissure the entire length of the vault of the mouth. There had been hare-lip on one side only, which had been indifferently closed in childhood; or else from insufficient support, subsequently to the operation, the cicatrix had thinned considerably. The alveolar process had been originally widely cleft, and the edges, though now approximated, were not in contact.

March 5. I detached as much of the muco-periosteum as I could, working with curved knives, from the edges of the fissure outwards to the very margins of the alveoli. The flaps were scanty, and required much stretching to make them meet at all. Lateral incisions were cautiously made, so as to allow the flaps to meet easily for a few lines, in the hope that a bar might be got across the gap, as a basis for further operations. However, the flaps were too feeble; there had been much hemorrhage, and much of them sloughed; so that when all had healed, in the course of two weeks, matters were much as before—no union taking place along the margins of the fissure. I consequently abandoned the hard palate, seeing that the gap was much wider than I could reasonably be expected to close. On the 19th I operated on the soft palate in the following manner:—I first passed a strong thread *ligature* through each half of the uvula, by means of a common Liston's *needle*; the ends of each ligature were brought outside the mouth, and given *in charge of* an assistant. Taking Mr. Fergusson's knife in my right hand,

I passed it into the mouth, and through the cleft, until it was beyond the hamular process. I now made the right side of the soft palate tense by drawing firmly on the ligature, which was previously passed through it, bringing the ligature well over to the opposite angle of the mouth. Turning the point of the knife upwards and outwards, I divided some of the fibres of the levator palati, and rendered the palate tolerably passive on that side. I next divided the posterior pillar of the fauces, containing the fibres of the palato-pharyngeus, with a snip of the curved scissors. This completely paralyzed the velum on this side. Before laying down the scissors I divided the palato-pharyngeus of the other side in the same manner. I now took up Mr. Pollock's knife. I made the left half of the palate tense, in the same manner as the right. Pushing the knife obliquely through the soft parts, just internal to the hamular process, until its point was seen in the fissure, I divided the fibres, which were expanded, fan-shape, on the back of the velum, and was gratified to find this side fall quite passive. It is easy to test the efficiency of this part of the operation by simply crossing the hands, and seeing if the sides of the palate come well into contact without a strain. The sides of the fissures were now pared by a very sharp double-edged and spear-shaped knife. The advantage of such a knife is, that by thrusting it through the cleft edge of the uvula, then cutting up and down, the paring of the edge is rapidly effected. Three points of suture were inserted in Liston's needles, the material used being housewife thread. The greater part operated upon united, and the sutures were removed on the third, fourth, and fifth days—the lower first, and the upper last. By a repetition of small operations, at short intervals, I was able to close the fissure up to a line on a level with the hamular process. Beyond this I could not advance, owing to the width of the gap and scantiness of material. However, the anterior margin of the united part became ultimately very firm, and almost straight, so that Mr. L'Estrange was able to adapt a gold plate, with perfect accuracy, to it, and so the entire gap was ultimately filled up. The satisfaction of the patient was extreme; the improvement in his voice was considerable even at once, and in about a year was still greater. Previously to operation he was scarcely intelligible, and in none of those operated on was the ultimate power of distinct articulation more increased. Considering the hopeless appearance of the gap at first, this result was as gratifying as it was unexpected.

Case 4.—Maryanne Elliott, age about thirty, cleft originally complete, but the gums had coalesced. The fissure ran along the centre of the soft palate and half of the hard, then turned to the left between the inter-maxillary bone and superior maxilla. The lip was not cleft. The two sides of the cleft had united in front. The fissure in the hard palate was high and narrow, and the soft parts were not too abundant, either where covering the bones or where free.

On the 21st of March, 1862, I operated for closure of the hard palate (uranoplasty). I used Mr. Smyly's knife for the first time, and with great advantage. It is, as I said before, a small square knife, set sideways on a long handle, capable of being introduced through the nostril. With it I commenced the separation of the muco-periosteum; it effected this rapidly up to a certain point, when Pollock's knife, used through the mouth, completed this part of the operation; the height and narrowness of the fissure made it hard to use any but small narrow knives, and the operation would have been immensely difficult but for Smyly's knife, which proved its value on this the first occasion upon which it was used. There was some strain on the flaps, rather from a tendency on their part to revert to their position in contact with the bones, than from any deficiency of material. Lateral incisions, parallel to the fissure, failed to relieve this tension, and it was not until I had inserted, on either side, wedges of sponge, which not only pushed the centre of the flaps towards each other, but which also interposed a mechanical obstacle to the return of the flaps to their original site, that this strain was overcome. This idea of pushing the flaps, rather than pulling them, was the happy inspiration of the moment, and has since been adopted with great advantage. After forty-eight hours these sponges were removed. Union was satisfactory *throughout all the* hard fissure, except at its two extreme points.

Anteriorly, in narrow fissures, it is hard to get raw surfaces in contact, in consequence of a redundance of tissue; and also because its density renders it unfavourable to union. At the posterior extremity the angles of the bones are rounded off; and there is almost invariably a deficiency in this point, such as often tries the patience of the operator. Mason Warren, Pollock, and myself have all had to complain of the difficulties of closing the lozenge-shaped gap or slit which lies at the border ground of hard and soft palate. Mr. Pollock has suggested a plan which seems efficient to close this gap. If the patient wear, for a time, a small plate, convex towards the mouth and concave towards the gap, there ensues a pouting of the lips of the fissure into the concavity, with a tendency of the sides to approximate and unite. In narrow slits I have attained the same end by touching the edges with strong blistering tincture. Where pin-holes remain in the pendulous velum this latter plan of closing is very good, and should be tried before the repetition of a cutting operation. To return, however, to the case of Maryanne Elliott, I was obliged to defer the operation for velosynthesis until the 23rd of April. It presented no special feature, and was performed as in the previous case, and with success.

On the 14th May I closed a small lozenge-shaped gap between hard and soft palate, by a third operation. The pin-hole at the extreme front closed after a few touches of tincture of cantharides.

The speech in this woman had not been very bad, so that the immediate improvement, though considerable, was not so marked as in Dooley's case.

Case 5.—Miss S., aged fifteen, had a congenital cleft of the entire soft palate only, no hare-lip; the anterior end of the cleft was not acute, slightly rounded; a more easy-looking case could scarcely be met with; yet we encountered unexpected difficulty from want of firmness, not so much in herself as in one of her friends, who insisted on being present during the operation. But for gentle perseverance and determination, and the kind and patient assistance of Messrs. Smyly and W. Stokes, junr., I could not have attempted, much less completed, this operation. It was only necessary to pare the edges and insert the sutures, no division of muscles was required, and yet the operation was long and laborious. I passed a strong ligature through the thickest part of each half of the uvula, and by a steady hold of each, in turn, I was able to stretch it, so as to pare the edges. Five points of suture were inserted, some of them by means of an ingenious needle of Langenbeck's, which was of great service in this case, although in ordinary cases too complex and large. The upper, lowest, and middle sutures were of fine silver wire, and were inserted near the margin of the fissure, while the intervening sutures were of strong silk, and were made to include a good breadth of tissue, being inserted three or four lines from the edge; these last were removed in forty-eight hours, and the wires one after another; the last, near the tip, remaining in more than a week. By accident the tips of the uvula got slightly twisted, so that the left half came in front of the right, and as they were freely pared they united in this position, with a result which, though agreeable, was not anticipated. Usually, in the best cases, some notch remains at the tip of the uvula; here, from the accidental superposition of one half in front of the other, the tip of the uvula, after all was healed, was delicately fine, and it would have required a close inspection to show that any operation had been performed. The conclusion of this case was as satisfactory as its commencement was unpromising.—*Dublin Quarterly Journal, Feb.* 1865, *p.* 96.

48.—ON THE TREATMENT OF ENLARGED TONSILS.

By Dr. Philip Crampton Smyly, Surgeon to the Meath Hospital.

The subject of enlarged tonsils has met with much attention and extensive observations; and yet there are few diseases on which authorities differ so much as to the nature of the disease, the use of the organs in health, the *effects produced* by the disease on the system generally, or the connexion of *these glands*, whether diseased or not, with other glands, however remote.

The indication for treatment is simple enough in theory, namely to diminish the size of the glands. Many and various are the means recommended; and the question as to whether the whole enlarged gland should be removed, or only a part, is not by any means settled. It is to a somewhat new mode of treatment I wish to direct attention in these notes.

When in London, last year, Dr. Mackenzie called my attention to a mode of treating enlarged tonsils by strong caustic, following Fournié. Fournié employed Vienna paste and bichromate of potash. Dr. Mackenzie uses what he calls "London paste;" it consists of caustic soda and lime. I saw several cases treated by him, and have treated many in the same way myself. In most cases I believe it to be very superior to any cutting operation.

I generally employ a piece of ivory, slightly roughened and hollowed at the end. Dr. Mackenzie formerly used a simple glass rod, but now uses a very ingenious instrument, made by Krohne. It consists "of a small shallow spoon or receiver, into which the escharotic is put; the receiver is provided with a lid or cover, which, when the instrument is grasped in the hand, can be pushed forwards or backwards by action of the index finger on a kind of trigger or half ring placed near the handle. The instrument is introduced closed, and, when opposite the tonsil, the lid is thrown back from the receiver, and the latter, with its contained paste, is pressed firmly against the hypertrophied gland for about five seconds; the lid is then pushed forwards over the receiver, and the instrument withdrawn."

This, of course, renders the application much more easy; though, with a little care, and a good light, such as is thrown into the pharynx by the laryngoscopic reflector, I find the piece of ivory quite manageable enough.

When applying the paste there are some cautions to be observed. 1st. The paste should be rather thick and tenacious, and not lumpy. 2nd. Only a little should be taken upon the holder. 3rd. The uvula should not be touched. 4th. Some vinegar should be at hand, in case any part but the tonsil be touched. 5th. The patient must not be allowed to use any water after the application, but allow the saliva to flow freely from the mouth; even after some time I have found a little cold water bring on very considerable pain.

I find from five to ten applications sufficient to remove the enlarged glands in most cases. The advantages appear to me to be:—1st. The application is less painful than cutting. 2nd. It is never followed by dangerous bleeding; and even a little blood is a great loss to many who have large tonsils. 3rd. There is little or no inflammation set up, and therefore less chance of the lung becoming affected, as pointed out by Stokes. 4th. There is no dread of a surgical operation. 5th. It can be applied to the youngest children, and 6th. The rapidity with which the parts may be removed. It would be tedious to give cases in detail; it is sufficient to say, that in all those I have treated in this way, I have not seen an unpleasant symptom.

Dr. Mackenzie, in comparing the "London paste" with caustic potash, observes "that it possesses the following advantages:—

"I. It tends to penetrate, rather than to spread.

"II. The action, though less violent at the moment of application, continues for a much longer time.

"III. It causes much less pain."—*Dublin Quarterly Journal, Feb.* 1865, *p.* 45.

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49. — ON THE TREATMENT OF PAINFUL CANCER OF THE RECTUM, WITHOUT OBSTRUCTION, BY ESTABLISHING AN ANUS IN THE LEFT LOIN.

By T. B. Curling, Esq., F.R.S., Surgeon to the London Hospital.

Amussat, in his well-known memoir, published in 1839, when describing the diseases which require the formation of an anus in the left loin, mentions *scirrhous affections of the rectum or of the large intestine which occasion great pain in defecation. He remarks*: "The establishment of an artificial anus

is the only means of retarding the progress of the disease, and of prolonging life as much as if the cancerous affection occupied an organ less essential to the preservation of the individual." Though founded on a correct pathology, these views were never acted upon by their author, and made no due impression upon the profession. An exaggerated notion of the gravity of lumbar colotomy, and of the difficulty of opening the colon unless greatly distended, and an erroneous idea of the inconvenience consequent on an artificial anus in the loin, have hitherto limited the application of the operation to cases of long-standing insuperable obstruction, generally as a last resource. The distress, in cancer of the rectum, arising from frequent scalding discharges of blood and mucus with liquid fæces, from incontinency, and still more from the establishment of a communication, in males, between the rectum and urethra or bladder, with the escape of flatus from the urethra and of liquid fæces with the urine, and, in females, between the rectum and vagina, with feculent discharges from the latter passage, is of so urgent a character that it might be expected bold efforts would be made to prevent or palliate these evils. These were made, indeed, by Lisfranc and Dieffenbach in their attempts to excise the cancerous rectum, but they seldom removed the whole of the disease, and the patient, if he did not succumb under this dangerous operation, was left in nearly as miserable a state as before; so that the excision of cancer of the rectum has never obtained a place as an established operation in surgery.

The circumstance that lumbar colotomy is in no degree *curative*, but can succeed only in diminishing suffering, retarding the progress of disease, and prolonging life, has no doubt weighed against the operation both with the surgeon and the patient. So long as it is viewed as one of great danger and of great difficulty, and so long as an anus in the loin is regarded as a disgusting infirmity, the objection will prevail. I have endeavoured, in a published work, to show the erroneous nature of these views. I have argued that the operation is not nearly so serious nor so difficult of performance as is commonly supposed, and that a well-formed artificial anus is attended with little inconvenience. If this be granted, the surgeon is scarcely justified in withholding from his patient even the temporary relief from the great miseries I have described which may be afforded by this operation.

In 1851, Mr. John Adams opened the sigmoid flexure of the colon in the left groin for the relief of obstruction consequent upon scirrhous disease in the rectum. In describing the case, Mr. Adams remarked on "the great relief afforded to the local disease by diverting the flow of fæcal matter into a new channel, thus removing one great source of severe irritation."

The first successful operation of lumbar colotomy to relieve the painful symptoms of cancer of the rectum *without obstruction* was performed, so far as I know, by Mr. Nathaniel Ward. I concurred with him in recommending the operation, and assisted in its performance. The patient was a man aged forty-five, in the London Hospital, who suffered considerable pain in the back and loins, particularly after an evacuation, which was attended with agonising pain. In order to distend the colon, Mr. Ward passed the long tube through the diseased rectum, and injected three pints of tepid water, and afterwards opened the colon without difficulty. The several symptoms were much relieved by the diversion of the fæcal matter through the artificial anus. The man survived eight months, and died exhausted.

The following case occurred to me at the London Hospital this year. The notes are furnished by Mr. Heckford, then house-surgeon.

Cancerous stricture of the rectum successfully relieved by making an artificial anus in the left loin.—W. B., a schoolmaster, married, aged twenty-nine, of dark complexion, and tolerably well nourished, was admitted into the London Hospital, under Mr. Curling's care, on the 12th of July, 1864, suffering from malignant disease of the rectum. There was no history of cancer in the members of his family, and his own health had been very good until sixteen months previously, from which time the symptoms of his present affection commenced. The first symptom noticed was constipation of the bowels. He was frequently obliged to have recourse to purgative medicines; occasionally the bowels were unrelieved for six or seven days, and the fæces were always

hard and lumpy. For the first seven months he managed to treat himself; but the costiveness becoming more obstinate, and a constant dragging pain and sense of weight being felt about the sacrum, and also round the anus, with an occasional discharge of blood, he applied to a surgeon. This gentleman did not examine the rectum; but said that it was probably a case of internal hemorrhoids. Soon after this he had an attack of erysipelas of the face, and fell into a low condition of health. He seems, however, to have entirely recovered from this, and at the time of his admission his aspect was certainly not that of one suffering from an exhausting malady. About Christmas, 1863, he noticed that the discharges of blood were much more frequent. Seven months ago he lost all power of retaining the fæces; and since then there has been a constant flow of feculent matter, mucus, and sometimes blood. His rest and digestion were also greatly disturbed.

He now came under the care of Dr. Guy, of Doncaster, who recognised the true nature of the case. For a time he used palliative measures, such as sedative enemata, &c.; but as the patient's health was giving way, Dr. Guy recommended him to come to London and consult Mr. Curling, which he did on the 9th of July last. Mr. Curling, being of opinion that he would derive benefit from an operation, advised his coming into the London Hospital. When he was admitted, all the previously named symptoms were more aggravated, and, of course, he was unfitted for his usual occupation.

Mr. Curling detected extensive cancerous disease, reaching nearly to the anus and narrowing the passage. He was unable to pass his forefinger fully through the mass. During the first week he was ordered two doses of castor oil, which had the effect of bringing away a considerable quantity of retained feculent matter. He also had half a grain of morphia every night.

On July 19th, Mr. Curling, with a little difficulty, passed a No. 1 bougie through the diseased mass. On the following day the operation was performed, chloroform being administered. The patient having been placed on his right side, a small tube was introduced beyond the stricture, and about three pints of linseed tea were injected into the bowel. To prevent its return the tube was kept in during the operation, and its aperture plugged. A transverse incision, four inches in length, was now made midway between the crest of the ilium and the last rib. The different fascial and muscular layers were divided on a director until the subperitoneal fat was arrived at. As this was rather abundant, some little difficulty was experienced before reaching the gut, which was at the bottom of a deep wound. The colon was found uncovered by peritoneum, and, having been drawn to the surface, a longitudinal opening was made in it, when a quantity of linseed tea immediately escaped. The gut and the lips of the wound were united by four sutures. For the first two days there was an almost uninterrupted flow of the intestinal contents, but at the end of this time an opposite condition supervened, necessitating a slight purge. For a time, a charcoal poultice was applied. From the date of operation not a single bad symptom occurred. His appetite and digestion were good; he slept well, and was cheerful in temper. The wound granulated healthily, the sutures being removed on the fifth day. His former distressing pain in the back also entirely left him, and he was able to sit up in about ten days.

After the first three weeks a slight difficulty arose in the rapid contraction of the artificial anus. To remedy this dilatation was assiduously employed. Weiss's three-bladed dilator was introduced twice a day, and the aperture stretched; and at intervening periods pieces of laminaria bougie, properly curved, were passed into the bowel, and retained there a sufficient time to distend the opening.

These means having proved effectual, he left the hospital on Sept. 3rd, and returned to his home in Yorkshire. His present condition, as described by himself, is most satisfactory. He writes, on Oct. 12th, thus:—"Since leaving the hospital I have been gradually gaining strength, and have not once felt the pain in my back, formerly so acute. There is a slight discharge of mucus from the natural anus, but no feculent matter. I have pains still at intervals in the rectum, but they are generally of short duration. The *artificial anus has shown* a tendency to contract, but to counteract

this I insert my fingers occasionally, or use boxwood plugs night and morning. Its external edge is becoming very firm and strong, thus rendering it more difficult to stretch. There is a slight involuntary discharge during the day, making it necessary for me to wear a small bag lined with india-rubber. With this exception my motions are regular, twice daily, and I only take opening medicine at intervals when I find it necessary. I rest well and have a good appetite, and am both in appearance and reality healthier than before the operation. If altered at all in bulk, I am rather more fleshy, though I exhibit no signs of requiring the 'Banting treatment.'"

Dr. Gay, in a late report of his patient, dated Dec. 11th, five months after the operation, states that his appearance is healthy, that he has gained a stone in weight, and suffers very little pain, but experiences a sensation of heat and stinging around the anus, from which there is a little discharge of mucus.

This case was a remarkably favourable one for operation. The patient was much younger than most persons affected with cancer of the rectum, and he had not suffered materially in his general health. It has improved since the operation, so that we may fairly anticipate that in addition to relief from, and prevention of suffering, the progress of the disease will be retarded, and that his life would be a good deal prolonged by the establishment of the artificial anus. The great diminution in the pain in the back in this case and in others after the operation shows that this symptom is greatly dependent on the passage of feculent discharges over an ulcerated cancerous surface.

I would urge the importance of not delaying lumbar colotomy till the worst evils of cancer are experienced, and till the constitutional powers have become seriously weakened by an exhausting disease. I have met with cases of genuine cancer of the rectum attended with very little impediment to defecation, and with very moderate local suffering. In such cases there is not sufficient call for the surgeon to operate; but when the local changes and symptoms clearly predicate the greater evils which will follow, it is far better to prevent their occurrence, by diverting the passage of the fæces at an early period of the disease, than to delay the operation till life is rendered miserable, and till the strength is so reduced that the operation must be performed under conditions less favourable to success. But even in an advanced stage of cancer of the rectum, when life cannot be much prolonged, the relief afforded by the operation is so great, and so much comparative comfort results, that patients in this state may be well advised to submit to it. The following case will bear me out in this remark:—

On Feb. 14th, 1863, Dr. West requested me to accompany him a short distance into Essex to see a lady who was labouring under obstruction of the bowels from a large scirrhous tumour in the anterior wall of the rectum, which compressed the bowel against the sacrum. A recto-vaginal fistula had formed, and before the obstruction, with the exception of very small quantities of liquid matter, all the fæces passed by the vagina. We found her in bed, weak and depressed, and suffering from constant sickness. There had been no action of the bowels for five days. I made a careful examination of the rectum, but could find no passage, the bowel being obstructed by a solid hard tumour of considerable size, which also closed the vagina. The sickness and depression rendered the case urgent, and an immediate operation was decided on. Chloroform having been administered by Mr. Davey, of Romford, the colon was opened in the left loin. The bowel was reached without difficulty; but it was contracted and deeply seated, so that it had to be dragged to the surface before being opened. The margins were then secured to the skin with sutures. The irritability of the stomach quite ceased by the following day, and in two days she was able to take food freely. There was a copious fæcal discharge from the loin. The wound went on well, and healed in about a month, leaving open a sufficient anus. Towards the end of March a fistulous communication took place between the bowel and the bladder, and urine escaped freely by the artificial anus, only a small quantity of flatus and urine, tinged with fæces, passing through the urethra. She became gradually weaker, and died of the cancerous dis-

ease on May 9th, having survived the operation three months. Mr. Davey wrote to me that she was almost entirely free from pain after the operation, and improved very much in spirits, complexion, and general condition; and about six weeks before her death she could walk about the house, and went out daily in a Bath chair.

I will conclude this communication with a few additional remarks on the causes which might deter surgeons from having recourse to lumbar colotomy in cases of painful cancer of the rectum—viz., 1st, the difficulty of opening an undistended colon without wounding the peritoneum; 2nd, the dangers of the operation; and 3rd, the inconvenience of the artificial anus.

A sound knowledge of the anatomy of the parts, some practice on the dead subject, and confidence in using the knife, are certainly very necessary to the satisfactory performance of this operation; but a skilful surgeon will seldom experience any real difficulty in opening a moderately distended colon. In Mr. Ward's case, and in the case of W. B., related in this paper, some distension of the bowel was secured by previous injection through the rectum; and although this cannot always be done, as in the case I saw with Dr. West, the surgeon is not likely to be baffled in reaching and opening the colon outside the peritoneum. In three of the six cases in which I have performed lumbar colotomy on the adult, the colon was contracted, and in one case it was actually compressed against the spine, and put out of the way by the distended small intestines. There might be great difficulty in reaching the colon in a very fat person; but the subjects of cancer of the rectum are not likely to be in this condition. They are always emaciated more or less. A recent writer, Mr. Erichsen, in reference to lumbar colotomy in cancer, remarks:—"In such cases the operation is very difficult in execution; for on account of the contracted state of the colon, that intestine recedes behind the quadratus lumborum, and the folds of peritoneum nearly surround or invest it; and it is so covered in by fat and cellular tissue that a very cautious dissection is required to expose it without wounding the peritoneum." Mr. Erichsen has clearly overlooked the circumstance that the bowel can generally be distended artificially in these cases before the operation is performed.

The parts divided in the operation are skin, muscle, and fascia, and there are no vessels of any great size in the way. The chief risk is of wounding the peritoneum. In the nine operations which I have performed or assisted in, this occurred only once. It could not be avoided in consequence of the very contracted state and displacement of the colon, although the case was one of obstruction. But no peritonitis ensued, and the case did quite well. The success which has attended lumbar colotomy in persons weakened by organic disease and want of nourishment shows that it is not so formidable and dangerous as is commonly supposed. Indeed in no case which I have witnessed has death resulted from the operation.

An artificial anus in the loin, well established, is attended with little inconvenience or trouble in a healthy state of the alimentary canal. Fæces pass only at regular intervals, and the escape of flatus and feculent matter at other times may be prevented by a well-adjusted pad and bandage. When diarrhœa occurs, or unhealthy gases are generated, then annoyance is experienced. An artificial anus, however, under any circumstances, is much less inconvenient and annoying than a cancerous rectum with incontinency, or with such frequent and pressing desires for relief that the patient is constantly disturbed during the day, and is obliged to rise eight or ten times in the night. I was much struck with the little inconvenience attending an artificial anus in the loin in the case of a gentleman with impassable stricture of the rectum, and a communication between the bowel and the bladder, who was successfully operated on by Mr. Pennell in Rio Janeiro in 1849. This gentleman, a merchant, was afterwards much engaged in business, and went a good deal into society. In 1854 he came over to this country, when, through the courtesy of Mr. Clendon, of Albemarle-street, I had an opportunity of seeing and examining him. He died only last year of another disease, having survived the operation fourteen years. In the case of W. B., Dr. Guy, in a letter *dated* December 11th, states that the artificial anus

looks well, and really seems to be assuming a certain degree of retentiveness —in other words, to allow the patient periodically to control the emptying of the bowels. The opening admits the finger, and is not at all sore or irritable. I have myself noticed in more than one case that when the finger has been introduced into the lumbar anus after the wound has quite healed, it has been grasped by the surrounding fibres of the abdominal muscles, much in the same way as by the anal sphincter when the finger is passed into the rectum.—*Lancet, Jan.* 7, 1865, *p.* 3.

ORGANS OF URINE AND GENERATION.

50.—ON LITHOTOMY AND LITHOTRITY.

By Holmes Coote, Esq., Surgeon to St. Bartholomew's Hospital.

The anatomy of the perineum has been made unnecessarily complicated by the arbitrary subdivision of the fasciæ into numerous layers, which do not exist in nature. We meet successively with the skin; subcutaneous tissue and superficial fascia; the muscular fascia; the layer of muscles—namely, erector penis, accelerator urinæ, &c., the interosseous membrane or ligamentum triangulare. Beyond this lies the pelvic fascia. Remember these two points: first, the triangular ligament does not extend below the urethra; secondly, the vessels of the perineum are very irregular in their course and distribution. The distance between the neck of the bladder and the surface of the perineum depends upon the thickness of the subcutaneous strata, varying from two to four inches. The neck of the bladder lies about an inch behind the symphysis pubis, in which spot it is held firmly by its ligaments. It is a fault with many surgeons to cut too much backwards when the knife leaves the groove of the staff and runs between the bladder and rectum. The operation is always an anxious one, because the greater part is performed on structures which are out of sight: but, when difficulties arise, they almost invariably proceed from the fact that the surgeon has missed making a proper opening for the extraction of the stone. In this hospital, where high antiquity gives a traditional interest to its rules, it has never been in my remembrance the custom to draw off the urine just before the operation, and to distend the bladder by the injection of warm water. Such a proceeding is unnecessary and tedious, and is apt to disturb parts. But the rectum is always carefully emptied by means of an injection, and the patient is enjoined to hold his water as much as possible. The position of the patient is that commonly adopted elsewhere.

I do not recommend that the first incision be very deep. When the skin has been divided to the proper extent, the fat and subcutaneous tissue will readily yield to any amount of pressure. By adhering to this rule you often avoid wounding arteries of considerable size, or even a part of the bulb. The point of the forefinger of the left hand should then feel for the staff as it lies under the pubes, and the knife should be made to enter the groove by being pushed obliquely upwards, inwards, and backwards, so as to pass if possible *behind* the bulb and its arteries, being introduced about the middle of the external wound.

The membraneous part of the urethra is that usually first opened, and some urine often escapes; but do not hurry the withdrawal of the staff. Press the knife onwards, followed by the left hand, until you can get the forefinger well into the prostate, when in most instances you can dilate to a sufficient extent. I believe the more we dilate and the less we cut the better. In a case which I had the opportunity of examining, the membraneous part of the urethra was opened, the prostate partially divided, and the neck of the bladder had the *smallest* possible incision. In the choice of instruments, every surgeon has *his own fancy*. I commonly complete the operation with an ordinary knife; *but in cases* of very deep perineum a beaked knife is to be preferred, and in *this case it is* perhaps better that, as he presses the instrument through the

prostate, the surgeon should depress the handle of the staff with his left hand. The stone is removed either by the forceps or the scoop. It may sometimes be turned out by the finger. There is no need of alarm should a moderate amount of hemorrhage ensue; indeed patients in whom this occurs seem often to make the most rapid recovery. Death from hemorrhage is extremely rare. Do not introduce a gutta-percha tube into the bladder through the wound. If the incisions are properly made, the urine will flow through the wound without difficulty. The patient should be put on his left side, a layer of mackintosh under the buttock; the feet should be tied together, the thighs slightly flexed. The diet should be nutritious, and an opiate should be given at night-time if necessary. Cases for the most part do very well, unless there should be disease of internal organs, and more especially if the kidneys be affected.

It has been asserted that in the London hospitals the mortality from lithotomy is 1 in $4\frac{1}{16}$. Whether such a statement be true or not in the aggregate, it is hard to say. There may be incompetent operators or ill-ventilated or badly-arranged buildings. In St. Bartholomew's Hospital the mortality is about 1 in 10, nearly the same as in the days of Cheselden. I have had six consecutively successful cases, and others of my colleagues could give an equally favourable return.

I directed your attention to the case of a young man in Pitcairn ward, who is convalescent from the lateral operation performed nearly three weeks ago. You may remember that he suffered severe pain in micturition, and was liable to epileptic fits in rapid succession. The stone, when extracted, was found to be composed of oxalate of lime; it was as black and as nodulated, and in size the counterpart of a mulberry. Since the operation the lad has had but one slight epileptic seizure, and that during sleep. He is now so far recovered that I can scarcely persuade him to remain in bed.

As in the male, so in the female, I should say, Dilate, but do not divide more than is absolutely necessary. Those who have not tried will scarcely believe the extent to which the female urethra, the homotype of the membraneous and prostatic portions of the urethra in the male, will yield to slow distension.

[During the last two years there have been thirteen lateral operations for stone, and of these two died, whilst out of the four lithotrity cases which have occurred, two died.]

Let us inquire into the causes of death in these two groups of cases. After the operation of lithotomy, of the two fatal cases, one, aged four years, died from diarrhœa; the other, aged sixty-three, from acute inflammation of a thickened bladder.

After the operation of lithotrity, of the two fatal cases, one, aged fifty-three years, died from bronchitis and abscess of the kidneys; the other, aged forty-seven years, from acute inflammation of the bladder, and granular degeneration of the kidneys.

These facts have been furnished me by the surgical registrar, Mr. Willett, whose accuracy and intelligence we all respect.

I grant that the data here given are not sufficiently large for permanent deductions; and yet they correspond with the general impression formed respecting the relative merit of the two operations—namely, that the dangers attending lithotrity are greater than are commonly supposed, and that the very favourable returns quoted by some surgeons proceed from their considering each "sitting" as a completed case; whereas the result should be regarded only after the patient has been freed of his complaint. The late Mr. Stanley was well aware of the dangers attending lithotrity; indeed he delivered a clinical lecture in this theatre in illustration of the fact. I believe that I express his opinion in saying that the operation of lithotrity is suited almost exclusively to cases of men in middle or advanced life, of gouty habit, and with a deep perineum, in whom, after intervals of a few months, a small uric acid calculus drops from the kidney into the bladder, the kidneys being in their general structure healthy. In such cases you may give the patient much relief and greatly prolong his life by crushing each uric acid calculus soon after it has dropped into the bladder, and while it is yet small.

The lithotrite also is useful in breaking up concretions formed around foreign bodies introduced into the bladder. From Ser. 36 (No. 196) I have selected this specimen. It consists of parts of a gutta percha bougie, about five inches in length, incrusted with deposits of lithate of ammonia, ejected from a man's bladder after being broken into several pieces by lithotrity. The bougie was being passed by the patient twenty-seven days prior to its removal, when it broke between four and five inches from its distal extremity, the fragment being left in the bladder. A surgeon immediately attempted its removal by cutting into the urethra through the perineum, but a spasmodic action of the membranceous portion ensued, and the whole fragment was drawn into the bladder. It there lay across the neck, was readily reached with the lithotrite, turned, and an effort made to withdraw it; subsequently I broke it into several pieces, each, as you see, about an inch in length. They were expelled at one act of micturition violently from the bladder the day following the last operation.

In this specimen (Ser. 36, No. 194) you see a portion of sealing-wax, which had been introduced into the bladder three years prior to its extraction. It is almost entirely incrusted with calculous matter.

The lithotrite should under no circumstances be used for the purpose of crushing an oxalate of lime calculus, the character of which is generally recognised by its sharp ringing sound. The fragments are always sharp and irritating to the bladder, and it not uncommonly happens that the lithotrite fails to act on the great mass of the stone.

Most calculi are composed of uric acid. We have a collection of above 200, and of these there are of uric acid alone, 30; of urate of ammonia, 13; of calculi composed of two layers, uric acid nucleus, 16; urate of ammonia nucleus, 25; of calculi composed of three layers, uric acid nucleus, 7; urate of ammonia, 18; total, 111. But this number would be much higher were it not that very many uric acid and urate of ammonia calculi have been thrown away as too small and too common for preservation. Next in frequency comes the oxalate of lime calculus, amounting to 40; of the fusible calculus, 19; phosphate of lime, 11; cystic oxide, 2. The other specimens are composed of carbonate of lime, or are concretions formed upon foreign bodies.—*Lancet, Dec.* 17 *and* 31, 1864, *pp.* 685, 739.

51.—THE PROOFS THAT LITHOTRITY IS AN EMINENTLY SUCCESSFUL OPERATION.

By Henry Thompson, Esq., Surgeon Extraordinary to H.M. the King of the Belgians, Surgeon to University College Hospital.

[An eminent London Surgeon (Mr. Coote) lately stated that "the general impression formed respecting the two operations," namely, lithotomy and lithotrity, is highly unfavourable to the latter, for "the dangers attending lithotrity are greater than are commonly supposed." The operation is moreover suitable "almost exclusively to cases of men in middle or advanced life, of gouty habit, and with a deep perineum." Mr Thompson differs from these opinions entirely.]

With the object of aiding the practical solution of this question, a group of consecutive cases of lithotrity will be presented, with their results. I shall, in short, adduce my own personal experience of the operation during the year just closed; that is, a brief statement of every case in which I have performed and completed lithotrity between Jan. 1st and Dec. 31st, 1864. And surely it will be admitted that, whether this *résumé* affords an exceptionally good result or not (and I contend that it does not), the possibility of such a result suffices to prove the extreme value of the operation for a very considerable range of cases. The patients on whom I have so operated during 1864 are nineteen in number; and I think the result may be regarded, although but a small contribution, yet as numerically much more valuable *for inferential* purposes than so small a number as four cases, of which two *were fatal, which* furnished the theme for Mr. Coote's remarks. As will

appear, not one of mine was so; in one case only the operation was followed by death in a few weeks, opinions respecting the cause of which may be various; the others made good recoveries.

I ought to premise that, having personally no choice as to the performance of the cutting or of the crushing operation, I have been ready to apply either, according to what, after careful consideration of all the circumstances of the case, appeared to be the best for it. Lastly, I have refused to operate in any way upon one case only—one in which the last chance of affording relief had wholly disappeared. Favourable cases, therefore, have not been selected for either proceeding. That there may be no error, I shall in each case name one individual who has seen it with me, so that a guarantee is afforded of the accuracy of my statement. Without any fear that it would be called in question, I still prefer this as the best rule to adopt, since it places the facts stated beyond the suspicion of error, and within the reach of present inquiry.

Case 1.—A gentleman, aged seventy-four. Uric-acid stone; medium size. Seven sittings. January and February, 1864; seen by Mr. Clover, who gave him chloroform at the patient's special request at first sitting, but was not repeated. Success perfect.

Case 2.—A gentleman, aged sixty-nine. Two uric-acid stones of considerable size. Eighteen sittings. Spring, 1864. A patient of Mr. Gayland, of Devonshire-street, who was present on every occasion. Success perfect.

Case 3.—A gentleman, aged forty-five. Uric acid and urates; full medium size. Eight sittings. Spring, 1864. Dr. Koepl, of Brussels, who was then in town, was present at two sittings. Success perfect.

Case 4.—A gentleman, aged forty-six. Oxalate of lime; small medium size. Five sittings. Spring, 1864. Dr. Pancoast, of Philadelphia, then in town, was present at one or two of the sittings. Success perfect.

Case 5.—A gentleman, aged seventy-six. A smallish phosphatic stone. Five sittings. April and May, 1864. This case was seen by Dr. Jenner two or three times. Success perfect.

Case 6.—A gentleman, aged forty-eight. A medium-sized, uric-acid stone. Six sittings. Mr. Hilton was present at one sitting. Success perfect.

Case 7.—A gentleman, aged seventy. Large mixed calculus. Eleven sittings. Summer, 1864. Seen two or three times by Mr. M. B. Hill. All symptoms of stone disappeared. Some trouble remains with pre-existing enlarged prostate; but he rides, drives, and is actively employed.

Case 8.—A gentleman, aged sixty-eight. A rather small calculus; mixed. Five sittings. Summer, 1864. Mr. Harris, of Gower-street, was present at every sitting. Success perfect.

Case 9.—A gentleman, aged forty-eight. A rather large uric-acid stone. Eleven sittings. Summer, 1864. Mr. Hilton was present two or three times. Success perfect.

Case 10.—A gentleman, aged sixty-nine. Small uric-acid stone. Five sittings; Dr. Koepl was present at the first. The symptoms disappeared, and he returned home. A fortnight afterwards the patient called on me to complain of a little return of pain. I advised waiting a week or two before sounding. This was at the beginning of June. At the end of the month he had severe rigors and fever, no instruments having been used. He gradually sank in the middle of July. At the autopsy a calculus, the size of a bean, and smaller ones, were found in the right kidney; in the left was a large abscess. In the bladder there were four calculi like those in the kidney, which had probably recently descended. The operation was incapable of removing the graver part of the malady—viz., that of the kidneys.

Case 11.—A gentleman, aged sixty-nine. Large uric-acid stone. Ten sittings. Summer, 1864. Mr. M. B. Hill saw this patient frequently; my friend Mr. Norman, of Southsea, also on one occasion. Success perfect.

Case 12.—A gentleman, aged sixty-two. Autumn, 1864. Small uric-acid stone. Three sittings. Mr. Hammerton was present each time. Success perfect.

Case 13.—A gentleman, aged sixty-two. Autumn, 1864. Large calculus,

uric acid and urates. Thirteen sittings. Dr. Harley was present at one sitting. Success perfect.

Case 14.—A gentleman, aged fifty-nine. Small uric-acid stone. One sitting. Mr. Clippingdale saw this case with me in Nov., 1864. A subsequent severe illness, from which he slowly recovered; but he writes me now that he has "no return of the old complaint."

Case 15.—A man, aged fifty-nine; University College Hospital. Rather small uric-acid stone. Eight sittings. Nov. and Dec., 1864. The calculus could never be found or seized in any other way than with the blades of the lithotrite directed downwards immediately behind the prostate, however high the pelvis was raised. Discharged free from all symptoms.

Case 16.—A boy, aged six; University College Hospital Small uric-acid stone, the size of a small bean, crushed at one sitting. Discharged cured Dec., 1864.

Case 17.—A gentleman, aged seventy-five. A rather small phosphatic stone, broken up and removed at one sitting in Dec., 1864. Mr. Tatham, of Brighton, was present. The patient had long passed no urine, except by catheter. This condition of course remains, minus the symptoms of stone.

Case 18.—A gentleman, aged fifty-seven. A small oxalate-of-lime calculus, crushed at one sitting in Nov., 1864. Mr. M. B. Hill was present. Successful as far as the removal of the stone is concerned. The patient is still subject to the formation of small calculi, which he has passed in great number.

Case 19.—A gentleman, aged fifty-eight. A uric-acid stone of rather more than medium size. Five sittings. Dec., 1864. Mr. P. Jackson and Mr. C. Jackson, of Wimpole-street, were present at each sitting. Success perfect.

This list comprises eighteen adults, all relieved of stones, of which but a few were small. One patient only died, and certainly not as a direct result of lithotrity, but of renal disease and renal calculus, after having recovered the primary effects of the operation. Of these eighteen adults, the youngest was forty-five years old, the eldest seventy-five years; the majority were above sixty-two years; four were seventy and upwards; and the average age of the whole number is also, as nearly as possible, sixty-two years. No one, I presume, will imagine that for these cases the operation of lithotomy would have afforded results in any way comparable; nevertheless many of the stones were of considerable size.

I venture to add that, in my opinion, the success of lithotrity depends upon the method adopted. During a considerable period of the history of that operation its results were undoubtedly inferior to the results of lithotomy in average hands; and if the same method be still employed, no better results can be expected.—*Lancet, Feb.* 25, 1865, *p.* 199.

52.—THE "MODUS OPERANDI" IN SUCCESSFUL LITHOTRITY.

By Henry Thompson, Esq., Surgeon Extraordinary to H. M. the King of the Belgians, Surgeon to University College Hospital.

[There is no operation in surgery which depends more on attention to minute details for its successful performance than does the operation of lithotrity. There are some leading general principles which direct its practice, which may be thus stated:—]

1. That mechanical contact with the bladder and urethra, whether from instruments or from rough fragments of calculus, is to be reduced to the smallest possible extent.

2. That the natural functions of the urinary organs are to be as much as possible rendered auxiliary to the operation: and that they are to be interfered with, and, *à fortiori*, to be impaired, as little as possible.

In illustration of the first principle—viz., to avoid all unnecessary instrumental contact with the bladder and urethra, I may say that in the nineteen cases related in the preceding article as having been operated on by me

during the year 1864, and comprising 124 distinct sittings, not one preliminary injection was employed, nor was there a single washing out afterwards to remove débris. The lithotrite was rarely introduced more than once at a sitting, never more than twice, and no sitting exceeded five minutes in duration; the majority did not last three minutes. Lastly, a large fragment was never withdrawn by the lithotrite. By this practice a great amount of mechanical interference was spared to the patient. I think I have seen as much harm result from rapid and forcible distension of the bladder by injections for the removal of débris as from any other part of the operation of lithotrity, excepting always, it may be said, unnecessarily rough and frequent use of the lithotrite. The evils which follow rapid distension of the bladder in washing out forcibly are, chronic cystitis and atony of its coats. No doubt the former condition is often treated in the most efficient manner by washing out gently, and with small quantities of fluid slowly thrown in; but that is a kind of washing which is useless to remove débris.

On the same principle, I greatly prefer to avoid any unnecessary introduction of the lithotrite. The object has been to use an instrument which will pulverize the stone as much as possible, so that it may be ejected during the natural efforts of the bladder, and not to remove more débris in the blades of the lithotrite than can be easily withdrawn without encountering resistance, so as not to damage the neck of the bladder or the urethra.

The sitting, also, should be short: two minutes, for the presence of the lithotrite in the bladder, suffice for all ordinary purposes; sometimes three minutes are necessary. Ample time is thus afforded to seize and crush fragments four, five, or six times, employing the flat-bladed lithotrite with no opening in the female blade (so often miscalled or confounded with the "scoop," a wholly different instrument), and removing with it only as much as can be contained without causing difficulty in the withdrawal. Rarely should there be more than a slight appearance of blood, either at the time or in the first urine subsequently passed; often there will be none. The operator requires no other instrument with him than the lithotrite, and for so short and unirritating a proceeding he requires no chloroform and no preparation. The urine may have been voided half an hour or an hour previously; it is of small consequence, provided that too much is not present, a large quantity usually making it less easy to seize the stone. The patient soon loses his dread of a proceeding so little disturbing; and, as the sitting is rarely followed by a chill, it may be repeated every fourth or fifth, sometimes in exceptional cases every second or third day, without inconvenience. If more than ordinary soreness or irritability of the bladder is found on the introduction of the lithotrite, it is withdrawn, or there is a shorter sitting than usual, and so a chill may be avoided; conditions which under anæsthesia would not be recognised. Thus, it may be added, that in two only of the patients referred to (the single child's case of course excepted) was chloroform administered, and each of those at the first sitting only; the second was submitted to, at my request, without it, after which each patient preferred to dispense with chloroform, and had all the subsequent sittings without it. (Cases 1 and 12.)

The flat-bladed lithotrite, referred to above, has the singular merit of finely pulverizing the large fragments very easily and expeditiously: for the object is not only to avoid irritating the urethra, and especially the neck of the bladder, by withdrawing large calculous fragments with rough and sharp angles, but, in conformity to the first general principle, even to hinder such fragments from traversing the canal by themselves. When thus pulverized, the results of the sitting are easily passed by the performance of the natural functions, which, after the operation has been completed, even the elderly patient ought almost always to retain unimpaired. But when large fragments have been drawn through the neck of the bladder, retention of urine often follows, not to say cystitis, and the expelling function is often temporarily, sometimes permanently, impaired or even destroyed. After the first or second crushing of a full-sized stone, when it is necessary to use a powerful and fenestrated instrument, large fragments must necessarily result, and very little *fine débris indeed is* made: but so far from desiring to remove

these, I cannot but regard it as an important maxim to discourage them from passing at first. By keeping the patient in bed for twenty-four or thirty-six hours after the first sitting, desiring him to pass urine only in the recumbent posture, and, if he can, while lying on his back, irritation is reduced to a minimum; the fragments do not pass, but soon get slightly waterworn, their sharpest angles rounded, after which, if expelled, they pass much more easily than at first. The urethra itself has had time to recover from any soreness or irritation which it had acquired. Both the canal and the fragment are better adapted to each other than at first. I would rather see only a little débris pass until after the flat-bladed lithotrite has been used to pulverize the fragments, when the product will be expelled easily enough. On these principles the rule is to interfere with Nature as little as possible. Nineteen-twentieths of all calculi are expelled by her without any help from us; for those of the remainder which come under the hands of the surgeon, she will aid us most efficiently and most safely if we will be content to trust her, and will not interfere to do roughly what she can do well and easily for herself. Time, a little more time, may be necessary to the process; but that is a small price to pay for increased safety to the patient. In this, as in other surgical proceedings, the *nimis diligenti* appears to be the especial source of numerous evils.

It would be affectation on my part to ignore the fact that the practice of removing fragments as little as possible by instrument is diametrically opposed to that of a well-known authority deservedly great on the subject of operations for stone; and, further, that the system which Mr. Fergusson advocates of withdrawing many times during a single sitting large fragments from the bladder, is even regarded by some as an improvement in lithotrity. With great deference to one whose experience and opinions I, in common with my brethren, so highly respect, and from whom, indeed, I regret not a little to differ so widely, I feel compelled to protest against this view of that practice. I have no alternative but to say that increasing experience confirms me in my adherence to the principles laid down as the very foundation of successful lithotrity—viz., to diminish mechanical contact with the bladder and urethra as much as possible; and to crush so efficiently that the débris can come away easily by itself. At all events here is the fact, that, by a rigid adherence to these principles, nineteen consecutive cases of lithotrity, the majority patients of very advanced age, and some with large stones, were brought to a successful termination during the past year without that amount of instrumental interference which is very commonly employed. Further, in not one of these cases has the function of passing water been impaired by the proceedings adopted.—*Lancet, March* 4, 1865, *p.* 225.

53.—RESPECTING THE INSTRUMENTS EMPLOYED IN LITHOTRITY.

By Henry Thompson, Esq., Surgeon Extraordinary to H. M. the King of the Belgians, Surgeon to University College Hospital.

In order to reduce a calculus to the proper condition for passing from the bladder easily, two distinct varieties of instrument are required.

The first, a powerful instrument, of which the female blade has a large opening, sufficiently large to admit the male blade, and through which opening fragments are forced by the action of that blade. The second, a less powerful instrument, of which the female blade is wide, flat, much larger than the male blade, and without any opening to admit the latter.

The first, or fenestrated instrument, is necessary to the first crushing of any stone at all above medium size, and often to the second and third crushings if the stone is a large one; an instrument of the second class, with flat and non-fenestrated female blade, not being sufficiently powerful for the task. Now the fenestrated instrument requires considerable care in its employment; the edges of the male and female blade are sharp, and fit accurately to each other in order to insure an irresistible "bite" on the large hard stone. The result of its action on such a stone is the production of

large, angular, wedge-like pieces, especially if the stone be composed of uric acid. Much space is also required for its employment; the blades must be opened widely in order to seize a large stone, and, from their own thickness, they project considerably beyond it; they must also have a length proportioned to the size of the stone. Being altogether a large instrument,—a condition necessary to the performance of its function,—it is not very readily moved in the bladder. Admirably adapted to the object—viz., that of reducing the entire calculus to several large fragments, it may be most advantageously replaced by the flat-bladed instrument when that object has been effected. To use it for reducing large fragments to smaller ones, is to employ an instrument which is not well adapted to that purpose, and one which is at the same time liable to produce an unnecessary amount of irritation.

For the purpose of reducing large fragments to smaller ones, or to débris, the flat-bladed instrument is greatly superior to the preceding. It is a lighter instrument, and occupies less space; it can be opened to the requisite extent without impinging, so much as the fenestrated instrument, upon the walls of the bladder, especially at its neck. Its blades are shorter, it can easily be turned right or left, or rotated completely if need be. The edges of the blades are rounded, since nothing could be gained by making them sharp; they do not even meet each other, an interval always remaining between them when the instrument is tightly closed, so that a tyro need scarcely fear to lay hold of the mucous membrane. Its blades being wide and flat, not hollowed or scoop-like, and without an opening (except a very small one at the angle), the fragments are easily seized, do not readily slip, and when crushed are reduced mainly to powder between two flat opposing surfaces. For the same reason, if the urethra is capacious, the instrument may be withdrawn, retaining a good deal of this powder (not sharp fragments) impacted between the blades; while if the urethra is not capacious, it is easy to disengage the greater part of this powder from the blades before withdrawing. As a rule, such an instrument may be used, I venture to say, with advantage, certainly seven times as often as the fenestrated instrument. In the nineteen cases I have reported, with 124 sittings, certainly not less than 100 to 110 were conducted with the flat-bladed lithotrite; indeed, the other was only used for the larger stones. But the flat-bladed instrument may be constructed on a very powerful model. If the male blade is made about half the width of the female blade, and with a very slightly wedge-shaped contour, very slight indeed, as I have suggested and employed, it will readily penetrate any uric-acid stone of an inch or an inch and a quarter in diameter. It should here be said that if the male blade has much of the wedge-shaped form, like a ⅄, for example, it not only cuts quickly into the calculus presented to it, but throws off the fragments laterally with extreme force. I tried the experiment some years ago, and was struck with the dangerous facility with which this happened: such fragments were thrown several feet from the instrument when air was the surrounding medium, and very far in water—a totally different result to that from fracture by the ordinary square male blade. The very slight wedge form above described has also the advantage of obviating impaction of the blades with débris, which is more likely to occur with a flat-bladed instrument in proportion as its size is augmented.

Nothing can be more distinct than the charaters of the true flat-bladed lithotrite and those of the instrument which has long been termed a "scoop." The latter was introduced into practice long before the flat-bladed lithotrite was made. In the scoop the female blade is much hollowed out, as the name implies, and the edges of both blades fit accurately one to the other: for this reason there are strong objections to the instrument; and, further, because its hollowed blade is necessarily easily impacted by débris; because a small quantity of débris largely augments the calibre of the blades; and, lastly, because this débris cannot be dislodged after impaction has taken place.

Next, in order to reduce mechanical contact with the bladder to its minimum, *it is essential to employ* a lithotrite of which the two motions, those

of the screw and the sliding movement of the male blade, are easily exchanged the one for the other. The lithotrite which has long been used in this country—always by Sir B. Brodie—is one in which the blades cannot be reopened after having been screwed home, except by the tedious act of unscrewing. Professor Fergusson overcame this great defect by substituting the rack and pinion for the screw as the mechanical power. Speaking generally, however, the screw is still much preferred to the rack and pinion, now that its action can be instantly detached, and its blades opened and closed by a sliding action convertible into screw action by a simple movement of the thumb. Regarding carefully all the points of importance, I do not know any instrument, British or foreign, which at this moment so completely supplies all the desiderata as a form of lithotrite which has been recently made by Messrs. Weiss: that is, one which does its work with so small an amount of concussion in the bladder. It can be turned smoothly round with the greatest facility, merely by the finger and thumb of the left hand, in prosecuting the most delicate search; it can be held immovable with the firmest grasp by its cylindrical handle when dealing with a large stone. I have long felt the want of such a handle in the French lithotrite, which up to three years ago appeared to many to be the most commodious instrument, and in consequence suggested the long cylindrical one; and it has answered beyond expectation. The mode in which the sliding action is converted into screw action, and *vice versa*, which Messrs. Weiss have designed and applied, is much simpler, easier to accomplish, and less liable to cause lateral movement of any portion of the instrument within the bladder, than is the French instrument.

So much for lithotrites. With regard to washing out the bladder for the removal of débris, I am sure all who have much employed it must confess how little productive is the process. When much débris is within the bladder, no doubt it is easy enough to remove some of it thus; but then no difficulty exists respecting this débris. It will pass off by itself readily enough. It is the obstinate fragment at last, or the two or three fragments, that do not come away, but remain in spite of repeated washings, which sometimes demand interference for their removal, and may occasion a little trouble. Some accidental shape perhaps causes this: the fragment may be a flat scale perhaps, which adheres to the bladder, and is not easily transmitted by the natural current, or washed through the eye of a catheter. Whatever the cause, however, so it sometimes happens. For myself, I have preferred in such a case to detect its exact situation in the bladder by means of a small sound delicately used, and, having done so, the patient not moving, then to pass a small flat-bladed lithotrite straight to the spot, when three times out of four it may be picked up at once, usually with the blades pointing downwards, and found often immediately behind the neck of the bladder.

I have recently seen, and two or three times used by way of experiment, an instrument for removing débris from the bladder, designed by Mr. Clover, which promises to be useful. He has contrived a very ingenious method of making a current of not more than one or two ounces of water pass to and fro from the bladder into a glass receiver, into which the débris is deposited. Mr. Clover will, no doubt, describe its action before long. It seems to me much more efficient than the recent application of the ancient method of irrigation by means of the screw, which has been employed by M. Maisonneuve, and one of which instruments I have.—*Lancet, March* 11, 1865, *p.* 254.

54.—THE PECULIAR APPLICABILITY OF LITHOTRITY TO CASES OF SO-CALLED "PARALYSED BLADDER."

By Henry Thompson, Esq., Surgeon Extraordinary to H. M. the King of the Belgians, Surgeon to University College Hospital.

[*Formerly* it was the custom to regard as contra-indicating the performance *of lithotrity* that condition of the bladder in which the patient has lost the *power of* passing any urine whatever by the natural efforts. This loss of *power in most instances* is due not to any impairment of the nervous supply

to the organ, but to some obstructive disease of the urethra, such as prostatic enlargement, impeding the action of an otherwise healthy bladder.]

In some cases, again, it may be due to loss of contractile power in the muscular coat of the bladder, produced by some accidental, perhaps voluntary, over-distension—in order words, to atony of that viscus; no obstruction existing in the course of the urethra. And, lastly, these conditions may co-exist to a greater or less extent in any given case.

I have for some time been led to regard such a condition of the bladder not merely as one in which lithotrity ought not to be performed, or might be performed with difficulty; but, on the contrary, as one which is peculiarly favorable to the success of the operation. I have no hesitation in regarding it as presenting circumstances more likely to conduce to a happy result than those which are presented by the ordinary or average run of calculous patients in middle life who have perfect command of the urinary functions. Nevertheless, examples of success in the cases referred to are still sometimes spoken of as unusual or exceptional instances. I ventured to express the opinion given above in the Lettsomian lectures of 1862, and I am no less convinced of its correctness now. The reason seems to be this, that in cases of patients who have been compelled to use their catheters several times daily for some months or years, the passages have become so accustomed to instruments that the pulverized débris may be withdrawn by the lithotrite with comparative impunity; and in these I do not hesitate to do so, still avoiding the removal of fragments which would irritate the urethra and occasion bleeding. I have operated on several such cases, and have had none that gave less trouble or made better recoveries. It is rare for such patients to suffer from rigor or other constitutional disturbance, for the passages are callous to those sources of irritation which in the patient of ordinary susceptibilities, and unaccustomed to the employment of any instrument in the urethra or bladder, so often arouse systemic derangements of a serious nature.

I believe that it is an error to regard the small calculus recently descended from the kidney into the bladder of an apparently healthy middle-aged man as a matter of very slight gravity and of small surgical importance. I have occasionally seen more distressing and prolonged illness arise from the crushing of such a stone, although only one short and easy sitting sufficed to readily accomplish the object—than from stones of ten times the size. The very presence of a stone in the bladder for a few months, irritating as it is, seems sometimes to render the bladder accustomed to mechanical contact; and the lithotrite may sometimes be used more safely in such circumstances than in a bladder which has but recently become tenanted by a stone, or which has never been entered by a catheter or sound. The cases of small and recent calculus are those which, perharps as much as any, benefit prospectively by passing bougies a few times preparatory to the subsequent use of the lithotrite. With all the usual precautions the case of small calculus becomes one of extremely favourable augury; but it is hazardous to be in the least degree indifferent to precautions because the calculus is small or recent.

I have operated in seven cases in which the patients had for years depended on the catheter to evacuate the bladder: in one instance from real spinal paralysis; in the other six, from atony of the bladder or from enlarged prostate. One only ever had rigors; all made excellent recoveries.—*Lancet*, *March* 18, 1865, *p.* 281.

55.—OBSERVATIONS ON LITHOTOMY.

By James R. Lane, Esq., Surgeon to St. Mary's and the Lock Hospitals, and to St. Mary's Hospital for Fistula, &c.

[The method of operating recommended by Mr. Lane is based upon that introduced and long successfully practised by the late Mr. Aston Key, with a straight staff. Mr. Key's objection to the ordinary curved staff was that it is of the *form least adapted to convey a cutting instrument where the eye of the operator cannot follow it.*]

The advantage of having a straight director for the knife has always ap-

peared to me to be very great. The pattern of the staff used by Mr. Key is, however, in my opinion, faulty in some important particulars. In the first place, it is not sufficiently curved at its extremity, so that there is still risk that in the hands of an indifferent assistant its point may escape from the bladder; secondly, the groove is continued beyond the straight part into the bend, which circumstance renders it possible that the point of the knife may pass out at the end of the groove, and even penetrate the opposite side of the bladder—an accident which I have been told has actually occurred. The first disadvantage may be avoided by having the last inch or inch and a quarter of the staff abruptly curved, so as to resemble in form the ordinary lithotrite; it will then not be likely to slip out of the bladder, and it will be of the most convenient form for discovering the stone before the operation is commenced. The second risk is avoided by having the groove made to terminate just before the curve begins, and to terminate, not gradually, but by an abrupt stop, against which the point of the knife may be distinctly felt to strike. The curved part of the instrument should be regarded in the light of a sound; the straight part, and that only, as a director to guide the knife into the bladder. During the first part of the operation the staff is held by an assistant, not perpendicularly, as when the usual curved instrument is used, but inclined at an angle of 45° with the patient's body. The superficial incisions having been made in the usual way, the urethra having been opened, and the point of the knife securely lodged in the groove, the operator takes the handle of the staff in his left hand, and depresses it nearly to the horizontal line before he commences his deep incision. At the same moment, holding the knife firmly in the groove of the staff, he turns the groove, which has hitherto been directed downwards, obliquely towards the left side of the patient, and thus lateralizes both knife and staff to the degree required for the section of the prostate gland. He then pushes the knife steadily onwards until it is arrested at the end of the groove, or until a copious gush of urine assures him that he has penetrated far enough. It is almost impossible that the knife should escape from the groove; and by holding the staff in his own hand while he is using the knife, the operator will have a feeling of consent and security in the manipulation of the two instruments, which can never be obtained while one of them is in the hands of another individual. By firmly depressing the handle of the staff, its point, and with it the prostate and neck of the bladder, are raised in a corresponding degree, and are lifted away from the rectum, the danger of wounding which is consequently diminished. Still further safety to surrounding parts may be obtained by the operator pressing his knife firmly against the staff while he is making his incision, so as to carry both staff and prostate upwards and towards the right pubic ramus. He then withdraws and relinquishes the knife, and takes hold of the staff with his right hand, while he passes his left forefinger through the wound along the staff into the bladder. Having ascertained in this way that the incision has been satisfactorily completed, and having felt the stone, he withdraws the staff, and exchanges it for the forceps.

The advantages of the straight over the curved staff in making the deep incision are great; it enables the operator to take the staff in his own hand, his left forefinger not being required in the wound to protect the rectum, or to direct the passage of the knife; it admits of being lateralized together with the knife during the deep incision; and in the most important and delicate part of the operation, the knife, while out of sight, has to move along a straight instead of a curved instrument as a director. The sole objection to it that I can see is that it does not afford the same support to the first incision, and that the groove is not quite so easily exposed. It carries the urethra further from the surface, and there is a probability that it may be opened too far forwards, and that the bulb may be wounded. A very little care and practice will enable the surgeon, by directing the point of his knife somewhat deeply, and aiming at the apex of the prostate at the moment of opening the urethra, to avoid this error. I cannot conceive that anyone who is competent to undertake lithotomy at all, can have any appreciable difficulty in safely *completing* this stage of the operation.

In making the first incisions in lithotomy nothing is gained by commencing

too far forwards. No more room is thus obtained for the extraction of the stone, while the liability to wound the bulb or its artery is increased. In my opinion the distance of an inch and three-quarters from the anus, sometimes recommended, is more than is necessary. I should say an inch and a quarter in an adult, and an inch in a child, afforded amply sufficient room in this direction. Some have recommended the incision to be commenced some distance to the left of the raphé, in order to avoid the bulb; it is quite true that by so doing the danger to the bulb is diminished, but the danger to its artery, which is of much more importance, is proportionately increased. The artery of the bulb passes obliquely upwards and inwards beneath the triangular ligament to enter the bulb upon its upper surface. Its position varies a little according to the level at which it takes its origin from the internal pudic, but its average course is parallel to, and a little in front of, the transversus perinei muscle. A due appreciation of this fact is of great practical value, for if the incision be begun away from the median line, and be continued deeply above the level of the transversus perinei, this artery is nearly certain to be cut across. I always commence the incision close to the raphé, and by doing so I believe the artery of the bulb is in almost absolute safety, while the bulb itself, which, however, is of less importance, may be avoided by not inserting the knife too deeply at the commencement of the first incision, and by aiming behind the bulb towards the apex of the prostate at the moment of entering the urethra.

A few years since (see the 'Lancet' for 1856 and 1857) an animated discussion took place as to the mode of holding the knife in the deep incision adopted by the late Mr. Liston, who seems to be generally accepted as the model lithotomist of modern times. In the drawing given in Mr. Liston's book, which has been followed in the earlier editions of Messrs. Miller, Pirrie, and Erichsen, the knife is placed over the hand—that is, it rests upon the palm, which is directed upwards, and the forefinger is advanced to support the blade, and steady it against the staff. According to Mr. Syme, Mr. Liston afterwards had the posterior part of the blade blunted, like the knife now in ordinary use, in order that the finger might not be injured. This, however, is not represented in the drawing. Mr. Fergusson maintains that there has been a mistake on the part of the artist; and that Mr. Liston was not in the habit of holding the knife in the manner represented, but *under* the hand, with the forefinger resting upon the back of the blade, which is the method adopted by himself. Mr. Fergusson's views are supported by Mr. Miller and Mr. Cadge, both of whom had ample oportunities of witnessing Mr. Liston's operations; while, on the other side, Mr. Syme, who is corroborated by Mr. Pirrie, positively asserts that Mr. Liston's practice was to hold the knife over the hand, supported by the forefinger, and that he "would have been horrified at the idea of running a knife into the pelvic viscera of a patient without confining it between the finger and a director."

I am, of course, unable to throw any light upon this controversey; I allude to it merely as exemplifying the importance attached to the point by several of the leading surgeons of the day, and for the purpose of adding that the method which instinctively recommends itself to me is to hold the knife *under* the hand, with the forefinger on the back of the blade, during the first part of the operation, and until the urethra is opened, but to shift the hand into the position attributed to Mr. Liston by Mr. Syme, and represented in Mr. Liston's diagram, before commencing the deep incision. It appears to me that a greater degree of security, with equal freedom of movement in making the deep incision, is in this way obtained.

Since I adopted the plan of operation with the straight staff herein described I have operated on ten cases of stone. Eight of these have occurred at St. Mary's Hospital; all have been children under ten years of age, and all have recovered without the intervention of a single unpleasant symptom. Of the other two cases, one was a child of four years, under the care of Dr. Baber, of Brompton, which also recovered favourably; the other, which terminated fatally, was a patient of advanced age, under the care of the same gentleman, in whom, however, the operation was undertaken almost as a last resource, to obtain relief from severe suffering, and without very sanguine

hopes of ultimate success. In this last case I found the straight staff answer remarkably well. The deep incision was completed with the greatest ease, although the prostate was of large size and the perineum deep, so much so that the bladder could not be afterwards reached with the finger.

In the nine children alluded to I never encountered the least difficulty or delay in cutting into the bladder, except to a slight extent on the last occasion, in which a second application of the knife was required, the incision not having been carried far enough at first. This case, I explained to the students at the time, afforded a good exemplification of the difficulty which may arise in children, owing to the bladder being situated higher in the pelvis, and therefore deeper from the surface of the perineum, than in the adult; and, perhaps in a still greater degree, to the looseness of its connexion with surrounding parts, and consequent greater mobility. I found that the incision first made would not permit the finger to enter the bladder, and that the slightest attempt to force it along the staff in that direction only caused the bladder to recede before it without any nearer approach to the object in view. I of course immediately desisted, and, having introduced the knife again into the groove, passed it onwards until the bladder was fairly opened. Nothing would have been easier in this case than to have excavated with the finger a space in the recto-vesical region, which might have been easily mistaken for the bladder; or, with a greater degree of force, to have detached the bladder completely from the urethra, and to have penetrated into the cellular interval between the bladder and the pubic symphysis. No doubt this is the true explanation of the failures to complete the operation in children which are occasionally heard of, where no stone has been discovered after a protracted search; but in the cases where the patient has had the good fortune to recover, one has been removed by a subsequent procedure. The bladder has, in fact, never been opened at all, but a cavity, which has been mistaken for the bladder, has been hollowed out by the operator's fingers in the perineum. The knowledge of this cause of failure is no new thing to those experienced in lithotomy; nevertheless it has not always been noticed by surgical writers so prominently as it deserves, and it is impossible that the caution against it can be too often repeated. Those unaccustomed to lithotomy in children are often insufficiently aware of the great depth to which it may be necessary to carry the incision, and of the facility with which the mishap here alluded to may occur to the unwary. Mr. Fergusson has recently dwelt very forcibly on this subject, in his lectures delivered at the College of Surgeons. (See the 'Lancet,' July 2nd, 1864.)

Children with stone in the bladder are, as every one knows, very liable to prolapse of the rectum, and the bowel is frequently protruded just before the operation, during the struggles incident to the inhalation of chloroform. Under these circumstances I have often seen it returned by the surgeon before commencing the operation. I believe, however, that this is a mistake, and that it is better to allow it to remain down. If returned, it is lodged in a mass just within the sphincter, and is in danger of being wounded, as it encroaches on the space required for the incisions. I have on several occasions allowed it to remain down throughout the operation, and have never found any inconvenience from so doing.

In the present day, when the operation of lithotomy has been decided upon, the question arises whether the median operation, about which so much has been said of late, should be adopted in preference to the old lateral method. The median operation—the essential features of which are, a limited incision of the urethra in the median line of the perineum, and forcible dilatation of the prostate and neck of the bladder—has, as is well known, been much advocated of late years; it has been adopted by various surgeons of repute, and has been attended with a considerable degree of success. The success, however, of lateral lithotomy in children is so well established that the median operation can scarcely surpass it in this respect; while in adults, if we may judge from recent statistics, the inferiority of the new method becomes evident. I allude to the tables published by Mr. Williams, of the Norfolk and *Norwich Hospital*, of forty-four cases of median lithotomy, compared with *the last forty-four* current cases of lateral lithotomy performed in that institu-

tion up to November, 1863. So far as the numbers go, this appears to be as fair a mode of comparison as could be devised. There were eleven deaths after the median, only two after the lateral operation. The patients were of all ages, but the median operation had the advantage of a rather larger proportion of children. There was one case of death after the median under five years of age, but the principal mortality occurred between the ages of sixty and seventy. The weight of the stone is given in each instance: in no case except one did recovery take place after the median operation when the calculus exceeded in weight three drachms two scruples. It is right, however, to mention that the period required for recovery was found to be less after the median than after the lateral operation: the average time being thirty days in the former, and thirty-seven days in the latter.

From this it may be inferred that the median lithotomy in children is not safer than the old operation, while in adults it is not so safe; that in either case, if the stone is large, it is decidedly the more dangerous of the two, and therefore should only be practised, if at all, where there are sufficient grounds for believing the stone to be of small size: and that its principal recommendation is the somewhat shorter period required for recovery. I have never yet felt disposed to adopt it, not having been able to see that anything is gained by making the incision in the limited space which is available in front of the anus, rather than in the much more convenient and spacious lateral region; and being far from convinced of the advantage of forcible dilatation and laceration of the prostate and neck of the bladder over the limited incision of those parts, now generally practised in the lateral operation.—*Lancet, Feb.* 11, 1865, *p.* 142.

56.—ON THE SOLUTION OF CALCULI IN THE BLADDER AND KIDNEY.

By Dr. W. Roberts, Manchester.

[A very interesting paper was read on the above subject at a meeting of the Royal Medical and Chirurgical Society. The treatment recommended is only applicable when the calculus is small and of the uric-acid kind, and when the urine is not alkaline from disease.]

His treatment consists in the administration of large doses of acetate or citrate of potass, so as to keep the urine constantly of a certain degree of alkalinity. For weeks together, the patient may have his urine thus kept alkaline without injury. Dr. Roberts, in fact, considers the administration of alkalies in this way as harmless as the taking of so much sugar. The paper was remarkable as a methodical, scientific study; and, also, well indicates the immense service which we may anticipate that chemistry will one day render to medicine. The real value of the proposed remedy can of course only be decided by future experience.—*British Medical Journal, April* 1, 1865, *p.* 326.

57.—ON THE CURE OF HYDROCELE BY ACUPUNCTURE.

By Dr. John Thomson, Bideford.

[Acupuncture is not unfrequently attended with a radical cure of the disease.]

In the congenital hydrocele of children, it will very generally succeed, and not unfrequently in that of adults. I admit its great inferiority to the practice by injection; but where this cannot be adopted, as in children and old persons, it seems to me but a common-sense thing to use the method next in order of efficacy.

For many years, I have not tapped a hydrocele with a trocar, unless as a prelude to injection; but have, in all cases, used acupuncture, and, to my great gratification, every now and then a cure is effected, with less inconvenience to the patient than had he been tapped as many times in the ordinary way.

It is no new suggestion to treat hydrocele thus, as it was long ago tried in Germany, and since in this country by Mr. R. Keates and Mr. Travers, who reported that "it acted favourably in a number of cases, but was not generally to be relied on."

I believe this is quite as much as can be said for it. My position is simply, that, as acupuncture properly performed gives the patient even less inconvenience than tapping with the trocar, and is frequently followed by a radical cure, which the operation by tapping is not, therefore it is advisable to use acupuncture in all those cases that are unsuitable for the treatment by injection, seton, &c.

There is something to be said about the manner in which acupuncture is to be performed. It is advised by a recent writer to use a grooved needle, but for what reason, I cannot understand. If the needle is cylindrical in the shaft, with a conical point, as is usually the case, it is a very unscientific shape for puncturing tight structures like the skin and sac. All needles intended to penetrate the human structures readily, should be either triangular or spear-shaped at the puncturing extremity; and the shaft should not be large. I always used a needle that Mr. Liston employed in the operation for hare-lip; it has an expanded spear-shaped extremity for piercing, and a small cylindrical shaft. From three to six punctures may be made with this, well through the skin and sac, the scrotum being grasped firmly on its hinder part in the left hand at the same time, so as to tighten the integuments. No marked pain is felt; a little fluid oozes from the punctures while the hand is applied, but ceases on its removel; in a day or two the tumour is gone.

The needle used by the sewers of kid gloves will answer equally well with that I have described; it has a sharp triangular extremity for piercing, our needle-makers knowing that this is the form necessary for entering tough animal structures; the round conically pointed needle is only applicable for loose textures.—*British Medical Journal, Oct.* 22, 1864, *p.* 465.

58.—ON THE TREATMENT OF HYDROCELE.

By Dr. William Jollie, Gateshead-on-Tyne.

[Dr. Jollie recommends the following plan which he has followed for some years, and invariably with success.]

I tap the hydrocele by trocar and canula in the usual way, draw off the fluid, and then introduce through the canula into the cavity of the tunica vaginalis a common surgeon's probe, which has been previously coated for an inch of its length with nitrate of silver. I prepare the probe by heating the extremity to a dull red heat in the flame of a gas-light, and placing it in a little finely-powdered nitrate of silver, and then again subjecting the probe to the heat, so as to form a smooth coating to the instrument. If your correspondent and other surgeons will make use of this method, they will, I have no doubt, quickly, effectually, and cheaply relieve their patients of a troublesome complaint.—*Lancet, Jan.* 14, 1865, *p.* 54.

59.—ON A CASE OF DEATH RAPIDLY FOLLOWING THE USE OF HOLT'S DILATOR.

By Henry Smith, Esq., Assistant-Surgeon to King's College Hospital.

William H., aged twenty-four, single, a clerk, was admitted into King's College Hospital under the care of Mr. H. Smith, on Feb. 8th, for stricture. He had suffered from it for three years, and had been under various surgeons. During the last nine months his symptoms had increased, and he applied at the hospital on the 3d of February, when the house-surgeon attempted to pass an instrument, but failing, sent him to Mr. H. Smith, who, after a great deal of difficulty, succeeded in introducing a No. 3 silver cathether into the *bladder*. There was a very tight stricture at the anterior part of the bulb, *and, after having* passed through the stricture, the point of the catheter di-

verged towards the left side, and was with difficulty made to find the right passage. Finding that the stricture was so tight, and that the patient was anxious to get well rapidly, Mr. H. Smith admitted him into the hospital. The patient in every other respect was a remarkably healthy man. On admission, Mr. Smith passed a No. 3 silver catheter, but with extreme difficulty. This was tied in the urethra.

Feb. 10th. The catheter keeps in the bladder, but he has had a rigor. In consequence of this the same catheter was retained, and he was ordered one grain of opium powder and two grains of quinine three times a day.

11th. No more rigors. At three p. m. to-day the patient was placed under the influence of chloroform, and a Holt's dilator was readily introduced, the largest tube passing down without difficulty. The stricture was split, a No. 11 catheter passed into the bladder, and the urine drawn off, scarcely any blood being mixed with it. The patient was placed in bed, and ordered to take the opium and quinine. He felt so well when he returned to bed that he was almost disposed to doubt that anything had been done to him.

12. He has had a good night, but passed no water until eight this morning. Very soon after this he had a most severe rigor, followed by vomiting. From this time until one p. m. he passed a considerable quantity of water highly charged with blood. At four p. m. he became very collapsed, blue in the face, and slightly delirious. He was ordered brandy, and hot turpentine stupes in the chest and loins.

13th. He has remained in much the same condition during the night. At eight a. m. the house-surgeon passed a catheter into the bladder, but only drew off a little blood. He gradually got worse during the day, and died at half-past four p. m.

The friends objected to a post-mortem examination, and therefore only the urethra and bladder taken out through the perineum could be examined. The urethra was found to be full of blood, and the strictured portion, which was in the bulb, was seen to be split by the dilator; but there was no laceration of the healthy portion of the canal, and there was no extravasation of the urine. There was no examination of the kidneys possible. The bladder was small and contracted. A false passage existed in the urethra, beginning in the left, just in front of the stricture, and running towards the right, not opening into the bladder.

This case presents features of grave interest, especially at a period when the treatment of stricture by rapid dilatation is being tested to a great extent, and I think it would be highly improper not to make the case public. The sudden and mysterious death following on an operation, unattended with the slightest difficulty, cannot fail to arrest the attention of even the most careless; and those of us who are too self-reliant are taught by it the humiliating lesson that disastrous results will follow those efforts which would appear most likely to be crowned with success, and least liable to failure. Here is presented to us all the conditions for success. A young man in remarkably good health, the stricture not complicated in any way, and not the least difficulty in performing the operation; careful watching afterwards, together with attentive medical treatment; and yet this man is completely killed, solely and entirely, as it were, by the shock of the operation, through it, by the impression made upon the system. That most terrible of all symptoms in the urinary organs—viz., suppression of urine, coming on a few hours after the operation, rendered the case hopeless, in my apprehension, from the first, and doubtless this was the condition which led to the patient's destruction; and although we know that it is a rare accident, still we must not for the future, in calculating the dangers of this operation, lose sight of the possibility of such an occurrence.

I regret to state that this makes the third case in which fatal results have followed upon this operation at King's College Hospital; and I am obliged to state that, so far as I have witnessed the execution of it in the hands of my colleagues and of myself, the proceeding has always been accomplished with great ease and strikingly little bloodshed or violence.

It is but right, however, on the other hand, to state that the results of my friend Mr. Holt have been wonderfully successful. He has informed me that

he has had only one death in three hundred operations, and I need not say that the greatest possible reliance may be placed upon that gentleman's word. I do not believe for a moment that he would conceal a single fatal case; therefore, those who might be inclined to underrate the success of this operation from the occurrences in my hands and in those of my colleagues must place against this bad fortune the extraordinary results produced by Mr. Holt. My friend Dr. Wiblin also informs me that he operated upon twenty-eight patients without a single bad result following, and was astonished when I told him of the disastrous event above recorded. I may mention that I have seen, and indeed recorded, as suddenly a fatal result after the simple passing of a catheter; and we know that sometimes the process of sounding or searching with a lithotrite has been followed by speedy death. These facts should be borne in mind by us whilst endeavouring to estimate aright such an occurrence as I have thought it my duty to record.—*Lancet, March* 18, 1865, *p.* 284.

60.—THE UTILITY OF THE ENDOSCOPE AS AN AID IN THE DIAGNOSIS AND TREATMENT OF DISEASE.

By Dr. Francis Richard Cruise, one of the Medical Officers of the Mater Misericordiæ Hospital; and Lecturer on Medicine in the Carmichael School, Dublin.

[The endoscope is an instrument for the direct exploration of internal organs by the sight. There is no portion of the human body into which a straight tube can be introduced in which it will not be found to be of service. The interior of the bladder, urethra, rectum, uterus, nasal fossæ, pharynx, larynx, and even in time, perhaps the œsophagus and stomach. It is, however, chiefly of use in diseases of the urethra. The following is a description of the instrument.]

In the first place, there is a tube or speculum, which is introduced into the cavity to be examined; and at one extremity of this a mirror of polished silver, perforated in the centre, is placed at an angle of 45°. The function of the mirror is to reflect the light, which is placed laterally, into the tube, so as to illuminate it to the end. As the calibre of the tube is very small, a most brilliant light is required, and, in order to obtain the best effects, it should be made to converge slightly upon the mirror. This convergence is attained by interposing between the light and mirror a plano-convex lens of suitable focal length.

The light being sufficient, the lens properly adjusted, the mirror bright and correctly placed with respect to the tube, it becomes a matter of facility for the eye of the observer, looking through the perforation in the mirror, to see clearly to the bottom of the speculum.

Such is the endoscope I have adopted, in principle identical with that used in 1824 by Fisher of Boston.

To obtain a suitable light is *the grand difficulty* in the construction of the instrument, and it has cost me no inconsiderable labour to overcome this obstacle. Even at the end of my prolonged experiments I am compelled to acknowledge that the discovery of the illuminating power I have selected was to a great extent fortuitous.

Experiments which I need not recall here, but which are familiar to those who have made investigations with polarized light, led me to the knowledge that one of the brightest illuminations which can be obtained by any means is that given off by the *thin edge of the flat flame* of a petroleum lamp. Moreover, the steadiness and intensity of the light are increased to the utmost by using a tall draught chimney, and by dissolving camphor in the petroleum, in the proportion of ten grains to each fluid ounce. I may further observe, that this light possesses the advantages of cheapness, freedom from danger, and total independence of such care in operation as to demand the aid of an assistant specially devoted to it. Similar attractions do not exist for the magnesium wire, oxycalcium or Drummond lights, or for that produced by *electricity*, which latter was proposed forty years ago for Fisher's apparatus *by Professor* Patterson.

Such then is the light I use and recommend, and I believe it will be found applicable to many other instruments. It is open, undoubtedly, to two objections. First, that it produces considerable heat; and secondly, the pencil of rays being extremely narrow, a very perfect adjusting apparatus is required to facilitate the movements of the lens and inclined mirror, so as to take advantage of it under all circumstances.

To obtain the best effect from the light a few precautions are requisite. The room in which the examination is made, so far as may be convenient, should be darkened; the lantern must be held steadily vertical whatsoever the position of the exploring tube may be, and the whole interior of the lantern and specula must be kept carefully blackened. I find that the best varnish for this purpose is made by adding a few drops of spirits of turpentine to some lampblack, procured by holding a piece of metal plate over a candle. Thus made, the varnish may be laid on with a camel-hair brush; it dries rapidly, leaving a dull black surface which reflects little or no light.

Besides a good illuminating power, we require for the endoscope a variety of specula, adapted to the exploration of different regions of the body.

For general use the urethral tube can hardly be excelled. It consists of a narrow portion, the size of a large catheter, which is just six inches in length; the remainder gradually dilates to form the part which fits into the receiving socket, wherein it is secured by means of a thumb-screw.

A wire stillete surmounted by a plug is provided, which can be inserted into the tube in order to facilitate its introduction into narrow canals such as the urethra. At one side there is an opening wide above and narrow below, intended to admit probes, carrying either cotton wadding or sponge to wipe the parts under examination, or caustic or other applications for the purposes of treatment.

Three or four sizes of these urethral tubes are required. They answer remarkably well for other situations, such as the uterine cavity, nasal fossæ, &c. By the aid of one I was lately enabled to ascertain, in a patient of my own, the precise attachment of a huge nasal polypus. In another case of obstructed nostrils, which Dr. Stokes requested me to examine, I was able with such a tube to exhibit a quantity of small gelatinous polypi, situated high up and far back out of sight, which produced all the annoyance.

For the exploration of the rectum a tube eight or nine inches long, and more than half-an-inch in diameter may be provided. With such an instrument Mr. Connolly was able to make a drawing of a stricture of the rectum in a patient now under my care. For the examination of the external meatus of the ear and membrana tympani, and for the performance of operations thereon, I have had an auriscope made. Already I have found it most useful. Although not comparable to a direct ray of sunlight, yet, in its absence it makes a reasonably good substitute, and the oblong lateral aperture allows of the introduction of suitable instruments, which can be manipulated under the eye of the examiner. During the early part of the past winter I was baffled from day to day by the darkness of the weather in my efforts to obtain a view of the membrana tympani in a lady whom I attended for a sudden attack of deafness. At last I used a rude ear speculum, fashioned at the time to suit my endoscope, and at once obtained a satisfactory view of the parts. Since then I have frequently used my auriscope with the most satisfactory results.

So far as my experience goes to carry out that of Desormeaux, the most useful field for the operation of the endoscope is the urethra. By its aid diseases of this part, otherwise merely subjects of conjecture and empiricism, are rendered as clear as to diagnosis and as satisfactory respecting treatment, as affections of the eye, or of any other external portion of the body patent to view. Before entering upon the study of the diseases of the urethra, I may premise a few words about the operation of examining it endoscopically.

If the urethra is free from constriction, which can easily be ascertained with a bougie, the best course is to examine it from the neck of the bladder to the glans penis. To do so we should place the patient in a suitable position. *In default of* a proper couch specially made for the purpose, I

place him reclining in an easy arm-chair, with the buttocks near the edge of the seat, and with the thighs well separated. Kneeling between the latter I introduce the tube with plug well oiled, until, by its direction, I know that it has passed the triangular ligament of the perineum. I then introduce the index finger of the left hand (previously well greased) into the rectum, and guide the passage of the tube through the membranous portion of the urethra into the prostatic region. So soon as it has traversed the latter, I withdraw the finger from the rectum, extract the plug from the tube, and attach the endoscope, which should previously be lighted and adjusted. Now, holding the endoscope in the left hand, I proceed gradually to withdraw it at the same time that I keep my eye closely applied to the eye-piece of the instrument.

As the tube passes each portion of the canal, the lining membrane comes into full view bit by bit. If a difficulty occur in seeing any portion it will generally be found to arise from oil, blood, mucus, or some other fluid, obscuring the surface. This difficulty is easily remedied.

Before proceeding to the examination a number of rods, with screw ends, should be prepared by winding the extremities with cotton wadding, and some should be provided with portions of fine moist sponge securely tied on.

When any obscurity of vision arises, one of the rods should be taken in the right hand, introduced into the central slit in the tube, and passed down to its extremity; a little pressure and a rotation or two suffice usually to take up the fluid, and the rod may then be withdrawn. Sometimes it will happen that the wadding or sponge is left behind, and to meet this emergency a little blunt hook should be at hand; this may be passed down to the sponge or wadding and rotated once or twice; though blunt it immediately becomes engaged therein, and by its aid the substance can be withdrawn. I prefer this hook to Desormeaux's miniature corkscrew, as being much safer and less liable to do mischief. During an endoscopic examination this wiping of the part may frequently need repetition. The screw end on the rods facilitates the removal of the cotton when soiled. It is hardly necessary to observe that the strictest care is required in the cleansing of the endoscope and its appurtenances, lest it ever might become a medium for the transmission of infection from case to case.

In the introduction of the endoscope catheter it is necessary to be careful not to enter the bladder lest a rush of urine fill it up and embarrass the examination. This is no needless precaution, for habit will teach that a straight instrument enters the bladder with almost as much facility as a curved one. In order thoroughly to investigate the canal it is well, while withdrawing the endoscope, occasionally, to reintroduce it a line or two, and to increase or diminish the light; thus views are obtained which may clear up points otherwise somewhat obscure.

I may also observe that the index finger of the right hand, well oiled, may, from time to time, be slipped into the rectum during the examination of the deeper portions of the urethra, as by its aid we are able to ascertain the position of the extremity of the tube, and to identify the portion which may be diseased. Thus I have frequently been able to ascertain, with positive certainty, morbid conditions of the prostatic, membranous and bulbar sections of the canal. In cases of stricture or disease anterior to the bulb it is not requisite to place the patient reclining on a couch—he may simply stand with his back against the wall, and the operator kneeling in front of him. I should here mention that the optical portion of my endoscope has been made for me by Messrs. Spencer of Aungier-street, Dublin, while the specula and other fittings were made partly by Charrière of Paris, and partly by Messrs. Thompson and O'Neill of this city. Messrs. Fannin have given me to understand that they intend to supply the instrument perfect in all details.

[While all other varieties of urethritis tend to a natural cure, the result of specific contagion frequently merges into a chronic form characterized by a slight colorless discharge, generally most noticeable in the morning. By the endoscope the mucous membrane from the bulbar to the prostatic portion *is seen to be* granular, exactly like the conjunctiva of a chronically inflamed eyelid.]

This once ascertained, treatment is a matter of facility. Cauterization which cures granular conditions of mucous membranes elsewhere—in the eyelids, uterus, &c.—will cure it here; and, thanks to the endoscope, can easily be applied to the exact spot diseased. Some caution is needed in the application of canstic for the cure of granular urethritis. The method which I prefer is as follows:—Having brought the anterior portion of the granulating surface into view, I first carefully wipe it dry with a portion of wadding carried down by a suitable rod. I next take a second rod, armed also with cotton wadding, and having dipped it in a solution of nitrate of silver or sulphate of copper (xv.—xxx. gr.—℥i), pass it down to the diseased part and hold it in contact therewith for a few seconds. I then withdraw the rod and endoscope catheter. This operation causes very bearable pain, which may be removed by a warm bath and moderate anodyne. In the milder cases when next we proceed to use the caustic—perhaps in the course of four or five days—we find that the portion last touched is healed and that the endoscope catheter penetrates further than before without causing pain. A second portion may now be cauterized, and in like manner, by degrees, the whole diseased surface cured. Under the influence of the caustic the granulations disappear—certainly, though rarely quickly—submucous effusion is absorbed—constriction disappears—and finally, the parts return to a condition of health from which they are not liable to relapse spontaneously.

[Much may also be learnt regarding the nature of strictures. The orifice of the stricture can in most cases be easily made out.]

When the stricture is brought fairly into view, and its orifice discovered by means of the probe, the operator can see the exact disposition of the indurated parts, and can easily, under his sight, divide the stricture, confining his incisions to the indurated and callous parts. M. Desormeaux uses for the purpose a miniature button bistoury, with a handle. I would venture to propose instead a blade somewhat different, which has a long probe point, and a short cutting edge, which is blunted towards its posterior extremity, so as to limit the incisions to the contracted parts, as in Maisonneuve's urethrotome. Such precaution is hardly necessary, as the stricture can be incised under the eye of the operator, but it is an additional safeguard against an over-extensive division of the parts. The stricture being in view, and the orifice discovered, this knife should be introduced carefully into the catheter of the endoscope, so as to avoid injury to its cutting edge by contact with the interior of the tube. The catheter may then be rotated, so as to bring its slit into the requisite position to enable the operator to turn the edge of the urethrotome in any direction he may think desirable. Now its probe point may be engaged in the orifice of the stricture, and the instrument gently pressed onwards until the constricted parts yield. The after treatment consists in the use of quinine and opium to prevent urinary fever, and careful dilatation to maintain the incision patent.—*Dublin Quarterly Journal, May*, 1865, *p.* 329.

DISEASES OF THE SKIN.

61.—ON THE NATURE, THE VARIETIES, AND THE TREATMENT OF ECZEMA.

By Erasmus Wilson, Esq., F.R.S. (From a paper read before the British Medical Association, at Cambridge, Aug., 1864.)

The prevalence of eczema is so considerable, that its name has taken root among the population, even as a "household word." There is scarcely a medical practitioner in Great Britain who has not, at this moment, a case of eczema under treatment; or who, within a limited period of time, has not had such a case brought within his observation. Eczema is common to all classes of society; is restricted neither by age nor sex; is equally frequent in the male and female; and is met with in the infant of a few weeks old, as

commonly as in those who have reached the more protracted term of human existence. In one thousand cases of cutaneous disease of every denomination, there are 298 instances of eczema; very nearly one in every thirty, or, in other words, thirty per cent.

What is eczema? Eczema is an inflammation of the skin, attended with alteration of its structure, and derangement of its functions. It is more vascular than in health, its vessels being in a state of congestion; its sensibility is morbidly increased, sometimes taking on the character of itching, tingling, or smarting, and sometimes that of pain; it is thickened by infiltration of serum into its tissues; is sometimes fissured and sometimes œdematous; it exudes a serous lymph at various times and in various quantity, sometimes in excessive abundance; the epidermis is sometimes raised into vesicles, sometimes wholly removed; and is reproduced unhealthily, so as to form muco-purulent secretions and squamæ of various size; and sometimes is replaced by a crust of greater or less thickness, resulting from the desiccation of the morbid secretions.

But eczema rarely presents all these characteristic symptoms in an equal degree; and the diversity of the symptoms, and the modification which those symptoms undergo in different parts of the skin, in different constitutions, and in different temperaments, are the foundation of its varieties. As the varieties of eczema are founded on the prominence of one or other of the symptoms of the disease, they may be easily designated and as easily recognised. For example: when the prevailing sign is redness, the eruption is an eczema erythematosum; when the pores of the skin are erected into the form of papulæ, the disease is an eczema papulosum; where there are vesicles, the variety is eczema vesiculosum; where excessive exudation of a serous or colourless lymph is the leading sign, the case is one of eczema ichorosum; where there are pustules mingled with the other signs, it is an eczema pustulosum; and where, as in the chronic forms of the affection, the epidermis is scabrous and scaly, the disease is an eczema squamosum. These terms comprehend the principal form of eczema dependent on symptoms; but additional terms suggest themselves naturally for different conditions; for example: the skin is sometimes cracked and fissured—eczema fissum. Sometimes the thickening and condensation of the diseased tissues in a chronic stage of the affection suggest the terms eczema sclerosum and eczema verrucosum. Sometimes the state of the part is aptly expressed by the term eczema œdematosum; sometimes, from the nature of the secretion, eczema mucosum. And, more than once, we have met with a case to which, on account of the severe pain by which it was accompanied, the term eczema neurosum was correctly applicable.

It is rare to find an eczema perfectly simple in its eruption, and capable of being represented singly by any one of the terms already mentioned. More frequently, too, or indeed several of the forms may be present at the same time, and sometimes, even the whole. For example: in one case we may have an erythematous eczema combined with papulæ, which we should then distinguish by the name of eczema erythematosum et papulosum (eczema lichenodes); or, it might be an eczema erythematosum et ichorosum; or, if the exudation of fluid from the inflamed surface were the most striking feature, we should name it an eczema ichorosum, which might be, but in a less degree, erythematosum et papulosum, or even pustulosum. Or, the case might be one of eczema vesiculosum et erythematosum; or, an eczema squamosum (eczema chronicum), which is, at the same time, in part, fissum; and in part ichorosum. These terms have the advantage of being simple, expressive and clear; they rise to the mind as freely as the terms: red, pimply, cozy mattery, or scaly; and, they render the narrator intelligible to every one conversant with medicine and medical language; while the commoner applications eczema simplex and eczema rubrum, convey to the uninstructed in cutaneous pathology, either an erroneous idea or absolutely nothing at all.

The names of eczema are also derived from situation; from the part of the eruption. For example, we have eczema cap-

eczema faciei; eczema aurium; eczema palpebrarum; eczema oris et labiorum; eczema axillarum; eczema mamillarum; eczema umbilicale; eczema inguinum; eczema pudendi; eczema perinæi; eczema ani; eczema articulorum; eczema manuum et pedum; eczema dorsi manus; eczema palmare et plantare; eczema digitorum; eczema inguinum, &c. Moreover, we are taught by experience that in certain situations we shall be most likely to meet with one or other of the established varieties of the disease. For example: eczema capitis et aurium are commonly ichorosum in a recent attack, and squamosum at a later period; eczema articulorum, eczema axillarum, eczema inguinum, and eczema pudendi, are also very frequently ichorosum, and sometimes mucosum. Eczema of the fleshy parts of the forearms and legs is not unfrequently vesiculosum; as is also eczema digitorum. Eczema dorsi manus is usually papulosum; while eczema palmare is squamosum et sæpe fissum.

The extent of the eruption is also to be considered in designating its varieties. It may be general or it may be partial. When partial, the patches of the disease may be single or multiple; they may be defined (figuratum); or diffused (diffusum). Sometimes they suggest the idea of the dimensions and figure of a piece of money, nummulare; and sometimes they are bounded by a prominent ridge (marginatum) and spread by the circumference, while the inflammation subsides within the included area; this is especially the case in a curious form of eczema marginatum common in India, where it is known as the Burmese ringworm.

Willan and Bateman include all the varieties of eczema under the three heads; solare, impetiginodes, and rubrum, corresponding with the eczema vesiculosum, eczema pustulosum, and eczema ichorosum of our improved nomenclature. Hardy proposes four varieties; namely, simplex, rubrum, fissum (fendillé), and impetigo. Hebra employs the suggestive terms squamosum, papulosum (lichenodes), vesiculare (solare), madidans (rubrum), and impetiginosum.

It will be perceived that Hebra makes eczema squamosum the first of his forms of eczema; whereas I take it to be the last. Let me explain. Eczema erythematosum is commonly accompanied with desquamation; and when the eruption is general, this is remarkably the case. Now, Hebra selects the moment when it is coated over with scales as the type of this form of the disease. But, it must be remembered that the scales may be disposed of in a variety of ways; there was a time before which they were formed; there is a time when they have ceased to be produced; there is an intermediate time when they may be removed artificially by dressings, poultices, or baths. What are we to call the disease when the scales and desquamation are no longer present? The answer is plain; the redness, the erythema remains; indeed, the erythema is always there; from the beginning, before the scales existed, at the end, after they exist no more; hence, the term "erythematosum" appears to me to be more appropriate than "squamosum." Again, there are forms of eczema in which there is redness, but nothing deserving to be called scales; and to these cases, which are numerous, the term "erythematosum" is especially applicable.

On the other hand, when eczema reaches its latest period, when it is chronic, there is still redness; but scaliness and desquamation are a more predominant character than redness; and it is to this period, that I prefer to attach the term "squamosum." Eczema squamosum, I regard as the chronic stage of eczema; whilst eczema erythematosum, which may also be chronic, applies with especial aptitude to the earliest stage of the eruption. Eczema squamosum is, therefore, in my view of the pathological indications of the disease, an eczema chronicum squamosum, and is the true representative of the much-abused term psoriasis. Psora represents the more active, and especially the exudative forms of eczema; and psoriasis, its chronic, its dry, and its desquamating form.

The Romans, who designated eczema by the name of scabies, drew attention to a characteristic distinction between its moist and its dry form—scabies humida and scabies sicca. The Greeks appear to have had the same object in view in employing the words psora and psoriasis; the former term corre-

sponding with scabies humida, the latter with scabies sicca. The distinction is a radical one, and of the first importance in the study of eczema. Eczema siccum, which is represented by eczema erythematosum, eczema papulosum, and eczema squamosum, is specially distinguished by an absence of exudation, or by its presence in a very slight degree; whereas eczema humidum, represented by eczema vesiculosum, eczema ichorosum, and eczema pustulosum, is remarkable for the exudation of a serous and sometimes a muco-purulent fluid, often in astonishing quantity—a positive flux. According to our observation, the dry forms of eczema more than double in frequency those of the moist or humid kind; therefore a classification that assumes for eczema a definition dependent on exudation as a pathognomonic sign, must, of a necessity, be incorrect.

Treatment of Eczema.—If the foregone premises with regard to the causes of eczema be correct, the treatment of eczema resolves itself into a treatment of debility—a treatment of constitutional debility, and a treatment of local debility—and the influence which we are called upon to exercise over our patients is one which shall have for its object to restore power, and thereby to regulate function and to confirm health; for health is nothing more than correct function united with normal power.

In general terms, our constitutional treatment must be directed to the regulation of the functions; and, concurrently with regulation of functions, to the restoration of the tone, the vigour, the vitality of the general system. In the most numerous group of cases, those which depend upon debility of assimilation, our attention is principally directed to the digestive organs and the secretions. In the next most numerous group, those which are due to nutritive debility, the digestive and secreting organs must also be carefully examined, although they are less likely to be faulty than in the preceding group; and we are enabled sooner than in the former case to resort to our strengthening remedies. The same remarks apply to the third group, originating in nervous debility; while in these latter, we have to deal with the special indication of weakened power of the nervous system.

If we turn our attention to the tonics, which may be found suitable for the treatment of eczema, we find: simple vegetable bitters, cinchona and quinine, chalybeates, the mineral acids—hydrochloric, nitric, sulphuric, and phosphoric; and above all, arsenic. Among simple vegetable bitters, with what respect must we speak of gentian, calumba, orange-peel, quassia, chirayta, chamomile, and hop; in infusion or decoction; in extract or in tincture; in combination with the mineral acids, or with ammonia, soda, or potash. How admirably cinchona and quinine perform their part; cinchona in decoction or tincture, or liquor, with sulphuric acid or with ammonia; and quinine in solution, with an excess of sulphuric acid. How perfect a remedy is one grain of quinine, with one drachm of sulphate of magnesia, seven minims of dilute nitric acid, and an ounce and a half of infusion of roses, both as a gentle aperient, a corrector of acidity, a promoter of mucous secretion, and as a tonic. Then the chalybeates, iron and its salts, the sulphate, the tincture of the hydrochlorate, the citrate, the superphosphate, the iodide, and the citrate of iron and quinine. Next, the mineral acids combined with the bitter tonics, and especially the nitro-muriatic and the phosphoric. Again, as a nutritive tonic, we have the cod-liver oil; and our list of remote predisposing causes will discover several sections in which that remedy is peculiarly applicable; for example: strumous diathesis, excessive growth, and sexual excess.

All that precedes applies to any disease of the human frame; whether it be of the lungs, the heart, the liver, the brain, the kidneys, or the womb; the treatment now under consideration is catholic and not special; its aim, to bring back normal function, and so to restore health; it is the routine of the physician's art; but there is a remedy which possesses a special influence on the skin, and has long been held sacred in cutaneous medicine, namely, arsenic. When all has been effected that can be accomplished by aperients, alteratives, derivatives, and tonics, then comes the reign of arsenic; and at that moment, arsenic is triumphant. But arsenic must be regarded, not as *our common weapon, but as our reserve*; to be administered when it can do *no harm, and when there is every probability of its doing good.* Arsenic is

a tonic, a tonic which influences chiefly the nerve-substance, and not only gives force to, but improves the nutrition of the nervous matter; in this way it operates upon the trophic system of nerves throughout the economy; and in cutaneous medicine we have evidence of its remarkable powers upon the skin. Arsenic accelerates those actions of the skin which tend to its nutrition and renovation, and therein it becomes a healer of its eruptions, and a valuable aid to the cure of eczema. But arsenic, above all medicines, must be handled with judgment and care; it must be jealously watched, lest it give rise to any unfavourable symptoms, and its use must be suspended at once if there exists even a doubt of its healthful operation. Above all, it is to be borne in mind that only at the right moment is arsenic applicable, and that right moment can only be determined by watchful care, or by long experience.

Our *Pharmacopœia* is rich in preparations of arsenic; but the most valuable are, the liquor potassæ arsenitis, and the liquor arsenici chloridi; the dose of the former applicable to an adult suffering with eczema, is two to four minims; and of the latter, double that quantity. Some years ago, in a paper on Eczema Infantile, I laid before the British Medical Association a formula for the administration of Fowler's solution; and experience has assured me that no better form can be employed for its administration.* Arsenic should always be administered in small bulk, and the formula in question gives one drachm of fluid as the dose, and always on a full stomach, or, if possible, in the midst of a meal. The object of these precautions is obvious; arsenic is intended especially as an improver of nutrition, and therefore should be made to enter the stream of blood with the chyle; in its nature it is an irritant to the mucous membrane, and, therefore, it should be administered at a time when the lining membrane of the stomach is coated with mucous secretion, and at the same time when, by mixing with the mass of the meal, the remedy is largely diluted.

All the processes of renovation which depend upon nutrition are slow; hence, as we look for the good effects of arsenic in the improvement of nutrition, we must be prepared to continue its use for a considerable time. This is another reason for administering arsenic in small doses, independently of the suggestions to the same effect gathered from experience. Indeed, by employing it in larger does, we naturally frustrate the objects which we ourselves have in view; we set up the irritant action of the medicine; we excite nausea, sickness, pain in the stomach, or alimentary canal, prostration of nervous power, or cough, and then we are compelled to suspend it. While, on the other hand, administered with judgment and discretion, there is no safer medicine in the *Pharmacopœia;* and, at the same time, in my opinion, there is none to equal it in excellence and usefulness.

To be a successful practitioner in the treatment of eczema, a medical man must be an accomplished physician; to manage the local treatment with success, he must also be an able surgeon. The local treatment of eczema must be conducted according to the general principles of surgery. The inflamed part must be soothed in the acute stages of the disease, it must be supported and stimulated during the chronic stages, and it must be excited to a new action in the most chronic stage of all. To soothe, we must employ water-dressing, unguents, cerates, well-adjusted bandages, and rest. To give local tone, we must have recourse to stimulant applications of various kinds.

The water-dressing is useful where there is heat, pain, itching, or tension, but should not be continued beyond the period during which those symptoms exist; for, when prolonged for too great a time, it lowers the tone of the tissues and perpetuates the eruption. In our list of remote predisposing causes, it will be seen that one amongst them is "moisture with heat," the exact condition which at one moment of the disease we employ with advantage in the use of the water-dressing. Whereas, we could hardly adopt any more certain means of producing eczema artificially, than by the application for a long time of water-dressing to a healthy part of the skin. This operation is often seen in practice, in the eruptions which follow the lengthened use of poultices.

* ℞. Vini *ferri* ʒiss.; *syrupi simplicis* ʒiij.; liquoris potassæ arsenitis ɱj.; aquæ puræ ʒij. *Misce. Fiat mistura; ʒj. pro dosi.*

When the local inflammation partakes of the sub-acute character rather than the acute, and when the water-dressing has effected the purpose for which it was applied, we may have recourse to cerates or ointments. As a preliminary to both of these appliances, all crusts and sordes should be carefully wiped or washed away from the inflamed part; if they be dry and hard, the water-dressing or the ointment-dressing may be applied with a view to soften them, and then they should be removed, and the dressing with the ointment nicely adjusted, and, wherever practicable, secured in position by a bandage: where this cannot be done, then we must have recourse to adhesive straps. In selecting an ointment, the best with which I am acquainted is the benzoated ointment of oxide of zinc. It should be spread thickly on the lint; the dressings should be shaped so as not to overreach the sound skin; and the roller of elastic cotton bandage should be put on smoothly, and with a view to produce equable pressure on the eruption, and support to the vessels of the limb. On a part of any extent it is more convenient, and we avoid wrinkles thereby, to apply the dressing in slips, and dispose them in the manner of a many-tailed bandage; and when a limb affected with eczema is carefully packed up in the manner described, it may be left for twenty-four hours, and in more chronic cases for forty-eight hours, without disturbance; then it should be unpacked, re-dressed, wiped with a soft napkin to remove sordes, and packed up again for a similar period. While packed up in this way, the itching of an eczema commonly ceases, and the patient is saved from the annoyance and suffering of that disagreeable symptom, while the skin returns gradually to its healthy condition. If there should be heat and burning of the limb when packed, the bandage may be sopped with water, and then we get the advantage of the water-dressing superadded to the tonic treatment.

Where great irritability prevails in the constitution of the patient, we require to have recourse to sedatives; and where irritability is present in the part, we may find it difficult to contrive a dressing which will perfectly suit the eruption. Sometimes grease of all kinds acts as an irritant, and then we are driven for a while to the use of lotions or powders. Sometimes a sedative, such as the acetas plumbi, added to the ointment, relieves the irritability; sometimes a more stimulating application, such as the unguentum resinæ flavæ, or the ointment of juniper tar. At other times, we may put an end to the irritability by pencilling the surface with a weak solution of the nitrate of silver in nitric ether. Again, we rarely fail to relieve the irritability by undressing the part, washing it thoroughly with the juniper tar soap, drying it, and dressing and packing it up again as before. The successful treatment of eczema needs many resources; but only such as may be explained on the recognised principles of physiology and surgery.

The erythematous form of eczema yields very kindly to the benzoated ointment of oxide of zinc: so also do the moist forms of the eruption, the dressing being aided by moderate compression with a bandage. Eczema ichorosum must be followed up patiently with this plan of local treatment, until the ichorous secretion ceases to be formed, and the eruption heals. But the chronic forms of the disease represented by eczema squamosum, require a stimulant treatment. Washing with the juniper tar soap, dressing with the unguentum resinæ flavæ, pencilling with a solution of nitrate of silver, or a strong lixivium of potash. Whenever these stronger local remedies are used, the eruption should be subsequently dressed, *more dicto*, with the benzoated ointment of oxide of zinc, and then carefully packed up.

In the moist forms of eczema, there is always a considerable quantity of serous lymph present in the tissues of the skin; and, until that serous fluid is removed, either by absorption from within or by excretion from the part, the skin cannot return to its normal state. It is to the presence of this fluid infiltrated in the substance of the skin, that the chronic forms of eczema owe their thickening, their induration, and their rupture; the latter giving occasion to fissures and cracks, and being most conspicuous where the skin is naturally thick and wrinkled, as on the hands and fingers, or the feet, behind the ears, along the borders of the mouth, and in the cleft of the podex. For *this state of* the skin, in an aggravated form, there is no remedy to compare *with a solution* of caustic potash. That application seems to purge the skin

of its abnormal fluids, to promote absorption, and to give energy to nutrition; and it is amazing with what rapidity the cracks will heal, and the most chronic state of disease get well under this treatment.—*British Medical Journal, Nov.* 19, 1864, *p.* 567.

62.—ON THE TREATMENT OF ECZEMA.

By R. H. Meade, Esq., Senior Surgeon to the Bradford Infirmary.

[Mr. Meade quite agrees with Erasmus Wilson that eczema is generally a disease of debility. Mr. Wilson's remedy *par excellence* is arsenic. Mr. Meade more frequently uses the bichloride of mercury and antimony, and his reason for this is, that though it mostly arises in weak states of constitutions, it is in its own essence inflammatory, and therefore]

While the strength is supported by generous diet, an alterative remedy, like bichloride of mercury, which is so generally efficacious in chronic inflammatory complaints, may be advantageously given, and even combined with tonics.

With regard to antimony, it is less generally applicable in eczema than corrosive sublimate, but it will sometimes be found very useful; and in some obstinate cases, I have seen these two remedies exceedingly efficacious in combination. The form of the disease in which antimony administered singly seems more particularly applicable, is that in which it is acute in its character, and occurs in persons of a full inflammatory habit, who are also, perhaps, subject to gout or rheumatism. Tartar emetic given here, in combination with aperient salines and magnesia, will often cure the complaint very quickly.

We frequently see very annoying cases of eczema in oldish people, in whom the skin round the anus and about the scrotum or labia is affected. I have found the combination of bichloride of mercury and tartar emetic in the same mixture particularly useful in these cases, and sometimes cure them in a week or two, when they had resisted other treatment for two or three years. The dose of bichloride which I generally find sufficient is one-sixteenth of a grain, given three times a day, with one-eighth of a grain antimony; in some demulcent mixture, as decoction of sarsaparilla or dulcamara.

Eczema very frequently occurs in infants, breaking out soon after birth, and becoming aggravated as teething commences. Most of these cases have, I believe, an hereditary origin—one or the other parent having a disposition to the complaint. These young subjects are mostly weak, and require strengthening treatment. Unless they are suckled by a strong mother or wet-nurse, they should have good beef-tea or broth given them, in addition to the milk; and have little or no farinaceous food. These are cases which will be especially benefited by corrosive sublimate. It must be given in very small doses, and continued a long time; it may be taken with occasional intermissions for months together, with advantage. A sixtieth of a grain, or even less, in a very young child, taken twice a day, with a little fluid extract of sarsaparilla and glycerine (which is a very good demulcent), will mostly check the complaint. Should the bowels be at all irritated by the mercury, a little opium may be combined with it.

In recommending bichloride of mercury so strongly as a remedy for eczema, I do not wish it to be inferred that I am bringing forward anything new; for I believe that this medicine has been long and largely used in this complaint, by the medical officers of the London Hospital for Diseases of the Skin. Having long used it in my own practice, I am anxious to bear testimony to its efficacy; and can recommend those of my medical brethren who are not yet acquainted with its virtues in the treatment of eczema, to give it a fair trial, both in combination with antimony and without it.

In the few remarks which I have made upon the treatment of eczema, I have made no allusion to external applications. I now mention them, for the purpose of saying that I by no means underrate their importance; but, as I am not professing to write a paper on the general treatment of this complaint,

but only wishing to call attention to one or two particular remedies, I think it unnecessary to enter into the subject.—*British Medical Journal, Dec.* 17, 1864, *p.* 673.

63.—ON ECZEMA OF THE EYELIDS, CONJUNCTIVA, AND CORNEA.

By Furneaux Jordan, Esq., Surgeon to the Queen's Hospital, Birmingham.

Many observers, and especially writers on diseases of the skin, have considered ophthalmia tarsi to be simply eczema of the lids. Dr. Mackenzie has pointed out that scrofulous, or as he terms it, phlyctenular ophthalmia, is frequently associated with eruptions on the skin. It is the object of this paper to show that not only ophthalmia tarsi is eczema of the lids, but that granular lids, a peculiar swelling of the sub-integumental connective tissue of the lids, lippitudo, strumous ophthalmia, certain forms of simple or catarrhal ophthalmia, keratitis and strumous keratitis, and certain ulcers on the cornea, are merely varieties of eczematous disease. Cases of extreme, firm, indolent, pale or pinkish swelling of the lids, occur occasionally, the only cause of which is eczema of the margins of the lids. The eczema may be very slight, or it may pass away quickly, and leave only the swelling behind. Unchecked eczema of the eyelids terminates in lippitudo just as persistent and progressive eczema of the cornea produces pannus. Both these conditions are analogous to the eczematously red, swollen, and moist condition of the skin which may persist for an indefinite period. Eczema of the conjunctiva presents many important features. The so-called strumous ophthalmia may be regarded as chronic eczema. The several stages of pimple, vesicle, ulcer, or thickened patch, admit of indisputable demonstration. In acute eczema of the conjunctiva, there is for a few days a uniform scarlet-colour; then a crowd of vesicles, which soon pass away, and leave an irregular or patchy redness—each patch, however ill-defined, having a redder, thicker, and possibly ulcerated centre. These cases have a slight muco-purulent discharge, and are always tedious. If treated as eczema, they speedily recover. The so-called keratitis, or strumous keratitis, is eczema of the cornea. When vesicles, white patches (necessarily white because of the anatomical structure of the cornea), or ulcers occur on the cornea in conjunction with vesicles on the conjunctiva, the term scrofulous ophthalmia is commonly used. If the same pimples (necessarily flat), vesicles, patches, or ulcers, occur on the cornea alone, especially near its centre, the term keratitis is applied, notwithstanding the symptoms are similar, and notwithstanding that there is usually, if it be carefully sought for, evidence of eczema of the lids or face, or ears or scalp. The characters of eczema of the cornea are quite as typical as they are of eczema elsewhere. The several varieties of eczema of the cornea, conjunctiva, and lids are combined in a great variety of modes. They are much more frequently combined than not, and very frequently indeed associated with cutaneous eczema in its favourite localities. Eczema is often limited to sites as small as the cornea. The treatment should be directed to eczema. Its chief features are non-stimulating diet and alkaline medicines, with a little iron added in most cases. If the lids are affected, as also in pannus, lippitudo, and granular lids, a little of any of the "eczema ointments" may be used, with the customary attention to details; if much photophobia, a little morphia may be given in the morning.—*Lancet, Jan.* 21, 1865, *p.* 62.

64.—CASE OF SYCOSIS MENTI VEL MENTAGRA.

Under the care of Professor Laycock, at the Edinburgh Royal Infirmary.—(Reported by Dr. Dyce Duckworth, Resident Physician.)

[The patient was twenty-one years of age, and stated that two years before admission a scale formed under the left angle of the mouth, in consequence *of cold, and this gradually spread.*]

On adm [illegible] lip and chin were seen to be covered with a yellow

scab matting the hair of his moustache and shortly-clipped beard. In parts small isolated pustules were seen, turgid with pus and having a hair issuing from their summits. On closer examination, it was obvious that the parts exclusively affected were those from which the beard and moustache grew; thus the eruption occurred at the angles of the mouth and at the parts below the centre of the lower lip—the site of the "imperial," but not in the whiskers. No pain was complained of, and there was no itchiness.

He was ordered to foment the chin well with warm water, and then to apply a strong alkaline lotion consisting of an ounce of the bicarbonate of potass to four of distilled water. This was with the view of dissolving the crusts. The hair being closely clipped to the level of the skin (for this is always necessary in using any lotion to the hairy surfaces), the lotion was applied on lint by means of a mask of oiled silk, which passed under the nose, included the entire chin, and was fastened behind the head. The parts were fomented with warm water twice daily. Dr. Laycock diagnosed the affection as sycosis. The fig-like character of the disease was more especially manifested on the 29th October, when the crusts and scabs being removed, the pustular points were seen standing out on a darkened red base, presenting very much the appearance of an open fig. The alkaline lotion was continued for a few days more. Dr. Laycock directed attention to the following points in connexion with the case. He stated that this affection is considered to be of parasitic origin and infectious, and cited cases which had occurred in his own experience which proved the truth of this belief.

"If this were so, then why did the disease not attack this man's whiskers?" The disease was proved to be communicable from chin to chin, but the whiskers escaped this particular form. He stated that there was a form of sycosis which attacked the whiskers solely, and not the skin. According to the Professor, there are three distinct portions of the facial hair, and the development of each is found to differ in different men. There is first the jaw hair or masseter beard; some men have this alone and no other kind. Next there is the chin hair, with that of the upper lip—the moustache—constituting the muzzle hair or beard. This is first developed, and very commonly the only part of the facial hair that is developed. And lastly there is the face beard, or that between the jaw and the muzzle beards, over the zygomatic and buccinator muscles. The face beard most commonly accompanies the jaw beard. The muzzle hair is in relation with both the ovaria and testes, and is apt to be fully developed in women sometimes, as in the *Fœminæ barbatæ*.

It appears that there are three forms of sycosis which may attack different portions of the beard.

Dr. Laycock pointed out how skin affections generally, and those affecting the hair regions in particular, follow this law. Even pediculi have their proper hairy habitat. He believes that there are special centres of innervation and nutrition for each of the before-mentioned portions of the beard, and are indicated not only by the varying development but also by the differences of each as to earlier and later greyness; and further, that there are definite peripheral nervous distributions in these parts which probably the scalpel will never reach. It is this consideration, then, which points to the pathology of these somewhat anomalous cases. We must believe that there are certain regions rendered liable to the morbid invasion by defective nutrition, while others, perhaps in immediate proximity, though in another nervous area, remain free from and unaffected by the existing morbific influences, because their nutrition is healthy. Now, in this case the disease never spread from the muzzle hair.

The alkaline lotion was discontinued, and it was decided to treat the case with arsenic locally. There was ordered, ℞. Glycerinæ, f. 3x.; liquoris potassæ arsenitis, f. 3ij. M. ut fiat lotio. To be brushed into the affected parts night and morning after ablution.

Two days after this treatment had been practised, the chin looked red and irritated. The arsenical glycerine was discontinued, and the alkaline lotion ordered to be reapplied.

November 7. The parts are improved in appearance. The arsenicated glycerine is being employed again.

13th. It seems that the disease has been spreading a little below the chin, and the pustules lie on an irritable red base. General health good. To discontinue the arsenicated glycerine lotion, and apply nothing to the parts. Dr. Laycock showed how the arsenical preparation had acted in inducing an active inflammatory condition; this was a step towards the removal of the disease. It is a peculiarity of arsenic that the inflammation it excites entails a new and modified action of the part subsequently. Sufficient inflammatory action was induced in this instance, and it was now prudent to suspend the remedy, and wait for the ultimate results of it.

19th. The pustules have dried up and formed crusts in various portions of the chin. The skin below is much less red. Ordered to wash the chin several times a-day with olive oil and the yolk of an egg mixed, instead of soap, and to foment the parts with warm water subsequently. The hairs to be clipped short with sharp scissors. The patient was decidedly better from this time, and but few pustules were seen to be forming. They were most numerous in the centre of the chin over the site of the imperial beard.

In two days after this all the scabs were removed, and a thickened, discoloured base was to be seen where they formerly existed.

30th. The chin and upper lip are now nearly clean. Daily ablution with oil and yolk of egg was practised, and the hair kept short by clipping. A few pustules still to be seen in the centre of the chin, and one or two below it. These were ordered to be punctured with a needle and let out, afterwards to be touched with the arsenicated glycerine.

December 5th. The above treatment has been practised, and the same process has occurred which was before described, viz., the formation of a crust, and resolution of the inflammatory action. The parts to be washed twice daily with oil and yolk of egg and fomented.

13th. All the pustules have disappeared, and the skin is merely reddended. Discharged this day.—*Medical Times and Gazette, Dec.* 17, 1864, *p.* 650.

65.—ON THE ACTION OF CREASOTE IN THE TREATMENT OF SYCOSIS.

By Dr. Masse.

Dr. Masse, in a note on this subject, says:—"The patient whom I was authorized to treat, in the large Hôtel-Dieu at Montpellier, was a young soldier who had a pustulo-tubercular eruption upon the cheeks and chin; he exhibited, moreover, *herpes circinatus* upon the back of his hand. He had contracted this malady while being shaved by a barber whose razor or linen, plentifully supplied with the parasites, had thus communicated the same affection to five or six other soldiers. I coupled microscopical examination with the information I had acquired as to the etiology of this affection, with a view of completely investigating the nature of it. I discovered, in the hypertrophied hair-bulbs, spores and tubes of the *mycelium* of a veritable *Microsporon mentagrophyte*. The treatment employed consisted of lotions, composed of 50 grammes of water, 50 grammes of alcohol, and 50 centigrammes of creasote, and which were used twice a day for a week. At the end of this period the amount of creasote in the lotion was doubled, the latter being applied three times a day. By the end of another week the pustulo-tubercular eruption had completely vanished. Microscopical examination failed to exhibit any parasites, and the invalid had completely recovered. This novel method which I came to make trial of cannot be said to be empirical treatment, since it is but a medical adaptation of the recent investigations of M. Béchamp upon the action of creasote in fermentation, and according to which creasote was found under these circumstances to check the development of the spores of the vegetable cryptogams, as well as of the ovules of *the infusoria*. The cryptogamous parasites appearing to me, viewed with *regard to their* organization, to differ but little from the mould developed du-

ring fermentation, I thought that creasote might in a like manner arrest the formation of parasite spores in diseases of the skin. By destroying the source of a malady we should effect a cure. Creasote does not immediately destroy the cryptogam of which the mycelium is already developed, but it renders the spores incapable of germinating; after a short time the mycelium becomes dried up, and the parasite disappears."—*Gaz. des Hopitaux; and Glasgow Medical Journal, January*, 1865, *p.* 482.

66.—ON THE TREATMENT OF SCABIES.

By Dr. J. F. Nicholls, Surgeon to the Royal Wilts Militia.

[Dr. Nicholls has tried the following plan in several hundred cases, and where the remedy has been properly prepared and used it invariably effects a cure.]

The contrast is extremely great between this and the old plan of treatment, as was shown by the men of my own regiment being only kept in hospital two or three hours, and then sent to their duty, while in the same hospital, men of other regiments treated according to the old method, have been kept for eight and ten days, nearly naked, with only a blanket or two saturated with sulphur ointment to cover them, and in an atmosphere of the most awfully offensive character.

The remedy which I have adopted is prepared by boiling one part of quicklime, with two parts of sublimed sulphur, in ten parts of water, until the lime and sulphur are perfectly united; during the boiling it must be constantly stirred with a piece of wood, and when the sulphur and lime have combined, the fluid is to be decanted and kept in a well-stoppered bottle.

The way in which I have it applied is as follows:—Every part of the body, with the exception of the face and scalp, is well rubbed with the preparation for half an hour, and then washed with soap and hot water. After the application an entire change of clean linen and clothing is put on. The patient's linen is disinfected by being soaked in boiling water, and his clothes are freed from contagion by being subjected to a temperature of about 200° Fahr., in an oven, for a short time. When using the remedy for young children I have had it diluted, by mixing one part of the solution, prepared as above, with three parts of water.—*Medical Mirror, March*, 1865, *p.* 169.

67.—A NEW AND SPEEDY METHOD FOR THE CURE OF SCABIES.

Dr. Decaisne, physician to the garrison at Antwerp, has addressed a communication to the Belgian Academy of Medicine regarding an almost instantaneous mode of cure of scabies in man. He proposes, says the *Presse Médicale Belge*, to substitute for all the therapeutical agents hitherto employed for this disease, the *oil of petroleum*, which, according to him, instantaneously kills the parasite, while it is, at the same time, a disinfectant against the *larvæ* which are wont to be found in the wearing apparel and bedclothes. This communication has been referred to a Commission composed of Messrs. Thiernesse and Thiry.—*Glasgow Medical Journal, January*, 1865, *p.* 482.

68.—ON THE TREATMENT OF SCABIES.

By Dr. John Lowe, King's Lynn.

The compound tincture of iodine is the cleanest and surest cure for scabies, where the surface affected is not very extensive. It should be applied once a day with a camel-hair brush. At the first application the clusters of spots may be painted together; but afterwards it will be necessary merely to touch any fresh ones which may appear. By this method the acari are effectually sealed up, and prevented from spreading over the surface. I have used the

tincture alone in the cure of scabies for the last six or seven years, and only in a few instances where the whole body was infested have I found it necessary to anoint with the compound sulphur ointment of the London Pharmacopœia. It is necessary to put in a "caveat" against applying the tincture over a too extensive surface. Where the amount of disease is considerable, it is better to take one part of the body each day, going carefully over the whole of it in succession. By this means all risk of iodism is avoided.

This plan of treatment I have found of the greatest value to those whose avocations did not permit the use of extensive inunction.—*Lancet, Dec.* 31, 1864, *p.* 754.

69.—ON THE TREATMENT OF SCABIES.

[In reply to a question of a correspondent of the 'Lancet,' Mr. T. M. KENDALL, of King's Lynn, replies that he has never found the following to fail as an immediate cure of itch.]

Sulphur combined with lime in a liquid form. It is prepared by boiling one part of quick-lime with two parts of sublimed sulphur in ten parts of water until the two former are perfectly united. It must be constantly stirred during the boiling, and when they have combined, the fluid is to be decanted, and kept in a well-stoppered bottle. Wash the body well with warm water, and then rub the liquid into the skin for half an hour. It is only needful then to wash the body well, and use clean clothes.

[Mr. G. STRUTT, of Tutbury, recommends the following ointment.]

Rub down one drachm and a half of iodide of potassium with one drachm and a half of lavender water; then add three drachms of precipitated sulphur and two ounces of lard. Mix well together, and rub well on the parts affected for three consecutive nights. On the fourth night the patient to be well washed with soap and water, and the linen changed. Of course the usual directions must be given for scalding the linen and baking the cloth clothes. When the surface is extensive, I generally prescribe the compound or simple sulphur ointment; but if both these are very strongly objected to, I have recourse to the white precipitate ointment, or a solution of bichloride of mercury in rose water. There are three or four formulæ in Paris's Pharmacologia, p. 595, vol. ii. I heard of a family some short time ago who had cured themselves with gunpowder mixed with lard.—*Lancet, Jan.* 14, 1865, *p.* 54.

70.—ON THE LOCAL EMPLOYMENT OF IODINE IN ITS PURE STATE IN THE TREATMENT OF INFLAMED SCROFULOUS CERVICAL GLANDS, AND OF INFLAMED INGUINAL GLANDS FROM SYPHILIS.

Under the above title, Dr. PRIEUR has addressed to the Academy a memoir, of which M. Ricord reports in favourable terms. The proceeding consists in applying to the enlarged glands laminæ of iodine, enclosed in a layer of wadding, where they are rapidly vaporized under the influence of heat. The iodine ought to be spread as uniformly as possible, over the half to a third, or a quarter, of the thickness of the wadding, which should be covered or fringed with a leaf of gelatine, the circumference of which adheres to the skin, and concentrates the iodine vapours to a determinate point. The apparatus is left in its place for from twenty-four to forty-eight hours, and the result is a phlyctena filled with a thick purulent, or bloody serosity. Dr. Prieur states that he has treated in ten years about one hundred and twenty patients by this plan, and has thus caused the disappearance of more than three hundred swellings. M. Ricord, in reporting on the paper, regrets that in speaking of the inguinal swellings, the author of the memoir has not specified whether he treated inflamed glands symptomatic of infecting chancre, *or simply* buboes sympathetic of the soft chancre, or even virulent buboes. *But the inguinal region, like the neck, is the chosen seat of strumous swellings*, and often a chancre or blenorrhagia is only the occasion of the

development of these enlargements. M. Ricord has himself tried the plan of Dr. Prieur in eight cases of well-marked scrofulous adenitis, and the results he has obtained have been confirmatory of the success of the treatment.—*British and Foreign Medico-Chirurgical Review, Jan.* 1865, *p.* 236.

71.—ON THE EMPLOYMENT OF BENZINE IN TRICHINIASIS.

By Professor Mosler.

In a work on intestinal worms, Professor Mosler has pointed out the poisonous influence exerted upon trichinæ by benzine. This substance was formerly obtained by distilling benzoic acid with lime, but is now procured in the preparation of coal naphtha. There seems to be no doubt that benzine destroys the trichina, but it is also poisonous to the animal infested by the parasite. It was therefore a question with Dr. Mosler, whether in the human subject, doses of benzine might not be given, which, without being poisonous to the patient, would prove a remedy for trichiniasis. After performing several experiments on the lower animals, Dr. Mosler was enabled to study the question in the human subject during an epidemic of trichiniasis, which occurred at Quedlinberg last year. The benzine treatment was adopted by giving benzine 3ij. with liquorice juice and mucilage of gum-arabic, of each ℥j., and peppermint water ℥iv. Of this mixture a tablespoonful was given every one or two hours, and in this form the benzine was well borne. Dr. Mosler was satisfied that none of the patients experienced any bad effects from the remedy. His conclusions on the subject are as follows:—Benzine holds the first place among anthelmintic remedies, and may be administered to the human subject with safety, in larger doses than was formerly thought possible; given in doses, which are well borne by the human organism, it kills the trichinæ in the intestinal canal, and so prevents propagation of the parasites; and therefore benzine is the only rational treatment to be employed in the trichina disease in man.—*British and Foreign Medico-Chirurgical Review, Jan.* 1865, *p.* 236.

AFFECTIONS OF THE EYE.

72.—ON TRICHIASIS.

By Dr. E. Percival Wright, Surgeon in Charge of the Ophthalmic Dispensary at Doctor Steevens' Hospital, Dublin.

[All the operations for the relief of trichiasis may be arranged under the following heads.]

1st. The removal of the offending cilia, and the extirpation of their bulbs.

2. The extirpation of the whole row of cilia.

3. The excision of a fold of the eyelids, so as to remove the cilia from the surface of the eyeball; grooving the tarsal cartilage, &c.

4. The dividing the row of cilia, with their bulbs, from the tarsal cartilage, and removing it higher up on the eyelid.

The first three are well known to British surgeons; and modifications of them have been from time to time proposed. Thus the extirpation of the whole row of cilia was suggested, as long ago as 1722, by Heister; approved of, some years later, by Cortum; was modified by F. Jæger in 1818; and, lastly, the operation was improved, in 1844, by Sir W. Wilde, who was among the first to introduce a knowledge of this operation into Britain. The excision of a fold of the eyelids, for the purpose of removing the cilia from the surface of the eyeball, will be found recommended in the works of Celsus, Actius, Paulus von Aegina; and from the days of these fathers of medicine down to the present time, some one modification or other of this operation has been in vogue; of these, that of Gaillard appears to me the best.

The last method on the list—that of directly displacing the cilia, and im-

planting them elsewhere—was first suggested, in 1844, by Dr. Jæsche, and was, almost immediately afterwards, modified by Arlt, at that time assistant to Fischer at the School in Prague, now successor to F. Jæger in the Allgemeine Krankenhaus, Vienna; and it is this method—which Professor Arlt himself was the first to call my attention to—that I now wish to recommend very strongly to the profession. The details of this operation are very shortly these:—1. The formation of an intermarginal excision. It was chiefly in this particular that Arlt modified Jæsche's operation. Jæsche made his first incision in the conjunctiva; Arlt, on the contrary, adopted Flarer's plan of making the incision between the tarsal cartilage and the cilia bulbs, and thus leaving the tarsal edge to a great extent untouched. Flarer originally proposed this plan for the purpose of extirpating the eyelashes—first making the intermarginal incision, then gently raising up the flap containing all the cilia and their bulbs, and then cutting this off with a sharp scissors; and, though I have seen most excellent results from the plan of Sir W. Wilde, above alluded to, yet, had I to perform the operation of extirpating the eyelashes, I would be inclined to adopt the method of Flarer, as by it the bulbs of the cilia cannot fail to be eradicated. In making this incision, it will be found of great advantage to insert under the lid a horn spatula (Jæger's or Saunders'); this will help to make the lid somewhat firmer, as well as put it on the stretch. An assistant should now slightly raise the lid upwards until the tarsal margin becomes exposed. The skin and the cilia are dissected from the tarsal cartilage (*vide* Fig. 1), by means of a sharp scalpel, the incision commencing at the inner angle, cautiously avoiding the lachrymal puncture. Care should be taken to glide the edge of the knife along the surface of the cartilage, so as to avoid wounding the bulbs of the cilia. 2. The next step is to disengage an elliptical flap from the upper surface of the eyelid. The first incision is to be made about two lines from the tarsal edge of the lid; the second incision is made two or three lines from the middle of the first incision, and slopes down at either side to join it (*vide* Fig. 2); this flap is then neatly dissected up, and cut off. 3. The flap containing the cilia, attached at either end to the body of the eyelid, is then freed by passing the scalpel in through the space between the tarsal cartilage and the flap with the cilia, until it makes its appearance in the upper wound; it should be then carried along from the puncture to the outer margin, disengaging in its way any attachments, but not cutting out at either end. 4. The scalpel being withdrawn, the flap is approximated to the upper part of the eyelid by four or five stitches; a compress of charpie, with a bandage, is applied, and in three or four days the stitches may be removed; union will be found to have taken place by first intention, and after the lapse of a few weeks not only will all annoyance from the inverted eyelashes have been removed, but there will not be the slightest trace of deformity.

FIG. 1.

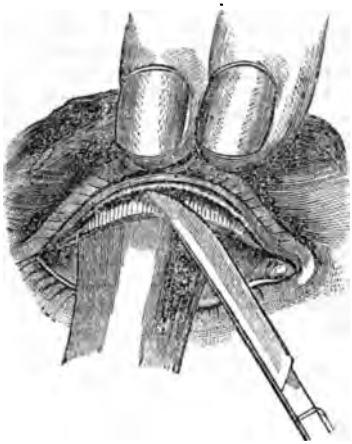


FIG. 2.

In very bad cases of trichiasis the shape of the elliptical flap dissected up, as described in the second stage of the operation, may be somewhat altered, and the second incision be commenced at one end of the first incision in the

eyelid, and then carried upwards as high as five lines above it, and down again to join the other extremity of the same incision—thus making a flap which will have the outline of the moon in its first quarter. In all cases the depth of the excised flap must depend on the amount of the trichiasis.

The operation is most successful when performed on the upper eyelid; but this is the lid most often affected. When trichiasis occurs on the lower lid relief will be obtained by adopting some of the many methods for removing a fold of the skin, such, for example, as Gaillard's.

From the winter of 1860, when I first performed this operation, I have had many opportunities of judging of its success, and I have never yet known it fail. One drawback it certainly has, and that is, the destruction of almost all the Meibomian glands; but then this is true of all the operations for the removal of the cilia, and practically I do not find this objection one of very great importance. Another objection is, that the flap may mortify before having formed a fresh attachment; but this has never happened to myself, and depends, I think, in a great measure on the care with which the operation is performed. The use of cold water fomentations are much more likely to lead to mortification of the flap than when it is dressed with charpie. [Since writing the above I have seen Dr. Waldhauer's paper in the *St. Petersburg Medi. Zeitschrift*, Bde. II., 301, in which he gives the results of 200 cases operated on in this manner; and in the great majority of these cases the results were most satisfactory.] Von Græfe's bandages here, as in all other operations about the eye, are of the greatest possible convenience, and it may not be amiss to mention that the charpie to be used is nothing but finely shreded Irish linen. On one occasion, I recollect that a small slough formed on the centre of the flap, but it did not proceed very far, and on cicatrization the defect was scarcely noticeable. As a rule the eyelids are not subject to sloughing; and if this operation be performed with a sharp scalpel, and the parts cleanly dissected, it is not much to be dreaded.

I believe that this operation of Jæsche's, modified by Arlt, has been successfully performed in this city by Drs. Smyly and Stokes, at the Meath Hospital, and Drs. Hamilton and Symes, at Steevens' Hospital, and probably by many others, but it has been a constant source of surprise to me that so excellent a method should be so little known in this country.—*Dublin Quarterly Journal, Feb.* 1865, *p.* 90.

73.—PHLYCTENULAR KERATITIS AND VASCULAR ULCERS OF THE CORNEA, AND ON THE USE OF THE SETON IN THESE CASES.

By W. Spencer Watson, Esq., Assistant-Surgeon to King's College Hospital, and Clinical Assistant to Mr. Wordsworth, at the Royal London Ophthalmic Hospital.

[Dr. Watson tabulates nine cases of this affection.]

The cases tabulated occurred in the practice of Moorfields, under the care of Mr. Hulke, by whom they were transferred to my care for observation and treatment. The principal point which they appear to prove (so far as such a small number of cases can prove anything) is that in the more severe forms of strumous ophthalmia, with irritable vascular ulcers, when other remedies have failed, the seton forms a most valuable and almost infallible method of treatment. Another point, to which I would direct attention, is the obstinate nature of such cases, as Case No. 6, of a young girl, at about the age of puberty, in which, though the disease has been temporarily arrested again and again, it constantly recurred. The value of the seton in these cases is the more fully demonstrated, as it was not resorted to until other remedies had failed.

I do not bring the subject forward as anything novel, but simply with a view of confirming previous observations by various surgeons before me, and at the same time of directing the attention of the profession generally to a plan of treatment which appears to me to be very valuable in a class of cases which are as a rule most intractable and most distressing to the patient.

It is, perhaps, *worth while* to mention that the seton employed consisted

simply of a thread of ligature-silk; that it was passed double through the skin of the temple, being carried in the eye of a large curved needle; and that in order to avoid any visible scar remaining, it should be passed in the upper and back part of the temporal region, so as to lie under the hair.—*Ophthalmic Hospital Reports*, 1864, *p.* 301.

74.—ON THE TREATMENT OF PROLAPSE OF THE IRIS.

By George Lawson, Esq., Assistant-Surgeon to the Royal Ophthalmic and the Middlesex Hospitals

[When the prolapse of the iris is small, and produced by a clean, sharp, but small cut near the margin of the cornea, the case is almost certain to do well. The sudden exposure of the eye to a strong light is simple, and may be tried, with the view of drawing the prolapsed iris back again within the chamber of the eye, but it will rarely or never succeed.]

Since the introduction into ophthalmic practice of the Calabar bean, we have at our disposal the means of causing a rapid contraction of the pupil, and of maintaining this state as long as it may be deemed desirable. This drug has been used with success in certain cases of prolapse of the iris, seen by the surgeon very shortly after the accident which caused it, and the steady and continued contraction of the pupil has succeeded, in some instances, in drawing it out of the wound; and in others, although the success has been partial, the prolapse has been diminished. As a rule, however, the patient is not seen sufficiently early to allow of this treatment, and other means have to be adopted, not for the purpose of returning the prolapse, as this a few hours after the accident is impossible, but with the view of preventing an increase of it, and of allaying the irritation which is apt to follow the injury.

Removing, with a pair of fine scissors, the Prolapsed Iris.—From the very slight amount of irritation which follows the operation of artificial pupil, when an opening is made at the margin of the cornea and a piece of iris is drawn out and cut off, it might reasonably be inferred that the snipping off of a piece of iris which has bulged through a small wound made accidentally instead of intentionally, would be followed by no more severe results. The cases, however, are not exactly parallel; in the one case, the wound is a simple incised one, made with a clean sharp-cutting instrument, its edges falling into accurate apposition, immediately after the iris has been cut off, and primary union speedily resulting; whilst in the other, the wound is nearly always a punctured one inflicted with a considerable amount of force, and producing a certain amount of contusion; hence, instead of a direct union of the edges, the wound is apt to remain more or less patulous, and after the prolapsed portion of iris has been snipped off, the constant drain of the aqueous will often cause an additional protrusion to follow, and thus render the subsequent displacement of the pupil greater than that caused by the primary injury. There are, however, cases where this line of treatment is judicious, but they require to be selected. Those to which it might be applied are—

1st. Where the wound is small, where it has been made by a fine, sharp instrument, where there is little or no contusion of the corneal tissue, and the prolapse is not large.

2nd. In certain cases where there is a portion of iris lying between the lips of a large wound across the cornea. The removal of it allows the edges of the cut to fall into apposition, and favors primary union.

3rd. It may be often resorted to with advantage at a later period in the treatment of a case of prolapsed iris, when the protruded portion has become coated with a film of lymph, and the edges of the wound through which it bulges are evidently sealed to the iris which fills it.

[In the majority of cases of prolapse of the iris from injury, it is best at *the onset* to close both eyes and place a little cotton-wool or charpie evenly *over the* injured one, so as to form a compress, which is to be kept in its *place by* a turn of a roller. This often affords considerable comfort, and so

long as this is the case it may be worn. The most satisfactory results sometimes follow judiciously pricking the protruded iris, as suggested by Mr. Bowman.]

The portion of iris which protrudes through the wound is usually a fold, bulged to its fullest extent by the aqueous behind, the hollow of the fold communicating directly with the anterior chamber, which it continues to do, until the opposed surfaces of the bag of iris are compressed by the falling together of the edges of the wound.

The object of the operation is twofold. 1st. By pricking the prolapsed iris, to allow the aqueous which fills it out to escape and cause it to collapse, and at the same time to permit the edges of the wound to close upon it, and come into more immediate apposition.

2ndly. To excite sufficient local action to produce adhesion of the edges of the wound to the iris and of the opposed surfaces of the iris to each other. The wound is thus closed, and the portion of iris beyond the cornea, cut off from its vascular supply, withers away.

Treatment of Prolapse of the Iris by Ligature.—In cases where a small prolapse occurs close to, or at the circumferential margin of the cornea through a *small* but *clean cut wound*, the placing a ligature of very fine silk upon the protruded iris is a very excellent mode of treatment.

It strangulates and cuts off the bulge and excites sufficient local action to close the wound, but this treatment is only applicable to a limited class of cases, as, if the wound is large, the diminishing suddenly the size of the protrusion by a ligature soon after the accident, would either favour an additional prolapse, or else, as the aqueous was re-secreted, would allow the prolapsed iris, with the ligature, to be sucked back within the anterior chamber. The latter accident occasionally occurs in iriddesis, if the opening in the margin of the cornea is made too large.—*Ophthalmic Hospital Reports*, 1864, *p.* 227.

75.—ON THE RECENTLY IMPROVED METHODS OF EXTRACTING IN CASES OF CATARACT.

By Dr. Charles Taylor, Honorary Surgeon, Nottingham Eye Dispensary.

[In the following paper, Dr. Taylor avails himself largely of the facts detailed in a pamphlet published by Albert Mooren, M.D., *On the Diminished Risks of Corneal Suppuration after Extraction of Cataract.* No translation of this has yet been published.]

Mooren was led to conclude that the occurrence of slight iritis was the starting point of all the inflammatory phenomena which led to acute suppuration of the eyeball and consequent loss of sight, and that the inflammation always commenced in that part of the iris compressed during the exit of the lens. He says:

"One is led by these facts to consider whether it be not possible either to prevent the occurrence of iritis in extraction, or at least to deprive this inflammation of its dangerous characters. Through an exact study of my operation cases, I believed myself to have arrived at a solution of this problem. Some years ago, in two cases in which the iris was much pulled in the passage of the lens, I had cut off the injured portion as in iridectomy; and, remarkable to say, no reactionary symptoms followed. In this procedure I saw at that time only an evil, and practised it through extreme necessity; not with any idea of preventing inflammation, but only to guard against prolapse. For a long time I remained in the same view; and until I obtained additional similar experience, I gave the matter no more attention.....

In March, 1859, I had occasion, through the unfortunate result of two extraction-cases, to observe what an extraordinarily important part is played by the iris in the healing of the corneal flap. There came to me a big, fat, corpulent, full-blooded peasant's wife, entering on her sixtieth year, to undergo extraction in both eyes; the removal of the lenses was accomplished without any difficulty. After completing the operation, the idea struck me that this patient, on account of the great plethora of her head, might probably suffer

from iritis, and the more as her enormous corpulence forbade her to remain long at rest, and compelled her to spend part of her time sitting upright in bed, even at the very point of applying the bandage. I introduced an iris-hook under the cornea, in order to withdraw and cut off a portion of the iris; on one side the manœuvre was completed; on the other, the pupillary margin slipped from me before I could seize the moment to cut it. In consequence of the pulling which the iris had sustained, this part inflamed with such severity, that on the second day the cornea was already totally infiltrated with pus. The other eye healed without any disturbance, and the patient to this day enjoys with it unimpeachable vision.

"Three days later, the same unpleasant accident occurred to me with another patient. Here, also, the iris slipped from the hook, and the rapidly developed iritis produced likewise destruction of the cornea. I had thus gained the knowledge that the seizure of the iris with a hook was a dangerous manœuvre, from which evil consequences might be expected; just as perfectly as the instrument satisfied every desire when the iris is immediately secured and cut, so extraordinarily dangerous is it, when the membrane slips off again, and is thus exposed to repeated traction. The application of the common iris-forceps, such as the one commonly used in iridectomy, would from time to time entail this consequence, that, in great unsteadiness of the patient, or by strong turning of the eye in an opposite direction, complete dialysis would be produced. In order to render deviation of the eye, and consequent slipping of the iris impossible, it would be necessary to hold the globe with a hooked forceps. But let us think of the extreme danger involved in such a proceeding. Nearly half of the cornea has been separated from its periphery by the section; the lens is removed; the vitreous body is only held in place by the thin posterior wall of the capsule. How light soever might be the pressure, and on whichever side it came, when the traction of the forceps was felt, bursting of the empty capsule and escape of the vitreous humour would be the first, and most natural result; at all events, to complicate the operation in such a manner for the sake of avoiding inflammation, would double the risks of extraction rather than diminish them. The procedure would be dangerous, and consequently is inadmissible. Another solution of the problem was therefore necessary.

"The first opportunity offered itself in May, 1859. A woman, past seventy years of age, with faded, wrinkled skin, and of such weakness that she could only walk when supported by a stick, compelled on account of great exhaustion to spend most of her time in bed, presented herself to me for extraction. Under these circumstances, it was evident that the least iritis would be followed by sloughing of the cornea; plainly speaking, the state of things seemed to me so desperate, that I did not undertake to operate without some feeling of repugnance. I determined, therefore, to divide the operation into two parts. Acquainted with the small risk attending iridectomy, I first made this from below. The healing process was completed without disturbance, and the patient was ordered the most strengthening diet. Three weeks later I performed extraction by an inferior flap. I thus had the advantage, first, that in premature escape of the aqueous humour the iris would not be pinched between the lens and the knife; secondly, that the cataract itself had not to be pressed through a narrow pupillary opening. In this way were two conditions obviated, which in the ordinary method of operating greatly promote the occurrence of iritis; as a fact, the fortunate result entirely surpassed my expectations. The healing process went on without any signs of reaction; and, fourteen days later, the eye no longer showed a trace of vascular injection.

"In the month of June, a second eye was offered to me for extraction, in which the chances of success by the use of the usual operation would have been very small. Encouraged by the favourable result I shortly before obtained, I determined to practise again the same method. This case occurred in a lady of 76, my own aunt. The patient had been blind for several years, and showed that delicate skin upon the face and hands at which, with reason, *all operators* become alarmed. This sign may be observed in more than *three-fourths* of the instances of threatened sloughing of the cornea. First,

iridectomy was performed; fourteen days later, extraction. The cornea proved to be extremely thin, and wrinkled itself upon the completion of the section, the centre and the cut edges sinking in; on the third day, the eye was opened. An insidious iritis had developed itself without pain, and had produced grey exudation masses in the pupillary space. The whole treatment consisted in the administration of decoction of cinchona. Three weeks later, all inflammation had subsided; but under its influence the pupil had become in great part closed, and the patient had only a very small amount of vision.

"A long illness that shortly afterwards befell me, did not allow me until autumn to think of opening the pupil by operative means. The patient underwent the renewed iridectomy without any inflammatory reaction. Four weeks later, with 2½ convex, she read No. 3 of Jäger's test-types; and this lady still enjoys such excellent vision, that at present she occupies nearly all her time in reading and writing. This case proved to me, more than the former had done, that the supervening iritis which, under other circumstances, would have totally destroyed the eye, had been deprived of its dangerous characters.

"Likewise, it seemed established that, by this method, the occurrence of disturbing processes might be prevented. It remained, therefore, to determine the limits within which it should be preferred. Speaking for myself, I could not but arrive at the feeling, that extraction, as it had heretofore been taught and practised, was no longer to be desired in opposition to my method of diminishing its dangers. *I saw my method, not only as a supplement, but as an absolute necessity in extraction.*"

I have preferred, in the above extract, to give Dr. Mooren's reasons for the adoption of a preliminary iridectomy as nearly as possible in his own words. He does not recommend his operation, however, to the entire exclusion of the ordinary method; but only in cases where, from the state of the patient's health, or the condition of the eyeball itself, the ordinary operation is almost certain to fail. Thus, he always makes a prior iridectomy, in cases of great age, complicated with general debility; in cases of disease likely to interfere with, or prevent the healing prôcess, such as severe cough, asthma, persistent vomiting, or affections which preclude long maintenance of the recumbent posture; a thin, silky, debilitated state of skin; or such an amount of constitutional cachexia as would lead the surgeon to anticipate disaster as the result of any operation.

The existence of pupillary adhesions, a diseased or deteriorated condition of the lens or eyeball itself, are also indications for his method of operating. Thus, if the pupil be thoroughly dilated with a drop of atropine solution, the lens will readily pass through the pupillary aperture, and there is so much the less indication for a preliminary iridectomy. Again, if the cataract have a small nucleus, surrounded by much cortical matter in coherent masses, the escape of some portions of this soft matter into the anterior chamber is so apt to provoke an insidious iritis, with subsequent suppuration of the globe, that, in this condition of the lens, a careful prior iridectomy is the only safe method of operating. The worse the patient's condition, the longer must the interval between the iridectomy and subsequent extraction be. In Mooren's cases, a fortnight or three weeks only was allowed to elapse; but six weeks is the shortest interval now adopted. I may mention that Mooren's practice, with the exception of Von Gräfe's, is the largest in Prussia, if not in the whole of Germany; he having had 578 operations in 1860, and 622 in 1861.

Since 1859, up to the period of the publication of his pamphlet (1861), Mooren practised his method in every case where the ordinary operation appeared certain to fail, with such brilliant results that he feared the incredulity of his readers; and, instead of contracting his narrative to a simple percentage proportion between successful and unsuccessful cases, thought it necessary to verify his statistics by giving the names and addresses of all the patients operated upon in this manner. Here are one or two average specimens.

"Frau Greis, from Eberfeld. Iridectomy, October 14th. Extraction, October 22nd. Very pasty individual, with thin skin, and very insufficient dilatation of pupil. She is so asthmatic, that she seems every moment on the

verge of suffocation, and is always, therefore, in the highest state of agitation. On completing the section, the cornea wrinkled itself very much; the removal of the lens was attended by very great difficulty, both from the unsteadiness of the patient, and also from firm adhesion between the cataract and posterior capsule. Tedious healing. Granulation, with much spasm of the lids. Recovery, with tolerable vision.

"Frau Berger, from Viersen, only fifty years old; but a picture of weakness, decrepitude, and anæmia. The left eye had undergone reclination elsewhere, and the uprisen lens irritated the eye. I attempted its removal. Upon introducing the knife, however, the aqueous humour and the vitreous humour of like consistence trickled forth, and the sclerotic fell into wrinkles. These conditions, together with circumscribed capsular opacity of the right eye, led to the conclusion that the vitreous humour was perfectly fluid in the right eye also. Iridectomy, Sept. 8th. The cornea wrinkled itself; the effused blood required forteen days for its absorption. The patient was so feeble as to be kept in bed for three weeks. She left her bed for the first time on the day before the extraction, which was performed on October 1st. Full diet. A secondary cataract remained, and, together with an insidious iritis, produced an obstructed pupil. In the beginning of December, therefore, a second iridectomy was required. Very satisfactory vision. In both the operations, the cornea fell completely; and the patient's condition of marasmus was so extreme, that my assistants, and Dr. Ruschhaupt, who witnessed the operations each time, earnestly endeavoured to dissuade me from performing them."

Without quoting further from Dr. Mooren, I will now detail two or three cases in which I have performed his operation; which will, I doubt not, be all the more satisfactory, as I shall be able to show you the patients.

Case 1.—Ann Allwood, of Mansfield, a feeble, cachectic individual, in her fifty-sixth year, consulted me in May last year on account of an amaurotic condition, with partial lenticular opacity of the left eye. On ophthalmoscopic examination, I found the retina extensively diseased; the vitreous body fluid, and crowded with shred-like patches, which floated freely to and fro; and her vision so much impaired that she required a guide in the street. The right eye had been useless for some years; having, in addition to a perfectly fluid vitreous body, indicated by a remarkably tremulous iris, a hard calcareous cataract. As she had some perception of light with this eye, I thought it possible that she might be benefited by extraction; and, accordingly, proposed the operation. I also considered this an excellent case to test Mooren's method, as the patient's health was very bad, the eyeball extensively diseased, and an ophthalmic surgeon had assured her that any attempt at extraction would be followed by suppuration of the globe. Iridectomy was performed early in June; extraction three weeks later. The patient recovered without a bad symptom. Ten days after the operation, there was scarcely a trace of vascular injection; and it was only on careful inspection that the fact that she had recently undergone extraction could be ascertained. After the operation, she had increased perception of light, but no useful vision. The ophthalmoscope revealed extensive disease of the retina and vitreous body.

Case 2.—Alice Duffield, Nottingham, a decrepid, pasty individual, in her seventy-seventh year, with thin wrinkled skin, and so feeble that she had required the support of a stick for some time past, had been in very bad health, and had suffered three slight paralytic attacks. Her medical attendant assured her that she was not fit to undergo the operation. I thought this a good case for Mooren's method; and accordingly performed a superior iridectomy on the left eye early in August last year, extracting a month later. The patient was very unruly; and the cornea was wrinkled and depressed in the centre. The lens was extracted without difficulty. She was perfectly comfortable, and without a bad symptom for five days; when, contrary to orders, she got up, removed the pad, and exposed her eye by looking out of an open window. Acute catarrhal ophthalmia, culminating in a very sharp attack of iritis with closed pupil, was the result. Three months later, therefore, when the eye was quiet, I performed a second iridectomy, opening up

the pupil, and restoring good vision. Eighteen months previously, when she was in much better condition, I performed ordinary extraction on the right eye. There was an escape of fluid vitreous humour, and considerable prolapse of the iris, followed by general iritis, turbidity of the humours, and the establishment of a corneal fistula through which the aqueous humour drained slowly for months. The eye was, however, ultimately saved, after she had undergone considerable pain and discomfort.

Case 3.—Joseph Falconbridge, a very feeble patient, wasted by poverty and disease, in his sixty-eighth year, was suffering from chronic psoriasis, with patches of ulceration on the skin, and had recently been an inmate, for three months, of a general hospital on account of chronic disease of the bladder, which necessitated his rising several times each night. He was also afflicted with ingrowing lashes, which had caused a patch of ulceration on the lower half of the cornea of the left eye, which was the site of a hard senile cataract. This was altogether one of the most unfavourable cases for operation that could well be conceived. Iridectomy was performed on July 2nd, 1863. Owing to a very prominent brow and great emaciation, the eyeball (very small and hypermetropic) was so sunken, that I had great difficulty in introducing the iridectomy-hook. Extraction was performed three weeks later. On opening the eye on the seventh day, the corneal surface was covered with pus, the result of severe conjunctivitis, caused apparently by the irritation of the lashes. The wound was quite healed: the cornea perfectly clear. The conjunctivitis yielded in a few days; and the patient recovered with excellent vision.

Case 4.—Mary Rame, a feeble patient, with thin, wrinkled skin, in her sixty-fifth year, was much reduced by poverty, confinement, and depression of spirits, the result of blindness caused by hard senile cataracts in both eyes. Iridectomy was performed in the left eye early in August last year; extraction six weeks later. The lens was very large, and surrounded by soft coherent matter, some of which floated into the anterior-chamber, and which, owing to her unsteadiness, it was impossible to extract. She did exceedingly well for five days; when, owing to the irritation of the lens-matter, or to some unexplained cause (I did not see her after the operation), severe iritis was developed, ending in closed pupil. I have not yet had an opportunity of remedying this defect; but do not doubt that a second iridectomy will restore her excellent sight.

Case 5.—Ann Fox, aged sixty-five, with hard senile cataracts in both eyes, was in reduced health, owing to depression and confinement, the consequence of blindness; and very obese, so that she could not maintain the recumbent posture for any length of time. She would probably have done well with the ordinary operation; but, as she was excessively anxious as to the result, I gave her the choice, and she selected the safer procedure by Mooren's method. Iridectomy was performed on the right eye early in August last year; extraction six weeks later. She recovered without the least unfavourable symptom; has excellent distant vision, and reads diamond type.

I found in these cases that the coloboma iridis was completely concealed by the upper lid; that the pupil was about the normal size; that it was influenced by a strong light, dilated by atropine, and contracted by Calabar bean. The patients did not complain of dazzling; and the only inconvenience attending the operation was its division into two parts, separated by a somewhat long interval.

Mooren publishes sixty selected unfavourable cases, of which the two I have quoted are fair specimens. These cases, he states, would have been lost with certainty by any other proceeding—an opinion confirmed by Dr. Ruschaupt and his two assistants, Drs. Meisner and Josten. The operations were attended by the remarkable result of only two failures. In one of these, when not under the doctor's care, the eyelashes became inverted, and raised the corneal flap; in the other, there was at the time of the operation, complete retraction of the corneal flap—in itself, tantamount to a local mortification. Prior to the adoption of his method, eleven failures in ninety-seven operations, or more than one in ten, was the average of failures with ordinary cases presumably *favourable* in the hands of the same operator.

The cases most likely to fail by the ordinary, or, indeed, any method, are those where the cornea is extremely thin—a condition mostly observed in cachectic and aged individuals; a condition, moreover, which it is impossible to diagnose with certainty prior to the completion of the section. We may always, however, suspect its existence when the skin is extremely fine and thin. There are three degrees of this condition; the first, marked by a central depression of the cornea, often with transverse wrinkling immediately on completing the section; a second, where the shrinking is such as to cause withdrawal of the flap; and a third, where there is more or less positive retraction. This complete retraction implies absolute loss of the elastic properties of the cornea; and, whenever it exists, leads to total destruction as soon as the ordinary reaction attains the height of an inflammatory process.

My experience of Mooren's method is, that there is much less danger of subsequent inflammation; and that, when this does occur, it is robbed of its dangerous properties. The preliminary iridectomy affords a permanently large pupil, through which the capsule of the lens is very readily incised, and through which the lens itself passes with the greatest ease. After the operation, the edges of the corneal flap are most easily replaced, and there is no chance of that *bête noir* of extraction, prolapse, to disturb the healing process. If the inferior section (the one always used by Mooren) be adopted, the remains of the lens are removed more easily; any portions left, too, are less liable to press upon and irritate the iris, when this membrane offers to them a diminished bearing surface.

There are three objections to Mooren's method. The first is, the deformity occasioned by the removal of a portion of the iris; the second, the liability of the patients to unpleasant dazzling through the enlarged pupil; and the third, the inconvenience attendant upon the division of the operation. The first two cease to be objections when the upper section (the one which I have always adopted) is used. The last is indeed a most serious contra-indication, and one that must necessarily limit the operation. This is so palpable a fault in Mooren's operation, that in the two other improved methods of extracting to which I have briefly to direct attention (Jacobson's and Schufte's), the iridectomy and extraction are combined. Jacobson makes the iridectomy the last step of extraction, and gets rid of the obvious dangers of manipulating the globe after incision of the cornea, pointed out by Mooren, by doing the whole operation when the patient is profoundly under chloroform. He, like Mooren, by continually inspecting the healing process after ordinary extraction, established the fact that the portion of iris corresponding to the flap, and compressed by the lens in its exit, is that in which inflammation always commences. This is the centre of all risk to the eye; and, to be danger-free, this must be got rid of. He keeps his patient for twenty-four hours without a large meal, and for some hours without solid food. He pours chloroform on a compress with a wax cloth back; holds it at a little distance, until the fauces and larynx have become accustomed to the vapour; and then moulds its edges (after a fresh supply of chloroform) to the face, so as to admit very little air, and to push anæsthesia rapidly. When the patient is nearly fit for operation, the compress is somewhat removed, and more air given with the last portion of chloroform. The patient being in such a condition that the contact of instruments produces no action whatever of the muscles of the globe or lids, the latter are held apart by an assistant, and Jacobson proceeds to make an inferior flap, with the line of incision not in the transparent tissue, but in the vascular limbus corneæ, so as to get a larger opening for the lens, and to place the wound nearer than usual to the materials for repair. For a large or externally softened lens, he makes his puncture and counter-puncture half a line below the horizontal meridian; for a smaller or hard lens, somewhat lower. This being premised, and also that he selects a knife of a breadth corresponding to the semi-diameter of the cornea, he proceeds, sitting on the edge of the bed in a convenient position, to seize the conjunctiva and subconjunctival tissue with toothed forceps, below and to the inner side of the cornea; with the disengaged hand he *enters the* knife at the junction of the cornea and sclerotic, carries it quickly *through* and out at a corresponding point, and pushes it gently on with its

edge inclined slightly towards the iris, so that nearly all the external wound will be in the conjunctiva. The knife, being carried on as far as convenient, is gently withdrawn, leaving a central isthmus undivided. If the blade be too narrow, this isthmus will contain a bit of cornea; but if it be of right width, it will consist of conjunctiva alone. Von Gräfe's fleam-shaped cystotome is then introduced, and the capsule freely opened; and then the isthmus is divided; its corneal portion, if any, with the point of the knife, in the line of the original incision; and its conjunctival portion with scissors, two lines farther down on the globe. The lens is started by digital pressure through the lids, directed in the first instance from above backwards and downwards, so as to make it rotate and present its lower edge at the pupil; and any *débris* are eased out with the scoop in the most delicate conceivable way. The lower segment of iris is then drawn out with proper forceps, and cut off close to the ciliary margin. The conjunctival space and the line of incision are cleared of blood and coagula; and the lids are closed. If the cornea be in right position, Arlt's compress is immediately applied; but if it be depressed, Jacobson waits from five to thirty minutes for a resecretion of aqueous humour to elevate it. The operation being a bloody one, there is a little delay between its stages, on account of conjunctival bleeding obscuring the parts and requiring removal. Between each two stages, the anæsthesia must be noted, and, if necessary, more chloroform given; the lids being, in the meantime, closed and supported by a ball of charpie and gentle pressure. No step must be taken without certainty that it can be completed with passive ocular muscles. If vomiting occur, the lids must be closed, supported by a ball of charpie and the hand; and the patient raised to a sitting posture, to vomit; then laid down again, and more chloroform given. The anæsthesia is to be maintained until after the application of compresses, which are applied in every case to both eyes.

For twenty-four hours after the operation, each patient has the undivided attention of a nurse, who anticipates all his movements, supports his eye if he should vomit, and so forth. If any pain, or especially any *heat*, be felt in the eye, iced poultices are applied over the compresses, and changed every few minutes for hours together. If this be insufficient to give speedy relief, and if the patient will bear depletion, four leeches are applied in front of the ear, and each replaced by a fresh one as it falls, so as to take blood continuously until benefit is derived. From forty to eighty leeches have been used in this way to a single case; and this treatment is had recourse to promptly, before mischief is allowed to make head. Jacobson does not hesitate to open the eye for inspection on the second day, and, after the third, commences the systematic instillation of atropine, to prevent adhesions between the pupillary margin and the coagula left for absorption after iridectomy. Jacobson claims ninety-eight cures in one hundred operations, presumably ordinary cases. I have not tried his method, as I conceive that chloroform vapour concentrated to the required extent, must of itself entail some risk, especially in patients whose health is such as to render the ordinary operation inadmissible. Again, I think escape of the vitreous humour would be favoured by the position of the incision; while the excessive attention to minutiæ, the risk of vomiting, and the extraordinarily careful nursing required, are all such self-evident contraindications, that I need not dwell upon them. None of these, however, apply to Schufte's or more properly Waldau's method of scooping out grey cataracts—an operation which, first received with caution, afterwards, owing to several failures, viewed with disfavour, is now rising so rapidly in estimation as to threaten, in the words of Mr. Critchett, to supersede the old method of flap-extraction altogether. In this operation, the lids are kept apart by the ordinary wire speculum; the operator then fixes the eye with a pair of forceps; and, with one of Jäger's bent iridectomy knives, proceeds to incise the cornea close to the sclerotic, so as to form a curved slit from three to four lines in extent. With a hook or forceps, a portion of the iris is then drawn out, and about one-fifth of its extent removed. It is well, in doing this, to leave a slight belt at the ciliary margin, so as to afford support to the vitreous, and prevent its escaping or prolapsing. The capsule of the lens is then freely opened, and any superficial softened matter coaxed out with the

scoop of the ordinary curette. Critchett's vectis-spoon is then glided between the posterior surface of the cataract and the hyaloid membrane. As it passes behind the equator of the advancing lens, the handle is slightly sloped backwards; and, when its centre has reached the posterior pole of the lens, with a gentle lever movement, it is cautiously lifted into the anterior chamber, and extracted. The greater part of the cataract is removed at once. Any remaining portions, however, must be carefully fished out with the spoon; while the globe is lightly rubbed with the lids from time to time, so as to wash any fragments concealed behind the iris into the pupillary area. Critchett's spoon brings away the cataract, or, at any rate, its firm nuclear portion, unbroken, and does not take up more room than a thin metallic layer. The introduction of this instrument has greatly facilitated the performance of Schufte's operation, and will render it much more general. It also enables us to deal with all varieties of cataract, however dense. The risks attending this method are, that the hyaloid may be ruptured, and the vitreous allowed to escape. There is also danger of bruising the iris between the cornea and spoon; and also that fragments of the lens may be left in the anterior chamber, probably concealed behind the iris. Any of these accidents may retard the progress of the case, or even seriously compromise the eye. If they are avoided, however, and there is much less risk of such accidents since the introduction of Mr. Critchett's spoon, there can be no question that the patient's chances with a comparatively small elastic slit in the cornea are far greater than with a large semicircular flap. Scarcely any after-treatment is necessary, and the eye need not even be closed. There are many objections to the old scoop with flat bottom and steep edges, originally used by Schufte. It was apt to become entangled in the substance of the lens before it had passed behind its posterior surface; to break it up, and push portions into the space behind the iris, where they excited destructive inflammation. Hard senile cataracts also could not thus be removed.

These facts will perhaps account for the ill success which has attended this operation in the hands of many surgeons. Mooren adopted it in thirty-two cases, with the decidedly unfavourable result of losing ten eyes. He also states that other surgeons were similarly unfortunate. A friend of mine performed the operation in two cases—in both the globe suppurated; and an eminent London ophthalmic surgeon informed me that he had tried it in three cases, the eye being lost in each. The only case in which I have performed the operation was one of traumatic cataract, in which, owing to the development of glaucomatous tension, it became necessary to perform iridectomy; and I subsequently extracted the lens with Mr. Critchett's spoon. In spite of the above facts, Schufte's is just now the operation *par excellence* at Moorfields; and I do not doubt that it will ere long occupy a similar position elsewhere.

I must not omit to mention that linear extraction, with and without iridectomy, is fast superseding the old operation of solution by needles, drilling, and discission, in a number of cases of fluid and semifluid cataracts. There is no risk of escape of the vitreous humour, collapse of the globe, or hemorrhagic effusion; and the results are, as Mr. Hart observes, in some respects incomparable. Of the various modifications of this operation, none yield such speedy and charming results, as Mr. Teale's revival of the ancient Persian method of extraction by suction. The operator must first, with two needles, tear up the anterior capsule, so that it may curl back from the area of the pupil, and be lodged behind the iris. Then, opposite the margin of the fully dilated pupil, make an incision in the cornea with a broad needle; through this opening, the suction-curette is introduced, and is gently buried in the opaque lens, which is then extracted by suction. Mr. Krone makes a very useful valve instrument for this purpose; and Mr. Weiss has two—one with a valve, and the other with a mouth-piece. I prefer the latter. Great care is required, to avoid injuring the posterior capsule. This instrument may be also used in cases where extraction by flap is contraindicated, such as, *e. g.*, diabetic cataract. The patient sees after the second or third day; and moderated light may be admitted in forty-eight hours after the operation.

In conclusion, I may perhaps be excused to mention that I have en-

deavoured to simplify the ordinary operation of extraction by semicircular flap, by carefully dividing the capsule with a fine cutting needle, as the first step of the operation, when the pupil was fully dilated with atropine; extracting as soon as the few drops of aqueous evacuated had been resecreted. I have thus avoided what always appears to me a difficult and dangerous part of the operation—incising the capsule when one-half of the cornea is converted into a flap; the patient's eye rolling in all directions; the iris in contact with the cornea; and the pupil, *in spite of atropine*, reduced to a point. The risks of pressure in this condition almost preclude the possibility of fixing the eye; and the difficulty of incising the capsule freely and carefully, the dangers of scratching the iris, obscuring the operation with blood, oppressing the eye, breaking up the lens, and thus exciting the first steps of what may prove a destructive inflammatory process, are self-evident.

Since the above was written, I have had further opportunities of testing both Schufte's and Mooren's operations, with excellent results. I may also mention that, at the recent Heidelberg Ophthalmological Congress, Professor Jacobson stated that in 140 operations he had only lost three eyes. In one remarkable case, a patient aged eighty-five was kept narcotised for three hours, and inhaled upwards of six ounces of chloroform. In an animated discussion which ensued, concerning various points connected with extraction, the general feeling appeared to be in favour of iridectomy only in special cases such as I have pointed out above. Von Gräfe also expressed an opinion contrary to the generally received notion, that of two cataracts the less ripe one afforded the best results from operation. Mr. Critchett, at this meeting, also stated that he had met with such success with Schufte's method and his own modified spoon, that he now never thought of performing the usual flap operation.—*British Medical Journal, Nov.* 12, 1864, *p.* 539.

76.—EXTRACTION OF CATARACT AND FORMATION OF ARTIFICIAL PUPIL IN A PATIENT WITH THE GLOBES OTHERWISE DISEASED.

By James Vose Solomon, Esq., Surgeon to the Birmingham and Midland Eye Hospital.

[The patient was a comb-maker, forty-five years of age. He had anchylosis of the cervical vertebræ. His *left* eye had been injured eight years ago; it was blind, disorganized, and staphylomatous from past choroido-scleritis.]

The right eye had been diseased a year only. It presented very advanced tissual changes from sympathetic irido-choroiditis; the cornea was bright, and surrounded by a large ciliary venous zone; the iris, which was bossulated throughout, nearly touched the arch of the cornea. The whole circumference of that membrane presented numerous and complex anterior synechiæ. The pupil was closed and adherent to the lens capsule. The vision was limited to a perception of shadows, which was more vivid than I have ever before met with. The patient, who had been addicted in his younger days to pugilism (which probably gave rise to the cervical anchylosis), was in excellent health.

Operations, August 15th. The disorganized eye was enucleated; and a narrow bit of iris was taken by the canular forceps from the right, in order to ascertain the condition of the lens, which it was believed might perchance be transparent, as his perception of shadows was unusually good. The lens proved to be opaque.

Operation, August 27th. The eye having recovered, the lens was extracted to-day by the lower section, the knife being carried through the sclera, and behind the iris. The cataract was easily removed, and without loss of vitreous, by a hook. It was a dark amber lens. A beautiful black pupil, with vitreous distending it, left nothing to be desired by the operator. The eye was therefore quickly closed, and dressed with jeweller's cotton wool, supported by a few turns of a roller.*

* In extraction by flap, and in cases of iridectomy, I have found this kind of dressing to supersede all others. The eye can be gently compressed without discomfort to the patient. A roller of flannel canvas ("domet") forms an admirable compressive bandage. The value of well-applied compression has been specially pointed out by Dr. Von Gräfe.—*Vide Ophth. Rec.*, vol. i., pp. 3-4-5.

Sept. 7th. The wound is cicatrised, the pupil is well defined and open; but behind it are shreds of capsule, some soft lens matter, and lymph. The vision remains as before treatment. Tension less than normal—(T1).

Sept. 21st. The anterior chamber, from recession and flattening of the iris, is enlarged; the inference is that a communication subsists between the two aqueous chambers, although the fundus cannot be illuminated.

Operation. A broad cutting needle was inserted through the upper circle of the ciliary sclera, and brought out in front of the iris, when with a sharp backward stroke the membrane was severed, and a good pupil, distended by vitreous, was obtained.

Sept. 27th. There is a still further improvement in the dimensions of the anterior chamber and pupil, but vision is not commensurately advantaged.

October 7th; operation. The pupil is drawn downward and closed, the upper border being well defined. The iris has lost its fibrillous character and become lighter in colour. It was therefore determined to make an artificial pupil, with a penetrating crochet iris-hook (Bowman's). A small puncture having been made slowly through the upper corneal circle without loss of aqueous, the hook was introduced, and an equilateral, triangular pupil was made, the apex of which became united to the point of puncture. The base measured about two lines.

November 22nd. The fundus can now be, for the first time, focused. The limits of the optic nerve are ill defined; there is gray atrophy. The tension of the globe is normal, chronic enlargement of the ciliary veins still exists, but there is no evidence of irritation from the surgical treatment. With a three-inch convex glass, features are distinctly seen at nine feet; at the same distance he counts fingers, but they cease to be visible when separated to a greater extent than nine inches, so much contracted is the visual field. The poor man's inability to rotate the head, in consequence of the anchylosed condition of the spine, and his amblyopic and contracted field of vision, render fixation very slow.

I am induced to report this case, from the very unfavourable results which have generally attended, in English subjects, the extraction of cataract, or the formation of an artificial pupil, where the same tissual changes have been present as were observed in the instance here given.

Having satisfied myself, by examination, that the vitreous was, in all probability, of good consistence, it occurred to me that liability to ophthalmitis, or shrivelling of the corneal flap, might be lessened, provided, in the performance of extraction, I abstained from interference with the disorganized iris, and made my section through the sclera. In considering the position of such section, it must be remembered that the iris was bossulated and united near to its periphery by numerous morbid adhesions to the cornea—conditions which permitted a wound to be made much nearer to the sclero-corneal union than would have been practicable in the healthy eye. The passage of the knife between the front of the lens and the posterior surface of the iris, by separating intimate adhesions and opening the capsule, must have materially assisted the extraction of the cataract, and so promoted early union of the flap. Bowman's hook being of small size, and furnished with a sharp-cutting inner edge, affords a most useful instrument in the case under consideration. It may be thrust through any selected point of the iris; its cutting inner edge is well adapted for the division of the pigmentous membrane which is frequently found behind the inner ring of the diaphragm, forming a bond of union with the lens capsule.

The advantages of making the section through the posterior aqueous chamber (a method practised by Whyte, in 1801, for the extraction of cataract, uncomplicated by other intra-ocular disease) appear in cases of annular synechia to be as follows:—(1) An easy release of the lens from its adhesions; (2) the formation of the flap, edged with a membrane (the sclera), more favourably placed for nutrition than the cornea; (3) a diminished liability to destructive irido-cyclitis, by non-interference with the diseased iris until the eye has recovered from the cataract operation.

*The theo*retical objection to scleral section consists in the greater probability of loss of vitreous. As, however, it is scarcely possible to obtain less

success than has hitherto attended the plan of performing iridectomy and extraction on the same day, I feel justified in suggesting to the profession a trial of the method here described, with a view of determining its value, as compared with other plans of treatment.—*Ophthalmic Review, April*, 1865, *p.* 28.

77.—REPORT ON CASES OF AMAUROSIS FROM INTRA-CRANIAL CAUSES, IN WHICH ONE EYE ONLY WAS AFFECTED.

By JONATHAN HUTCHINSON, Esq., one of the Editors of the Royal London Ophthalmic Hospital Reports.

The ophthalmoscope has enabled us to separate into different groups all the various cases of defect or entire loss of sight resulting from changes in the deep tissues of the eye. It has, however, left us still a considerable group of cases, which we are obliged, for want of a better term, to put under the old name of Amaurosis, in which the source of damage exists not in the eye, but in some part of the visual nerve—apparatus within the skull. Of these cases, by far the majority exhibit, by the ophthalmoscope, only anæmia and subsequent wasting of the tissues of the optic nerve itself. In some we observe swelling of the optic nerve (neuritis), and a proof of some pressure upon it in the fact that the veins are dilated and tortuous; but the common condition is that known as white atrophy. In some we suspect the existence of a tumour in the brain or cerebellum, affecting the optic centres or the optic nerve tracts either by reflex influence or by direct pressure. In others, some occult change is probably taking place at the nuclei of origin of the nerves (so called tobacco amaurosis). In others, the sexual organs appear to exercise some reflex influence upon the function of sight. In others, again, we have the history of blows on the head, or of cerebral concussion. In a few, constitutional syphilis, either acquired or inherited (very rarely the former), induces in some unexplained way entire loss of sight, without any morbid process in the eye itself. In a few, and chiefly in the aged, there is reason to suspect ordinary cerebral apoplexy, or it may be softening of the brain. Lastly, in a very few, the suddenness of the attack, and the fact of the coexistence of valvular disease of the heart, leads us to infer that embolism of the retinal artery has occurred.

All these possible causes of intra-cranial amaurosis, excepting the last (embolism), are likely to affect, more or less, both eyes at the same time. For anatomical reasons it is difficult to understand how any disease, whether acting by reflex influence or by direct disorganization, could, if situate behind the optic commissure, affect one eye only. One eye may suffer chiefly, or one eye may become blind before the other, but that one eye should become utterly blind, and its nerve disc white and atrophic, whilst the other enjoys its perfect function, and presents, on ophthalmoscopic examination, no morbid condition whatever, is inexplicable, excepting on the supposition that the nerve trunk anterior to the commissure, or the artery which supplies it, is the part involved. It is just possible, however, that we may be proceeding a little too fast in our supposed possession of anatomical knowledge, and that there do exist cerebral nuclei for the two eyes, disease of one of which could involve one organ only. To effect this it would of course be needful for the fibres which crossed at the commissure, to cross back again in the mesocephalon.

In practice, cases of complete, monocular amaurosis, of the kind alluded to, are very infrequent. Through a period of about seven years, during which my attention has been alive to the peculiar interest of the subject, I have been able to collect very few. These I now propose to group together, in the hope that, by their investigation in juxtaposition, some light may perhaps be thrown on a very intricate subject.

It is almost needless to remark that cases recorded without ophthalmoscopic examination are of no value whatever in this inquiry. We meet with cases of monocular blindness almost every day, but with only very rare exceptions indeed we are able in such, by the ophthalmoscope to find adequate explanation in *the state of* the deep structures of the eye itself. A few of

these cases, however, those for instance in which large retinal apoplexies occur only in one eye, and in perfectly healthy patients, or in which large extravasations take place into the vitreous body without assignable cause, are not quite without their interest in relation to our present subject. They at least prove the possibility of extensive unsymmetrical hemorrhages in patients who seem healthy at the time, and in whom nothing further follows. Similar extravasations into the optic nerve sheath might produce the exact condition to which we refer, and it is probable that in some cases such is the fact.—*Ophthalmic Hospital Reports*, 1864, *p.* 235.

78.—ON THE RADICAL CURE OF EXTREME DIVERGENT STRABISMUS.

By James Voss Solomon, Esq., Surgeon to the Birmingham and Midland Eye Hospital, formerly Surgeon to the Birmingham General Dispensary.

[A case is one of Extreme Divergent Strabismus when the patient is unable —let his muscular efforts be ever so great—to do more than bring his eye to the centre. In the following paper the author confines himself to the consideration of instances where the deformity has had its origin either in a too free division of the tendon of the inner rectus muscle and the subconjunctival fascia, by Dieffenbach's operation for squint; or in paralysis of the inner rectus, unconnected with tumour of the orbit.]

The indications to be fulfilled by any proceeding undertaken for the radical cure of *Extreme* Divergent Strabismus, when a result of previous tenotomy of the inner rectus, are :—To give the eye a central position ; to restore the length and the proper point of attachment to the abductor muscle, thereby insuring balance of the opposed muscular forces and *permitting normal convergence.*

The division of the attachments of the inner rectus, with its conjunctiva and subjacent fascia (in a manner to be presently described), and of the tendon of the external rectus, suffices to place the eyeball in the axis of the orbit.

The great difficulty consists in protecting and fixing the globe until the inner rectus obtains a proper attachment to it. Careful study of the subject led me to institute in persons who are in good health the following plan.

Anæsthesia having been induced by chloroform, and the lids separated widely by a self-acting speculum, the first step of the operation is commenced by making a flap of the parts which contain the inner rectus. To effect this a rather long perpendicular incision is made at about half a line from the inner margin of the cornea, down to the sclerotica; with curved scissors the inner part of that membrane is dissected bare. In order to make the flap come forward and over the cornea, two short incisions are practised through the extremities of the first incision, towards the nose. (Fig. 1.)

(2d Step.) As soon as it is found that the flap will reach, on being stretched, the vertical diameter of the cornea—when the eye-ball is in the axis of the orbit—the external rectus is divided by a rather long perpendicular incision, just behind the insertion of its tendon.

The conjunctiva, which intervenes between this incision and the outer margin of the cornea, is reflected forwards, *and made to form a second flap.* (Fig. 2.)

(3d Step.) The two flaps are united over the vertical meridian of the cornea by sutures, three of which are generally sufficient, one in the centre, one just above and one below the upper and lower margin of the cornea. (Fig. 3.)

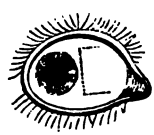


Fig. 1.

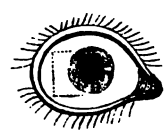


Fig. 2.

Fig. 3.

The eye now occupies the axis of the orbit; the inner rectus is stretched *to its utmost* limit, and the tendon placed over the most favourable site for

adhesion to the sclerotica; the external rectus retracts, and if the power of moving the globe inwards and outwards be tested at this stage, it will be found still to exist, but to a slight degree only, the antagonism being about equal.

The *after treatment* aims at securing the immediate union of the inner flap, and the preservation of the nutrition of the globe. These indications are fulfilled by keeping both eyes closed, and with the temples covered for about ten days by jewellers' cotton wool. The central suture is removed on the second or third day so as to obviate *corneitis*. Inflammatory chemosis, should it occur, must be met by the application of two, three, or four leeches to the temple, and pain in the eye by the internal use of morphia. If there be much puro-mucous discharge the local use of astringents is necessary. At the end of three weeks the conjunctiva should be trimmed of any uneven processes.

I have practised this method for upwards of four years with great success. —*Mr. Solomon's Paper on the Radical Cure of Extreme Divergent Strabismus, read before the British Medical Association at Cambridge, Aug.* 1864.

SYPHILITIC DISEASES.

79.—ON THE THERAPEUTICS OF SYPHILIS.

[Gonorrhœas, simple ulcers, and soft suppurating sores form by far the majority of venereal disorders; and mercury is unnecessary in their treatment. It is not, however, of these that the present paper treats, but of syphilis proper.]

Taking syphilis as a whole, there is no known remedy equal to mercury; none so reliable; and to certain complications, such as iritis, syphilitic orchitis, &c., this applies even more strongly. The stages in which it is of doubtful efficacy are the primary and tertiary. Of the forms, the pustular syphilides are the least amenable to it, and the scaly, chronic papular, and certain forms of chronic superficial ulceration the most so. For the tertiary stages, marked by diseases of bone and periosteum, unhealthy suppurative sores on the integument, the iodide of potassium is the best remedy. The above may, we think, be taken as a correct expression of the views entertained by the more moderate section of mercurialists.

[The writer considers that sufficient attention is not given to hygiene. The patient should be warmly clad; live upon a good but plain diet: take moderate exercise in the open air; use warm baths occasionally; and avoid stimulants unless specially indicated. The system must never be depressed, for the disease itself tends directly to induce a chloro-anæmic state.]

If a chancre be the portal through which the system is first tainted; if it be the "*continuing* source" of affection, as Dr. Humphry epitomises Virchow's doctrine; or even if it be a mark and effect of the general taint being already present, as many Continental writers, who have regard to the period of incubation which precedes its appearance, seem to think, its continuance must, nevertheless, be an abiding source of contagion to others, and it may be a focus of infection to the system, so that its early removal is not a matter of indifference; spite of the low appreciation in which the knife and caustics have been held by some high authorities (Dupuytren and Vidal). In practice we cannot determine, at early dates, what is the exact nature of any given ulcer. If it prove of the non-infecting type, then its progress is, without doubt, capable of being arrested by excision or destruction; if it be infecting, no harm is done. If a primary lesion can be excised thoroughly as well as early the practice seems to be good; but if no secondary symptoms follow, we can manifestly have no proof that the operation averted their occurrence. Mind, we do not say that excision or complete and early distinction of a *true* chancre may not be regarded as very good practice in influencing the severity and duration of the consecutive phenomena, for it may, and must indeed, effect this if every primary manifestation be a present and future focus of

contamination. But excision, at any rate, must have a very limited range for many obvious reasons—the sight of the lesion, the presence of induration in the glands, and refusal on the part of the patient.

If cauterants be used, these must be of the strongest and most penetrating, such as fuming nitric acid or the stick of potassa c. calc. In passing, we may remark that a rapid and continued affusion of cold water wonderfully mitigates the pain after the application of nitric acid.

What are the signs in the primaries from which we may anticipate severe and protracted forms of constitutional disease? Of course we must not set aside the constitution, previous and present health of the patient, as indifferent elements in forming a prognosis, but we will confine ourselves to the features of the disease, and, here, an approximation to the truth is all we mean to aim at. The density, size, and extent of the induration, and its persistence, in the chancre and inguinal glands; the amount of ulceration in the chancre, —these appear to stand in some connection with the nature and severity of the subsequent phenomena.

Now, it must be conceded that, do what we will, secondary symptoms will almost invariably ensue after true chancre. Mercury does not *prevent*—save in the most rare and exceptional instances—the occurrence of secondary disease. Mercury, says M. Diday, has been obliged to abandon the pretension of so *radically* curing syphilis as to render all relapse impossible. Its claims to use in the primary stage are—that it dissipates induration, and heals ulceration quicker than any other remedy, and it has the power, at least, of delaying the appearance of syphilitic symptoms. Some excellent observers go further than this, we are aware; some—as, for example, Dr. Marston—believe that mercury does so modify, in some instances, the course of the disease that the ordinary external manifestations are slight, or do not appear, although the patient may exhibit indications of syphilitic cachexia in his pallid aspect, impaired health, and liability to rheumatismal and other pains.

If, therefore, there be the history of a previous attack of syphilis, if the induration be moderate or small in amount, if the attendant ulceration be limited, if it do not prove very indolent, and can be healed by the application of local remedies, and the induration tends to disperse, and if, moreover, there be any *doubt at all* about the nature of the sore, mercury may be withheld. Wherever there is sloughing, rapid ulceration, &c., whether depending upon hereditary, constitutional, or other causes, mercury is a dangerous remedy, and requires to be used, if at all, tentatively.

Now for the so-called secondary manifestations. It is here that mercury appears to us of such undeniable utility that most practical men, and patients too, are agreed upon its use.

So far we have endeavoured to hold the balances even, and we must here ask ourselves two questions: is the prolonged administration of mercury (such as Ricord advised, for instance) necessary or justifiable? Does mercury always effect a cure, and is it necessary in all cases?

To the first, we may say, that it is always better to try the effect of the endermic use of the mineral by the moist calomel vapour bath, or by inunction, before we administer it internally; and we must add that we do not believe that any such prolonged courses are needed or advisable. When mercury has been continued for a long period with a view of preventing relapse, the health may be much impaired, a cachectic aspect acquired, and a tendency to a diseased action in internal organs engendered. But it is no good contending for a position which has long since been surrendered, for no one gives mercury in the wholesale way which characterised Hunter's day. Acknowledging many of the evils alleged against mercury, our own observation has led us to form very different conclusions from those advanced by the anti or non-mercurialists. We can only regard the long array of charges laid to its door as highly exaggerated expressions. *If mercury be exhibited with ordinary care and discrimination, and particularly if it be applied through the skin, there would seem to be little or no risk of injury to the constitution.* Among the large number of cases which we have so treated, we have *never* witnessed any of those disastrous effects described by many authors. *Of this* we are quite sure, that some of the very worst forms of constitutional

syphilis have appeared in those who have either taken no mercury or very little, and, on the other hand, the instances within our own cognisance are now numerous wherein the most remarkable recoveries from severe forms of constitutional syphilis have been effected by the aid of mercurials where every other treatment had been tried in vain. Those who are most severe in their denunciations of mercury—those who would seem to regard the use of the mineral with the greatest dislike, and who would debit every bad symptom which may appear in the course of a syphilis to the mercury used, instead of the syphilitic virus—seem to have lost sight of the fact that mercury alone has not only never been proved to be capable of giving rise to the combination of symptoms witnessed in constitutional syphilis when specifically treated, but that all experiment and observation is opposed to the assumption. If there be one thing more certain than another, it is that mercury was not employed for the treatment of syphilis for many years after the epidemic in 1494–5. We possess a history of its dreadful ravages then, and the descriptions given leave no doubt whatever of the terrible scourge that it then was, and of the effects it produced upon the bodies of the sufferers and the minds of those who were eye-witnesses. Of a given number of cases of true syphilis not specifically treated, all will exhibit secondary symptoms of some kind, and of these a certain proportion will be of a severe and chronic type, and followed by the phenomena of the so-called tertiary syphilis. The proofs of these statements are too ample for us to do more than allude to them. Let it suffice that many of the patients which served to swell the Hamburgh lists went elsewhere to be *cured* of later manifestations; of those treated by syphilisation, some have exhibited tertiary phenomena, and very many more will turn up, in all probability; and there seems to be very good reason for doubting whether any remedy, or any combination of remedies, be as efficacious as mercury in preventing the hereditary transmission through the germ. M. Diday, of Lyons, has established, by direct observation, that out of forty-three patients in whom the natural evolution of syphilis was carefully watched, in seventeen the symptoms assumed such a serious aspect as to threaten permanent injury to the constitution, and with these he was compelled to have recourse to mercury. Diday is among the *most moderate* section of mercurialists, and his *positive* results may be relied upon. But it must not be forgotten that the *negative* observations can never be equally certain, because we know too well, unfortunately, that syphilis may remain latent for long periods, and manifest itself after very long intervals. In looking over our notes of cases, we find that of thirty-eight primary syphilitic sores not specifically treated, twenty-one exhibited a mild form, eight a medium form, and nine a severe form of constitutional syphilis. How far these results might be modified if we possessed all the subsequent histories of these patients we cannot say, of course.

In order that our readers should more clearly perceive the tenor and spirit of our remarks on the therapeutics of syphilis, we must call their attention to the following illustrations of the forms, course, and progress of the disease.

We venture to think that a grouping into mild, medium, and severe types will be found to have some foundation in nature. We assume that the description applies to the first attack, because, as we have said already, there are either no subsequent ones, or these are very modified. The present and past health of the patients tolerably good: the subjects of no inherited taint.

1. *Mild.*—The chancre, as a rule, with but little induration, and affected with a slight degree of ulceration; the inguinal glands enlarged on one or both sides, and moderately so. Within from five weeks to three months, some enlargement of the cervical glands; vague muscular and articular pains; a slight febrile disturbance, perhaps, preceding the appearance of a dull-red, mottled roseola, with slight vascularity of the fauces, and very trifling loss of hair; subject to some manifestations of the disease, decreasing in intensity, at intervals, which become wider as the case progresses; iritis, if it appears, yielding easily to treatment; no (observed) tertiary manifestations.

2. *Medium Severity.*—Induration of chancre, and glands well marked and

persistent; moderate degree of ulceration, which tends to become indolent and chronic; chloro-anæmia and osteocopic pains pronounced; exanthem (later in appearance than in the mild) papular, scaly; marked congestion of throat, which at later dates ulcerates; sores on lips and buccal mucous membrane; iritis, occasionally tertiary symptoms (nodes, swelling of tendons, &c., with pustular disease of skin). Frequent recurrences of syphilitic manifestations, which last longer and have shorter intervals of quiescence than the previous form. The disease produces very important modifications of the health, but is chiefly dangerous to life from a connection with predispositions to other constitutional maladies (hereditary or acquired).

3. *Severe Type.*—Induration large and chronic, in chancre and glands; ulceration well marked and phagedenic, sometimes extensive, and liable to return after healing; chloro-anæmia, loss of health, emaciation, loss of hair, well marked and gradually increasing, making up a cachexia; osteocopic pains severe; eruptions discrete, and affecting deeper layers of skin, or pustular and relatively later in their appearance; iritis sometimes double, and often chronic or recurrent; throat affection severe; tertiary symptoms very common, affecting scalp, bones, tendons, larynx, or subcutaneous tissues, and often accompanied by some symptoms referable to diseases of internal organs. If engrafted in one the subject of some hereditary predisposition the effects may be permanent, or the cause of death, from acting as the excitant to other morbid processes. Of course, it is not meant that these limits are arbitrary, for they are approximative only; nor is it intended that all the symptoms should be present in any given case, but that these will conform to one or other of the above types.

In the first variety mercury is not *required* at all for the primary and may be deferred to the secondary stages, and then only used when any of the symptoms become intractable or chronic, or in certain kinds of iritis, or when a scaly syphilide appears. Warm baths, aqueous vapour baths, steel and diaphoretics, with strict attention to hygiene, often prove sufficient to cause a disappearance, at any rate, of the syphilitic manifestations.

In the second variety mercury alone appears to exercise any influence on the absorption of the interstitial deposits (indurations) present in the primaries, and frequently heals a chronic or indolent sore, when every other remedy has been tried; but mercury very rarely prevents the occurrence of the subsequent phenomena. For the cure of the constitutional manifestations, mercury, in the form of a calomel vapour-bath, is by far the best remedy, and it is almost invariably followed by *most marked benefit*, if employed with care and ordinary judgment.

In the third variety the mineral is also the most trustworthy remedy for the dissipation of the primary symptoms, and it may be used, unless plainly contra-indicated by some constitutional causes, or a sloughy state of the chancre, particularly if the ulceration or unhealthy action in the sore advances when the mineral is employed. For the secondary phenomena it should be tentatively, and only endermically used with much discrimination, while the most diligent efforts are being made to raise the vigour of the body by tonics, good diet, &c. For the later stages mercury generally appears to be most injurious, but fortunately iodide of potass, in large doses, and iron here come to our aid as very reliable agents.

Dr. Wilks seems to us to have gone to the root of this question, in the paper to which we have already referred, when he speaks of true syphilis and its sequelæ. The *effects* of the virus are shown in the albuminous deposition, while the *sequelæ* exhibit the degeneration of the tissues from the long continuance of the morbid action. Now, mercury opposes the first, but favours the second, hence the opposite views entertained about this drug.—*Med. Times and Gazette, March* 18, 1865, *p.* 282.

80.—ON THE UNITY OF THE SYPHILITIC VIRUS.

By Geo. G. Gascoyen, Esq., Surgeon to the Lock Hospital, and Assistant-Surgeon to St. Mary's Hospital.

There is no difference between the pustule which gives rise to the indurated

ulcer and that which results in the soft sore; nor between the early chancres themselves until induration has set in. The most strenuous advocate of the double virus theory has not attempted to diagnose the one sore from the other before this distinguishing feature has manifested itself: and we cannot be surprised, therefore, that grave mistakes have sometimes happened in experimental inoculation, where a very recent sore is usually selected for the purpose, under the impression that its secretion is more energetic than from one of older date. If, then, it be true for syphilis, as it unquestionably is for vaccinia, that the *condition* of the fluid inoculated determines the nature of the disease, whether local or constitutional, an explanation is afforded of those hitherto perplexing cases where, for purposes of experiment, persons have submitted to inoculations with the matter of soft chancres, considered by competent judges to be of the simple or non-affecting kind, and which, to the surprise and discomfiture of all concerned, terminated in ulcers which indurated and gave rise to secondary disease. It will account also for those instances—happily few—where indurated sores have arisen during the process of syphilization, when that horrible treatment has been practised upon persons for the cure of venereal sores, or for the *prevention* of constitutional disease.

There is no period of incubation for either the infecting or the simple ulcer, but a local action commences from the moment that the virus finds its way beneath the cuticle and comes in contact with the vascular tissues. Artificial inoculation therefore, by at once placing the matter under the skin, gives rise to a chancre more quickly than ordinary contagion, where some time elapses before the fluid can pass through the epithelial layers which cover the dermis.

The early symptoms are the same whether for the infecting or non-infecting sore: a little redness appears at the point of insertion; then swelling becomes perceptible to the eye and touch; a papule succeeds, on the summit of which a vesicle speedily forms, increases in size, and is surrounded by a bright-red ring; the contents become turbid, then purulent, and the characteristic pustule is seen, beneath which is the chancre. The period required for the evolution of these phenomena varies; but in the case of the simple sore the pustule is usually formed on or before the fourth day, whilst it would appear that a little more time is necessary for the development of the infecting chancre. If, then, a pustule were somewhat tardy in its formation, I should be inclined to suspect that it would give rise to an ulcer of the infecting variety.

When the chancres are fully established differences begin to announce themselves. The ulcer, which will remain a local affection, becomes surrounded by inflammatory swelling, extending more or less beyond its circumference, and gradually subsiding in the adjacent tissues; its margins, vertical and sharply cut, are undermined and angry-looking; its floor, irregular and worm-eaten, has a greyish unhealthy aspect, and it secretes purulent matter copiously which can be re-inoculated again and again. The inguinal glands are not usually influenced by this, but, when affected, it is a single gland which inflames and suppurates, the pus being sometimes of a virulent nature, capable of inoculation, and producing an ulcer similar to that which originated the bubo.

The chancre, which is the forerunner of constitutional infection, begins to lose its inflammatory margin somewhere about the eighth day, and to become circumscribed by an effusion of plastic lymph. This is manifest to the touch generally by the ninth or tenth day; it slowly increases, forming a complete bed for the ulcer, and extends deeply into the tissues around and beneath it. This induration has a peculiar cartilaginous feel and ceases abruptly, so that its outline is distinctly defined; there is no discoloration of the neighbouring skin, and, as the infiltration goes on, the edges of the sore, which are adherent to the parts beneath, assume a pale, shining appearance, showing the absence of all inflammatory action: the floor is smooth, red, and glazed, covered by a whitish, semi-membranous-looking secretion, which adheres closely, and is incapable of re-inoculation on the patient himself. When induration commences at the base of the ulcer the inguinal glands take on a simi-

lar action, becoming enlarged and hard, but painless, and continue in this condition for weeks, or even months; they rarely suppurate, and, if by chance they do so, the pus formed is never inoculable. The chancre has now assumed an indolent character, and the ulceration remains stationary for a considerable time, or else slowly increases *pari passu* with its bed of induration without any attempt at healing. This is not due, I believe, to any specific influence, but to a mere mechanical cause. The plastic effusion which constitutes the induration offers an obstruction to the free passage of blood to the part, so that there is not sufficient reparative power to heal the sore, or limit the ulceration; and now we see why local applications are incompetent to heal this chancre without constitutional remedies, and why mercury in such cases will often act as a charm.

This drug, by its depressing effect upon the system, appears to hinder the organization of plastic exudations, and is said to stimulate the lymphatics to increased activity, thereby promoting absorption of the indurated mass, and relieving the vessels from pressure, so that they can convey blood in sufficient quantity to enable the sore to heal. It is not until the induration has very sensibly diminished that cicatrization of the ulcer is obtained.

Another *questio vexata* now comes under consideration: How does the economy receive the virus, and why is it that the syphilitic poison should permanently contaminate and remain in the system, whilst vaccinal disease, with which we have compared it, and whose early symptoms so closely resemble it, should give rise to such very different constitutional effects, as the only point of resemblance between these two affections, after they have ceased to be local, is the creation of a diathesis which prevents any further infection?

It has been maintained, that as syphilis becomes a blood-disease, it must enter the economy by means of the blood-vessels; but a little consideration will, I think, convince us that it is impossible it can gain admission in this way. When an irritant poison is introduced behind the skin determination of blood to the part immediately ensues, engorgement of the vessels and the various phenomena of inflammation follow, ending in suppuration. Constitutional disturbance is at the same time provoked, its severity depending upon the amount of local irritation. This is a healthy action on the part of nature to be relieved of the irritant, and occurs after a dissection-wound or the insertion of any foreign substance. When, however, the virus of certain diseases—and by virus I mean the active contagious principle, the essential element of reproduction of a disease—is introduced into the circulation, it excites not only local symptoms at the seat of puncture, but mingles with the blood, and speedily shows its effect upon the system by a general febrile movement, accompanied by certain manifestations—usually eruptive—which are peculiar to, and symptomatic of, each of these several affections.

These symptoms would appear to be an effort of nature to rid itself, by a constitutional revulsion, of the *materies morbi* which is present in the blood; and if successful in doing so, certain elements of the blood having been eliminated or altered or destroyed by the action of the virus, there no longer remains a nidus in which the germs of this particular disease can again find a resting-place, and the person remains free for ever from another attack.

If we except that which produces hydrophobia, we may say that all known animal poisons, when admitted into the circulation, produce their acute toxhæmic effects *at once*. If the syphilitic poison entered by the same channels, it should do so too; but we know this is not the case, as a considerable though varying period elapses before what are called its "secondary symptoms" make their appearance.

The poison of syphilis creates a local lesion, and gives rise to symptoms which are at first almost exactly similar to those of other inoculable diseases; but whereas, at the moment when contamination of the system occurs, the latter give evidence of their entrance into the circulation by exciting a general febrile reaction, the former presents a marked contrast to them, by losing an inflammatory condition that may have attended it, and by the effusion of *plastic lymph* at the base of the sore, constituting what is termed "*specific induration.*" Directly this symptom has set in about the chancre, the edge

cent lymphatic glands become similarly involved, and give corroborative testimony of a general infection; the absorbent vessels, also, which lead to these glands may sometimes be felt as hard knotted cords, from a spread of the indura ion along their course. Does not this lead us to infer that the syphilitic virus, contrary to the other morbid poisons adverted to, enters the system *indirectly* through the medium of the lymphatics? The poison is carried by these vessels to the chain of glands in immediate relation with the sore, and is gradually passed on through the numerous sets of glands which successively intervene to prevent the admission of any noxious material into the circulation. During its progress the virus is subjected to the modifying agency of these organs, and by the time it has reached the venous system it is deprived of those active properties which an animal poison possesses when it is introduced *directly* into the blood; yet it has not altogether lost its power for harm, since it gives rise to a constitutional disease, which is usually ushered in with headache and fever, marking the moment when the virus has entered the general circulation. This vital effort is however slight, and totally insufficient to get rid of the poisonous matter, which contaminates the blood, traverses the body, vitiates its tissues, and remains in the system of the patient throughout life, rendering him at any time liable to an outbreak of the disease. A constitutional disorder is thus created which is capable of hereditary transmission, and, under certain circumstances, is even communicable to a healthy person by physiological contact.

This view of the manner in which syphilis infects the system receives strong confirmation from the fact which daily comes before us, that a person who is of a delicate or strumous habit of body—one in whom the glandular system is more especially at fault—suffers much more rapidly and severely from constitutional syphilis than is the case with one whose functions are unimpaired.—*Lancet, Dec.* 13, 1864, *p.* 737.

81.—ON SYPHILISATION

By the Editor of the British Medical Journal.

Dr. D. J. Simpson, in the 'Edinburgh Monthly Journal,' gives a paper on syphilisation read by him before the Royal Medical Society of Edinburgh. Dr. D. J. Simpson appears to be an out and out admirer of this new remedy. But he has, we conclude from his paper, no personal experience at all of its action. The following is a short abstract of his remarks. The announcement of Auzias-Turenne, that the syphilitic taint could be removed by syphilisation from the constitution, has, during the last eleven years, been verified by the experiments of M. Sperino, of Turin, and more especially by those of Professor Boeck, of Christiania, to whose labours we are indebted for the greater part of our knowledge of this wonderful remedy. The following is an abstract of two cases of very severe constitutional syphilis which defied the ordinarily employed curative means, but which, under the care of Professor Boeck, yielded at once to syphilisation:

Case 1.—Mr. —— contracted a chancre in 1853. After the lapse of a few months, the tonsils began to ulcerate. The ulcers, however, healed up under repeated applications of nitrate of silver. Iodide of potassium was then given internally, with sarsaparilla. No further symptoms manifested themselves until March, 1858, when an ulcer formed on the inner side of the right knee, which healed up after being blistered and strapped. In December, 1859, the tonsils and soft palate became extensively ulcerated. No local application seemed to be of any avail in checking the ulceration, and the constitutional treatment by iodide of potassium and sarsaparilla proved equally powerless. In August, 1861, the patient came to Edinburgh for advice, and was examined by several of the most eminent physicians and surgeons in that city. His tonsils and soft palate were almost entirely destroyed by ulceration. He looked quite cadaverous, and was so weak that he had the greatest difficulty in walking. His condition was the more alarming, as several *members of his* family had died of phthisis. He was advised to

proceed to Christiania, and submit, as a last resource, to treatment by syphilisation. This he did; and arrived in Christiania on September 1st, 1861. On his arrival he put himself under the care of Professor Boeck, who commenced treatment at once by inoculating him on the chest with matter from an indurated chancre; reinoculating every third day with matter from the previous pustules. At the end of a fortnight the patient's appetite, which he had almost lost, became ravenous. He could also walk without experiencing much fatigue. Three weeks later, inoculation failed to produce any result; the successive crops of pustules having become gradually smaller, the last proving abortive. Inoculation was then tried on the arms; and pretty large pustules followed. After three weeks' treatment, the pustules aborted here also. The patient left, *completely cured*, on December 1st, 1861, having gained three stones in weight, and expressing himself as "almost as well as he had ever been in his life."

Case 2.—Mr. —— was advised, in the summer of 1861, to go to Christiania and to place himself under the care of Professor Boeck, for a very severe attack of tertiary syphilis. Six years had elapsed since he contracted a chancre. In the summer of 1861, when he came to Edinburgh seeking advice, he had a large syphilitic ulcer over each tibia, and one over the left clavicle. Perforation of the palate had taken place, and some pieces of bone had come away from the interior of the nose. The frontal bone was swelled, indicating the commencement of a corona veneris. The ulcers were very obstinate, and continued to increase in size, in spite of treatment. The patient had previously undergone a complete course of mercury on two or three occasions. He had been treated by the heads of the profession in Edinburgh, London, and on the Continent of Europe, without experiencing the slightest benefit; and, despairing of recovery, he determined to go to Christiania. So reduced was he that he had to be carried on board the steamer on leaving Britain, and from it on his arrival in Norway. He underwent a course of syphilisation; and so rapid was his return to health and strength, that within two months after the time of his landing in Norway, he was able to hunt. After an absence of three months, he returned home perfectly cured. The cure in this case was the more remarkable, as the patient was labouring under an enormously enlarged liver, which he had acquired in China, when serving there as an officer in the army. Dr. Simpson saw both of the patients whose cases have been described, immediately after their return from Christiania. Both spoke in confident terms of the treatment. Up to the present time, neither of them has had the least sign of a relapse. Syphilisation may be practised either with matter from a *soft*, or with matter from an *indurated chancre*. Most late writers on the subject of syphilis maintain that the poison giving rise to an indurated chancre is essentially different from that which causes a soft chancre; but Professor Boeck has pointed out that both forms of chancre are the result of one and the same virus. According to him, it depends only on the difference of intensity of the virus whether a soft or indurated chancre be produced. Very intense virus gives rise to a *standard soft*, less intense, to a *standard indurated chancre*. Virus intermediate in intensity to these two extremes gives rise to intermediate forms, so that our prognosis must be always extremely guarded as to whether secondary symptoms will ultimately supervene. No satisfactory theory to account for the extraordinary disappearance of the constitutional symptoms under a course of syphilisation has yet been advanced. Dr. Danielssen, of Bergen, has mooted the idea that the action of the remedy is purely depuratory. In consequence of many persons holding this view, experiments were set on foot in Christiania to ascertain whether by means of irritants an equally happy result could be obtained as by syphilisation. Pustules produced by friction with tartar emetic ointment were the media employed. The committee appointed to investigate "Tartarisation" report as follows:—"The members of the committee all agree that they do not know any manner of treatment which works more efficaciously, or even so *efficaciously*, as syphilisation does in cases of secondary syphilis in individuals *not previously* treated by mercury." That the ulcers produced should *be syphilitic*, and syphilitic alone, a[illegible] be the great secret of success,

as is shown by Nature herself throwing off the disease by the production of tertiary syphilitic ulcers. In comparing the average of relapse after the different methods of treatment, we see that after mercury thirty-two per cent. relapse; after iodide of potassium, twenty-one per cent.; after syphilisation, only nine and a half per cent. Professor Boeck, however, since he has confined himself to inoculating with matter from indurated chancres, has not had to record a single relapse. The two modes of treatment which claim our chief attention, are syphilisation and tartarisation. After tartarisation twenty per cent. relapsed; after syphilisation only nine and a half per cent., or, according to Boeck's last series of experiments, not one. The greatly diminished chance of relapse, as well as the possibility of arriving at a certain point of immunity where we can pronounce our patient *cured*, which cannot occur with tartarisation—as in the latter case we can produce sores *ad infinitum*—shows the superiority of syphilisation over every method of treatment which has yet been proposed. At its first introduction, discredit was brought on the practice by its discoverer, Auzias-Turenne, proposing to employ the remedy as a prophylactic, like vaccination. This, of course, is revolting to the mind of every practitioner. Moreover, syphilisation does not act as a prophylactic, as one or two cases after having been cured by the treatment have subsequently contracted a fresh chancre, which has been followed by constitutional syphilis. Nor is its employment as a remedy for the primary sore in any degree a justifiable proceeding; for we can never tell whether any chancre will be for certain followed by constitutional syphilis. But as a remedy for the *constitutional* disease it stands pre-eminent, for syphilisation appears capable of curing *every* case of constitutional syphilis. Surely, instead of being summarily rejected, as it has been in this country, the remedy is entitled to a fair and impartial trial.—*British Medical Journal, Jan.* 28, 1865, *p.* 89.

82.—COMMUNICABILITY OF SECONDARY SYPHILIS BY INTERCOURSE.

By W. Allingham, Esq., Surgeon to the Great Northern Hospital.

[The question "Can the mother be infected by the semen without having been pregnant?" is one usually answered in the negative.]

I have several times seen cases which did appear to point very decidedly to syphilisation of the female by the seminal fluid without pregnancy. Two of my cases especially made a great impression on my mind, and with your kind permission I will briefly relate them, because this is really a very important and practical question; and I think if every surgeon who has opportunities of carefully observing the varied forms of syphilitic disease would record his cases, we might at least hope to arrive at a more settled belief on many disputed points.

A gentleman, aged twenty-seven, very healthy, and residing in the country, came to me and showed me an indurated sore on his prepuce; it had existed four weeks. His wife had been confined one week ago, and he had not been able to have intercourse with her for two months prior to her confinement, as she had been in very delicate health. He could trace the sore to an impure connexion five weeks before my seeing him. Three weeks after our first consultation he manifested well-marked, but not severe, secondary symptoms. The sore was healed in four weeks, and all induration had disappeared at the termination of six weeks, while the rash was fully out upon him. He, having no sore-throat, not the slightest symptom of the original sore, or any mucous tubercles, resumed intercourse with his wife, seven weeks having elapsed since her confinement. Between nine and ten weeks after the resumption of connexion, he brought his wife to me with decided secondary syphilis—squamous eruption, small scabs on scalp, sore throat and hoarseness of voice,—an examination with the speculum revealed ulceration of the cervix and copious uterine discharge of muco-purulent character.

It is now more than three years since this case occurred, and the child (which I ordered to be weaned directly I saw the mother) has not had any symptom of constitutional syphilis.

How did the mother become syphilised, if not through the seminal fluid of the male? I do not think that any deceit whatever was exercised towards me, and there was no reason why there should be. Both the wife and husband agreed that no intercourse had taken place for fully two months prior to her confinement; there was every reason to believe that she was a most virtuous woman, and there could have been no opportunity for her being otherwise; so, on the whole, I think it even more than probable that this was really a case where the seminal fluid of the male, without impregnating, induced constitutional syphilis. I would put it as a question, whether the uterus, seven weeks after parturition, was not more than usually susceptible of absorbing a poison?

Case 2.—An officer in the Peninsular and Oriental Company's service, after eight months' absence from England, arrived home two days subsequent to his wife's confinement. He was suffering from secondary syphilis, which he had been affected with some few months, and which had succeeded a primary sore contracted in Alexandria five months before he came home. For his secondary disease he consulted me. He had rather severe psoriasis, no sore-throat, no mucous tubercles, no sore, or remnant of one, on the penis. Between the fourth and fifth week after his wife's confinement he had intercourse with her, and three months afterwards he brought her to me suffering from severe secondary symptoms—ulcerated throat and rash. I was not able to make a vaginal examination. The child was weaned, and, when fifteen months old, presented no symptoms of syphilis. This, perhaps, is not so clear a history as the first case I have related, but still points most decidedly in the same direction.—*Medical Times and Gazette. Dec.* 10, 1864, *p.* 633.

83.—ON DIFFERENT MODES BY WHICH CONSTITUTIONAL SYPHILIS MAY BE COMMUNICATED.

By Henry Lee, Esq.

Until within a comparatively short period, the author observed, it was of late years a well-nigh universally received opinion in Europe, that syphilis could only be communicated by a primary syphilitic sore; that a chancre which was followed by secondary symptoms was always the result of contact with the secretion of a similar affection. Great and grievous have been the injuries inflicted by this theory both on single and on married persons. For anyone who in any way had contracted syphilis under the reign of this cruel dogma, was pointed at with the finger of scorn, was accused of having necessarily contracted the disease in some illicit way, and was not unfrequently most unjustly excluded from society. More than this: Cases not unfrequently occurred, and have occurred under the author's own observation, where legal proceedings have been threatened or actually carried into execution, in consequence of a person having, as was supposed, contracted fresh disease after marriage, the parties instituting the proceedings being misled by the doctrine which then prevailed. The following case was given in illustration:—A gentleman who had syphilis married. A few months afterwards his wife was found to have some indurated tubercles about the labia, with corresponding enlargement of the inguinal glands. A secondary eruption followed. The friends of the lady ascertained the nature of the disease, and being instructed according to the prevailing theory, accused the husband of having been unfaithful after his marriage. In this stage of the case the question was put to the author, "Can the husband have given the disease to his wife without himself having had a primary chancre?" Mr. Lee was enabled here entirely to clear the husband's character as far as his conduct subsequent to *his marriage* was concerned.

Nor is the evil which this false theory has produced restricted to private

life. Many and various have been the plans suggested for the prevention of the extension of disease amongst our soldiers and sailors; but while much pains have been at times bestowed to prevent the contagion of primary disease, comparatively little attention has been given to the subject of the equally contagious nature of certain secondary affections. It is to be hoped that the Government Commission appointed to investigate this subject will now take into consideration the whole of the facts calculated to illustrate the real nature of the disease, the best mode of preventing its extension, and the most approved methods of treatment.

Scarcely six years have elapsed since a most eloquent and able course of lectures was delivered, the principal aim and object of which was to prove that the whole series of secondary syphilitic affections were not inoculable, and most carefully and fully did the talented author of those lectures express the opinions which were then generally received. Since that time the subject had not, Mr. Lee believed, been brought fully under the notice of the Medical Society, and he had ventured, therefore, to do so on the present occasion.

Cases were then read illustrating the communication of the disease to a healthy woman from a diseased child by means of an indurated ulcer on the breast, and the communication of the disease by vaccino-syphilitic inoculation.

Numerous other cases of vaccino-syphilitic inoculation have now been recorded; and Dr. Viennois quite recently communicated to the Medical Congress at Lyons two fresh observations in addition to those which had previously been collected. On the 15th of May, 1862, six children were vaccinated. Five out of the six between three and four weeks afterwards had indurated ulcers on the inoculated parts. Each one of these five children communicated the disease to members of its own family, so that twenty-three persons were infected from this single source. The mothers and nurses of these children had first a chancre on the breast. The fathers of the children and the fathers of the nurses remained healthy until some form of secondary disease appeared on the private parts of the mothers and nurses, and then the husbands in some cases contracted the disease, which appeared in the form of a primary chancre. In a second case mentioned by Dr. Viennois, two children were vaccinated from the same source, and at the expiration of thirty-five days they both had an indurated chancre upon the inoculated parts.

Dr. Viennois had proposed, in order to prevent the recurrence of such serious mischief, that the inoculation of cow-pox or the horse-pox from the animals themselves should be substituted for the ordinary mode of vaccination; and it appeared that at Naples there actually exists an establishment for the purpose of carrying out this idea. The cow-pox is there inoculated on heifers. These heifers are driven about to different parts of the town as donkeys are here, for the purpose of furnishing donkeys' milk. When anyone wishes his children to be vaccinated, a heifer is brought to the door; the driver removes a vesicle with a pair of scissors and gives it to the doctor on a pair of forceps, who then vaccinates the children with its contents. The fee for this kind of vaccination is five francs. The author observed that in speaking of vaccination our foreign neighbours very frequently use the term "vaccine pustule," and he had reason to believe that want of accuracy here might lead to some very serious mistakes. The vaccine poison produced a disease which essentially was not pustular. The vaccine lymph, examined under the microscope, contained no pus-globules at the time when it was fit for use. If the vesicle were allowed to remain, no doubt pus-globules would be formed; and if the purulent fluid were inoculated, a suppurating sore might be produced instead of the proper vaccine vesicle. Attention to the nature of the inoculating fluid he (Mr. Lee) believed to be very important, and thought that many of the "bad arms" seen after vaccination might depend upon other matters besides the pure lymph having been inoculated. There could be no doubt, he remarked, that the action which produced a pustule was one of an entirely different nature to that which produced a vesicle; and unless these two actions were practically distinguished, medi-

cal men ran a great risk of inoculating a different disease to that which was intended. In these observations he did not allude to the presence of any separate specific poison in the inoculated matter, but simply to the kind of action which was intended to be communicated by the operation. If pus were inoculated, a pustule would probably result; it would be a suppurative action, and liable to all the complications which occasionally follow suppuration in a part. The author was informed by Mr. Tomkins, of the National Vaccine Establishment, that he had seen very serious constitutional effects follow the inoculation of the cow-pox taken from the cow. How far such effects depend upon pus being inoculated instead of lymph demanded our serious consideration. In the interest of science, we were called upon carefully to distinguish these two actions, and in practice certainly vaccination should never be performed with matter taken from a pustule.

In the year 1854, the author published some lectures on syphilis which had been delivered the previous year at the Lock Hospital; and in the fourth lecture, he dwelt upon the two kinds of results produced by syphilitic inoculation. He then stated: "In one case the appearances are in their origin—those of the adhesive inflammation; in the other, of the suppurative inflammation. And these constitute two very different classes of disease, both in respect to the local affections and their constitutional consequences." At this time every syphilitic inoculation was generally supposed to produce a "pustule;" and the production of this pustule was not only taken as the sure evidence of the syphilitic nature of the disease, but as a test for the necessity of giving mercury for its cure. Had we earlier learnt to distinguish the different kinds of morbid action which syphilitic inoculation gives rise to, much of the confusion which has reigned with regard to this branch of knowledge would have been avoided. It is now generally admitted that real syphilitic eruption does not produce a pustule; that the disease which infects a patient's constitution commences as an abrasion, a pimple, or a tubercle; that the disease which does produce a pustule when inoculated is a disease of an entirely different kind, and that its effects are entirely local. Those who, therefore, maintained that the production of a pustule by inoculation was an indication for the administration of mercury, were advocating exactly the reverse of that which more recent observation has established.

There can be no doubt but that the real infecting form of syphilis, commencing with a form of adhesive action, will, during some period of the existence of the primary affection, give rise to a puriform secretion, just as the vaccine disease will, after the ninth or tenth day, produce some pus-globules; but that does not in any way prevent us from recognising the essential character of the disease, in its origin and in the induration which is subsequently produced, as quite distinct in one case as in the other from any purely suppurative action. Mr. Lee was led to dwell somewhat upon this subject as the distinction which he drew between the two kinds of syphilitic sores as far back as 1854 has been called in question by some of our continental brethren. They have said that a true chancre will secrete pus; a fact which Mr. Lee had always allowed. And so will, he observed, a vaccine vesicle, especially if irritated.

In one case as in the other, the disease begins without suppuration; and, if accidental causes of irritation be prevented, a very few days will leave the disease marked by the circular induration, and unaccompanied by any suppurative action whatever. One of the author's critics had supposed that in some drawings published last year of the microscopical appearances of the secretions of the two different kinds of syphilitic sores Mr. Lee had fallen into a mistake from not knowing the appearances of a pus-cell under the microscope. Mr. Lee thought he might at least have escaped this criticism had he mentioned that his friend, Prof. Beale, had been good enough to make the drawings in question for him.

The two kinds of action which were now described were very important, as enabling us to understand some forms of inoculation which otherwise would *appear to* follow no fixed law; particularly cases in which there has been a *twofold inoculation*, and where a mixed form of chancre has resulted. Such cases are not unfrequently observed in practice, but the evidence of their

twofold origin is there not so satisfactory as if it had been the result of experiment. The evidence from direct inoculation is not, however, wanting. M. Melchior Robert has shown that if the secretions from the two kinds of chancres be inoculated by a single puncture on a healthy person, a soft chancre originating in a pustule will first appear, and subsequently an indurated chancre followed by secondary symptoms. M. Lindwurm has proved that if the secretion from an indurated sore be inoculated on a soft or suppurating sore, that sore will gradually be transformed into an indurated chancre, which will be followed by secondary symptoms; and M. Rollet has often demonstrated the fact that if the pus from a single chancre be placed on the surface of an indurated sore, the affection will assume all the characters of the "mixed chancre."

The limits of the present communication would not allow the author to go more fully into the subjects of the different kinds of secondary affections which are communicable by inoculation, nor of the inoculability of different kinds of secretion, as the milk, and the semen derived from a syphilitic subject. He would only remark, that, so far as his own knowledge went, he was not prepared to admit the inoculability of any secretion derived from a syphilitic subject without some increased or diseased action in the organ producing that secretion. Thus, the saliva from a syphilitic person with a healthy mouth would not, so far as he knew, produce syphilis in another person. The milk from a healthy breast of a syphilitic woman would not produce the disease in the mouth of a child. The semen from a syphilitic person with healthy organs of generation would not produce syphilis by being brought into contact with the mucous membrane of the vagina. But let any of the parts become diseased, or let them be subject to increased action in a syphilitic person, and then the secretions from those organs have a power which they had not before, and might communicate the disease direct to any part with which they came in contact. The following case was given in illustration:

A gentleman requested to know what was the matter with him, and was informed that he had a chancre which would infect his constitution. He said it was impossible, as he had never seen but one person, whom he had known for a long time, who had nothing the matter with her, and who was quite willing to be examined, if the fact were doubted. This person was accordingly examined. The mucous membrane of the vagina was red and congested, and a muco-purulent discharge issued from the vagina. There was no sore or abrasion of any part. On examining the chest and arms a distinct syphilitic eruption was visible, which she said she had had for some years, and believed to be the "nettle rash." It was then apparent in this case that the man had not been infected by this person, although in the way of becoming so, until, from some cause or other, some local increased or diseased action was produced in the organs of generation, and that then the secretion from those parts was inoculable. In this case the patient obtained the certificates of two medical men to say that she was free from disease.—*Lancet, Dec.* 10, 1864, *p.* 661.

84.—ON THE DIAGNOSIS BETWEEN SYPHILITIC AND NON-SYPHILITIC DISEASES OF THE SKIN.

(An abstract of a paper read before the Harvelan Society by Dr. B. Squire.)

The author, after remarking on the importance of the subject, observed that an eruption, even when the question of antecedent primary syphilis was well made out, was by no means necessarily of syphilitic origin; and that where an eruption was clearly syphilitic, it was often extremely difficult to obtain a history of primary syphilis. He, therefore, placed very little reliance on inquiries of this kind as a means of diagnosis. The best evidence that we could obtain of the nature of an eruption was the evidence that was within our personal knowledge. The most reliable direct evidence of previous chancre was the cicatrix left by that chancre. One of the most important *means of distinction* between syphilitic and non-syphilitic eruptions

was the situation of the eruption. The most frequent situations for syphilitic eruptions in the order of their frequency were the neighbourhood of the alæ of the nose, the angles of the mouth, the forehead near the roots of the hairs, the back of the neck near the roots of the hairs, the inner canthus of the eyelids, the middle of the chest, the inner surfaces of the limbs, the neighbourhood of the axillæ and groins. It might be stated generally that the face was the most favourite locality for syphilitic eruptions. The age at which syphilides most commonly appear for the first time was another means of diagnosis. The most common age for this was between twenty-five and thirty-five. Infantile syphilis appeared generally at the age of one month. The earliest time at which the author had met with a syphilitic eruption dependent on syphilis communicated by intercourse was in the case of a female aged eight years. The colour of the eruption was a favourite, but, unless under certain restrictions, a fallacious means of diagnosis. He meant that the notion that a coppery tint would enable one to distinguish syphilitic from non-syphilitic eruptions was an erroneous one; this tint was found more or less marked in every case of non-syphilitic psoriasis. Again, syphilitic eruptions did not present it at all at their outset; it was only as they became developed that the coppery tint gradually appeared. The form of the eruption was a more reliable means of diagnosis. Most commonly this was annular or oval. If not in actual rings, the eruption formed segments of circles—incomplete rings; or by the juxtaposition of two rings, it might form figures of eight. This, of course, however, would not enable us to distinguish, as by a touchstone, syphilitic from non-syphilitic eruptions. Psoriasis, *e. g.*, sometimes occurred in rings; so did lichen; so again did herpes circinatus. Lupus was another example of a non-syphilitic eruption of annular form. The absence of itching and smarting was a peculiarity which would often aid the recognition of a syphilide. The mixed character of syphilitic eruptions was another means of distinguishing them. Thus, in an eruption that was not syphilitic, it was rare to find rashes mixed with vesicles, or pustules with scales. Not so in a syphilide. What might be termed the "products of the eruption"—such as the scales or the crusts—were a means of diagnosis. The scales of a syphilitic eruption were finer, smaller, and more adherent to the surface beneath than those of a simple squamous disease. The scaly patch, too, was circumscribed by a whitish border, to which great importance had been attached by Biett as a diagnostic symptom. This whitish border was the result of the separation of the epidermis from the cutis around the diseased patch. The crusts, which might either follow the scales or succeed to pustules, were much harder, thicker, and greener, than the crusts of a simple cutaneous disease; they were sometimes, at all events at their centre, as thick as they were broad. The ulcers that were left by either pustular or tubercular syphilides were generally circular, with abrupt and perpendicular edges, and a grey pultaceous floor, the skin around them having a copper tint. The scars left by a syphilide, though at first of a violet hue, soon acquired even a more tawny brown tint than the eruption that had preceded them; this tint in its turn at length faded, but even when the scar had at length been left perfectly white, its annular or reticulated form, as the case might be, would distinguish its origin. Another means of recognising a syphilitic eruption was attention to other changes produced by syphilis besides those wrought in the skin itself. With regard to the sore-throat, we should be on our guard against relying too much on it as a diagnostic symptom. There were many diseases of the skin (and some that resembled pretty closely one or other of the syphilides) that were dependent on scrofula, or, at all events, occurred more frequently in the scrofulous than in any other constitution. No people were more liable to sore-throats than the scrofulous, and perhaps no two diseases were more frequently confounded together than syphilitic lupus and the lupus that occurs in the scrofulous. In doubtful cases the question, "Have you suffered from sore-throat?" is often allowed to settle the difficulty. The most characteristic condition of a syphilitic sore-throat was either the presence of mucous tubercles or of cicatrices. The presence of mucous tubercles, either in the throat or elsewhere, was one of the most conclusive signs of syphilis that one could have. The

erythema of the buttocks and scrotum, that was so common in sickly children whose cleanliness was hardly attended to, was often mistaken for infantile syphilis, and if they happened to have a cold at the same time, it was still more likely to be set down to a specific cause. This mistake happened the more commonly that simple erythema in infants, as it faded, often assumed more or less of a tawny tint. The effect of the common remedy for infantile syphilis—grey powder—on the other class of cases, was greatly to aggravate them. Again, the author had frequently seen severe cases of eczema in infants taken for infantile syphilis, and treated for it with the same unfavourable result. There was no better or surer means for establishing the disease to be syphilitic than the existence of mucous tubercles when these were present, and in the majority of cases of infantile syphilis they were. Their most common situations were, the angles of the mouth, the alæ of the nose, the vulva, the anus, the scrotum, or within the mouth. The author then referred to rheumatic pains in the limbs, along the bones, or in the larger joints—to bitemporal neuralgia, to falling off of the hair, to indolent swellings of the glands of the neck, and, in the female, frequent abortion, or still-births, or suspension of the catamenia (not arising from any other ascertainable cause), as evidences of the syphilitic nature of an eruption, and to the chronic course and the progressive changes in the character of the syphilides, observing, however, that it was not upon any one symptom alone, but on the general issue of the evidence afforded by many symptoms that we should rely for our diagnosis of syphilides from the other diseases of the skin. He concluded by expressing his regret that the limits of a paper precluded his entering on the much larger subject of the diagnosis of the different syphilides from the several diseases of the skin for which they might be taken.—*Medical Times and Gazette, Jan.* 28, 1865, *p.* 103.

85.—CHANCRE AND SYPHILITIC LICHEN ATTENDED WITH INTENSE PRURITUS.

(Case under the care of Dr. Sisson, at the Royal General Dispensary.)

In September a German, aged twenty-six, presented himself, and stated that ten days or a fortnight ago he observed a small pimple on the glans penis, which, from the intense itching accompanying it, he was compelled to scratch. This he had done constantly to the present time, and he was now afraid that the penis was mortifying.

On examination, a large indurated chancre was discovered on the glans penis, covered with a black scab; the parts around were much inflamed, but there was *no bubo*. Ordered a lotion of lead and opium, which was changed in a week for one of sulphate of copper, and a mixture containing bichloride of mercury and iodide of potassium thrice a day. In three weeks the chancre healed.

On the 7th December the patient again presented himself, suffering from sore-throat, enlarged post-cervical glands, and syphilitic lichen on the face, chest, and back. He stated that the eruption itched so violently when in bed that he was unable to get any rest. Ordered one drachm of the solution of bichloride of mercury with five grains of iodide of potassium in camphor mixture, thrice a day.

Dec. 17th. Sore-throat well; eruption on back and chest has disappeared entirely; that on the face nearly. Itching had entirely ceased after taking the mixture three days.

Dr. Sisson has always subscribed to the opinion that the absence of itching is one of the most valuable diagnostic signs in syphilis, and this case only proves that there is no rule without an exception. The case, he says, is also valuable as showing the fallacy of Ricord's dogma, that there can be no infecting chancre without an indurated bubo. The absence of eruption on the limbs and its presence on the face were strong presumptive evidence that the lichen was not intermingled with scabies, and the result of the treatment has, he thinks, clinched the matter.—*Lancet, Dec.* 31, 1864, p. 743.

86.—MERCURIALISED COLLODION AS AN APPLICATION TO CONDYLOMATA.

A patient aged fifty-six, under the care of Dr. G. Finco of Padua, had around the anus a large number of condylomata, which had increased in size under the use of nitrate of silver. Dr. Finco mixed twenty five *centigrammes* of corrosive sublimate with fifty-two *grammes* of collodion, and, having shaken the mixture well, applied it with a brush over the two largest tumours. The next day, these had almost disappeared. In the course of sixteen days, Dr. Finco destroyed more than sixty condylomata; and the patient had no return of his disease.—*British Medical Journal, Jan.* 14, 1865, *p.* 44.

87.—ON THE NON-MERCURIAL TREATMENT OF SYPHILIS.

(Cases under the care of R. W. Dunn, Esq., at the Farringdon Dispensary.)

Case 1.—M. W., aged two months, came under notice on August 1st. The mother of this child had suffered from sore-throat and eruption of the skin, and had been in-patient at one of the London hospitals for some months on account of it. The infant first showed the sign of an eruption at four weeks. When first seen, it had syphilitic psoriasis on the buttocks, snuffled very much, and was gradually wasting. Two grains of chlorate of potash in water were ordered to be taken three times a day.

Aug. 28th. The mother brought the child, fearing it would be suffocated. It could scarcely breathe through the nose, which was ordered to be well syringed with warm water, and the medicine continued.

Sept. 22nd. The infant had taken the medicine up to the present time, and was discharged cured.

Case 2.—C. M., aged three months. The mother of this child had suffered from constitutional syphilis, and had three miscarriages before this infant was born. The child had been ill for five weeks previous to its coming under Mr. Dunn's are, and upon examination was found to be very debilitated and much wasted, with a syphilitic eruption on the legs and arms, mucous ulcers on the lips, and snuffles. The same medicine was ordered as in the previous case. The little patient came under treatment on the 11th of August, and was discharged cured on the 22nd of September.

Case 3.—N. W., aged two months. This child's mother had suffered from ulcerated sore-throat more or less for a long time, but there was no distinct history of syphilis. The child had a syphilitic eruption on the face, body, arms, buttocks, snuffled very much, and did not take the breast so well as usual. Ordered two grains of chlorate of potash three times a-day. This treatment was commenced on August 15th, and continued until Sept. 20th, when the patient was discharged cured.

Oct. 20th. A relapse having occurred, the same medicine was again ordered to be given.

Nov. 3rd. Discharged cured.

Nov. 10th. Had an attack of measles. No relapse since.

Case 4.—G. G., aged four months. Both parents syphilitic; the mother had four living children previous to this one, and frequent miscarriages. All the former children died before six months, and all had eruptions, which had been treated with grey powder and mercurial ointment. Upon examination it was found that this child had syphilitic eruptions on the buttocks, head, arms, and legs; ulcer on the anus; snuffles; is very thin and aged in appearance. Ordered it two grains of chlorate of potash three times a-day. The medicine was continued till Nov. 24th, when the patient was discharged cured.

Case 5.—H. F., aged thirteen weeks. No history; a nurse child. Had syphilitic eruptions on the buttocks, ulcers on the anus, and mucous ulcers on the lips; rejects food; very emaciated; diarrhœa, extremities cold.

Oct. 6th. Ordered it injections of strong beef-tea by the anus—a table-

spoonful about every four hours,—and a drop of the tincture of opium by mouth in a little chalk mixture every six hours.

8th. Diarrhœa and sickness stopped. Ordered the child to be fed upon milk and water and a little beef-tea, and to take two grains of chlorate of potash three times a day.

14th. Much better, and continued improving. Discharged cured Nov. 24th.

Case 6.—L. J., aged six months. Was quite well till vaccinated; four weeks after an eruption appeared on the back, legs, and face; snuffles. No history of syphilis. Upon examination this child was found to be very much emaciated, with an eruption on her back, legs, and arms; ulcer on the anus; snuffles. The infant's face had that peculiar aged look which is so often seen in syphilitic children. Ordered three grains of chlorate of potash three times a-day on Nov. 3rd.

Continued the medicine till Dec. 1st, when it was discharged cured.

Mr. Dunn has related these six cases from amongst thirty of a like character, which he had treated in a similar manner, with only one death.

Statistics as to age when the first appearance of the symptoms had been noted.

				Cases.
Before the completion of	one month	after birth	...	4
"	"	two months	" ...	9
"	"	three months	" ...	7
"	"	four months	" ...	1
"	"	five months	" ...	1
"	"	six months	" ...	1
"	"	seven months	" ...	1

—*Lancet, Dec.* 31, 1864, *p.* 742.

88.—ON TWO NEW SPECIFIC REMEDIES FOR GONORRHŒA.

By Dr. Thos. B. Henderson, Glasgow.

[The following paper was read before the Glasgow Medical Society. The paucity of remedies for gonorrhœa has been regretted by almost every general practitioner.]

The first of the medicines I have to introduce to your notice is the Oil of Yellow Sandal wood. It is obtained by distillation from the wood of the tree Sirium myrtifolium, of the genus *Santalum*. It grows in the East Indies. One lb. of the wood yields 2 drams of the oil. Lindley writes, "This oil is said to be used to adulterate the oil of roses." Professor Redwood, in his Supplement to the Pharmacopœia, on the authority of Dr. O'Shaughnessy, writes, "Sandal wood in powder is given by the native physicians in ardent remitting fevers. With milk it is also prescribed in gonorrhœa." I have ransacked many works on Materia Medica, and have not found even the name of Yellow Sandal wood.

In my experiments with this drug I have found it perfectly innocuous even in large doses. From 20 to 40 minims three times a-day diluted with three parts of rectified spirit, and flavoured with ol. cassia or ol. cinnam., is the ordinary formula I employ; water and a confection after. In cases of the disease at the first, second, or third stage, in susceptible persons, I have often seen the most marked suppression of the discharge within forty-eight hours. It has the great advantage of being a pleasant medicine, not liable to cause sickness, agreeable to the taste, and grateful to the stomach. It is a medicine as to efficacy, in my opinion, equal, and frequently superior, to bals. copiab. or cubeb pepper. I have often succeeded with it, when both had been fairly tried, and failed. Besides, it is convenient and portable; and if the patient is delicate, or in bad health, or the system disordered, the possession of a remedy which will act as a stomachic medicine, and cure the disease, is, I think, to be highly valued. I have used it in many cases during the past five years. I have no theory to offer as to its mode of acting. My experiments have been numerous, but entirely of a practical character. The

odour of the drug is slightly perceptible in the urine. Its action on ... is observed, in susceptible cases, within a few days after beginning its ... Almost every druggist keeps it for perfumery purposes.

The other remedy I have experimented on, is the Gurjun or Gurgina balsam, or wood oil. It is the product of the *Dipterocarpus turbinatus*, an immense tree growing in different parts of India. Incisions are made and heat by fire is applied to the root. One tree yields about 40 gallons in a season; distilled with water, it yields 35 per cent. of volatile oil. Wood oil is a liquid of the consistence of olive oil, of a dark reddish colour and slight odour. Pereira gives a good account of this medicine when speaking of the adulterations of bals. copaib. In the new edition of Royle's Materia Medica, p. 319, it has the honour of occupying one line. Referring to the products of the Dipterocarpæ, it is written, "There is a wood oil which contains a principle analogous to copaiba." In the other works of Materia Medica it is either not mentioned or only slightly noticed. The description of this medicine which caused me to try it for myself, is contained in the valuable "Manual of Practical Therapeutics," by Edward John Waring, of the East India Company's Service. Smith, Elder, & Co., Cornhill, 1854, pp. 200. From which I beg to make the following extract.

"Gurjun or Wood oil tree is found at Chitagong, Pegu, the Tenasserim Provinces, &c. It is found abundantly in all the bazaars of India. By distillation it yields an essential oil, which in all its medicinal properties and actions closely resembles copaiba. Dr. O'Shaughnessy employed it in numerous cases of gonorrhœa and gleet; and the results seem perfectly conclusive, that in the treatment of these and other affections of the genito-urinary system, the essential oil of gurjun is nearly equal in efficacy to copaiba. It generally causes a sensation of warmth in the epigastrium, eructations, and sometimes slight purging. It greatly increases the quantity of the urine, which has a terebinthinate odour. Dr. O'S. found that some obstinate cases of gonorrhœa and gleet, which had long resisted copaiba and cubebs, were cured by this remedy. E. J. Waring writes, 'In the few cases I have had an opportunity of trying it, the results have been uniformly satisfactory. It might be advantageously introduced into English practice as a cheap and efficient substitute for copaiba. The dose is 10 to 15 drops, thrice daily.'"

It is now several years since I commenced to experiment with wood oil. I have only used it in cases where copaiba had been fully tried and failed. In every case it was successful within a week. No symptoms of inconvenience in any of the cases were produced. I gave it in what may be called large doses, a teaspoonful two or three times a-day, *uncombined*. I have not been able to investigate its action further, as my supply became exhausted, and is not easily procured in this country. I am thoroughly convinced it is excellent medicine. I think it is probable this oil was introduced into England without there being a demand for it, and those holding it tried to quit of it by mixing it with copaiba. Being detected, the cry has against its use in that way, and very properly so, fulfilling the old saying, "Give a dog a bad name, &c." That it can be procured abundantly moderate price, I have no doubt.—*Glasgow Medical Journal, April*, 1865.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

89.—ON SOME OF THE SYMPTOMS OF EARLY PREGNANCY.

By Dr. C. H. F. Routh, Physician to the Samaritan Free Hospital for Women and Children.

[The symptoms upon which reliance is usually placed in forming a diagnosis are well known, but Dr. Routh in the following paper introduces what may be called an additional symptom, which can in most instances be detected.]

Hitherto much reliance has not been placed on *auscultation* in the first period of pregnancy. The so-called placental souffle, if heard at all, has been heard quite at the end of this period. Dr. E. Kennedy of Dublin, it is true, has given some examples in which he was able to hear it with certainty at the twelfth, eleventh, and even in one instance at the tenth week; but his experience has not been confirmed by others. Nägele states that it is only heard after the fourth month. Rigby admits that it can be detected as early as the fifteenth or sixteenth week. Murphy states that it is not generally heard before the fourth month.

Years ago, Dr. Rigby premised that there was every reason to suppose that it might be heard at a still earlier period, if the uterus were at this time within reach of the stethoscope. The object of this communication is to show the means by which I have attained to this end; and henceforward, I imagine, it will be in the power of all occasionally to foretell pregnancy by this symptom as early as from the seventh week to the ninth, probably even from the sixth, when the placenta is situated near the cervix. At any rate, this is the earliest period at which I have heard the distinct souffle. At this early date, however, the sound differs somewhat from the placental souffle, as heard at a later period. Nor is it very easy to give an accurate description of it in words. It is more like the sudden cessation for a moment of a general splenic murmur. If I may so express it, it is a sudden interruption of an intensely vesicular and muffled murmur, synchronous with the pulse. This it is which makes it very difficult to catch sometimes, and which requires the closest attention and a most practised ear to make it out at all. I have also observed that the souffle, when it does occur at a very early period, and when very distinct, has somewhat a high pitch; so far confirming the opinions enunciated by authors, who have described it as assuming a piping character when the placenta is attached near the cervix. Curiously enough, in those cases where I have heard this high-toned pitched murmur *per vaginam*, I have heard it very early also in the iliac region of one side.

It is but right, however, to add that, as with ordinary auscultation-sounds heard over the abdomen, so with these pelvic sounds, they may be absent altogether. Sometimes we may lose them at a sitting, and are not again able to find them. Thus much, however, I think we may safely conclude, that during the first period, and in a doubtful case, no examination is complete without the use of the vaginoscope.

The cases now related amount to nine, in which the pregnancy was detected as early as from the sixth to the thirteenth week. All but one were married, and therefore there was no object in deceiving me; and, as I saw them all purposely, and by special appointment, to make out whether pregnancy existed or not, no difficulties were put in my way in the examinations made.

Of course, these do not include all the cases which I have examined or noted *particularly from the* sixth to the thirteenth week; but they are fair

samples among several others, a few of which I regret I have been obliged to set aside, as I omitted to note at the time the date of their first examination by me. In these also, however, the pregnancy was made out very early. I have named these several instruments which I now show you *vaginoscopes*. Perhaps, were I to adopt the plan followed in the words *stethoscope* and *ophthalmoscope*, it would be better to call them *coleoscopes*, using two Greek words, instead of a compound of Latin and Greek. The examination of the uterus was made with these instruments introduced within the pelvis in other ways. Thus, a flexible tube passed up *per anum*, with an earpiece at the distal end, answered for auscultation. This method, though, as it might be supposed, useful, was sufficiently disgusting to discourage any further attempts. I have also attempted to hear through the sacrum; but the indications here were very uncertain. Stethoscoping *per vaginam* is not new. I am told that the late Dr. Stroud had devised a plan to effect this, and, if I mistake not, some German author; but I have not yet been able to obtain their memoirs. An examination *per vaginam* does not, however, in these days entail the same reprehension as formerly. Thus my facilities have been greater.

If a woman be placed on her side, with her legs very much flexed upon her abdomen, then auscultation may be readily made with a common stethoscope, provided the distal end be small, by pressing directly with it against the perineum. This, by the apposition of its internal surface, impinges against the vagina and uterus, and so we can often hear sounds originating in the latter organ. Where, however, the woman is fat, and the perineum thick, the sounds are muffled. In this case, it may be passed *per vaginam*, and made directly to impinge upon the uterus. With a glass syringe, in doing this, as the rubbing of the clothes is not heard, no exposure whatever is needed. The same auscultation may be made with the woman on her back, with a curved glass speculum such as I show you.

It would be quite a little history, were I to enumerate the several plans I have adopted in the hope of devising a more perfect instrument for the purpose. I have tried metal stethoscopes, bent at a right angle, sometimes using a thin membrane at the vaginal end, of India rubber. I have used metal tubes, with a membranous sac in the interior, so constructed that I might blow air into it at will, and make the sac more or less tense. I have used wooden vaginoscopes, with a membrane interposed, at the union of the vaginal with the external portion of the stethoscope, which was here enlarged so as to form a cavity of greater dimensions than on any other part of the stethoscope. This cavity I have made exactly elliptical, so that the sounds conveyed would be concentrated at a focus, and be carried thence by a tube to the ear, upon the principle of Mr. John Marshall's ear-trumpet. The object of the membranes and membranous sacs was twofold: to reverberate, and thus intensify in the cavity the sounds, as is the case with the tympanum. Secondly, the small apparatus with which it was connected, and by which it could be tightened or slackened at will, was to alter the pitch, so that sounds might be heard which the uterus might convey, on a membrane as tense as would produce the note conveyed by the womb-sounds. This, however, in practice, was found in no way to act as I expected. Sounds appeared to be equally well transmitted without as with the membrane, while a double membrane seemed rather to muffle the sound than otherwise.

I have even used a metal vaginoscope with fluid compressed within it by a membrane, to try if I could feel thereby any wave of pregnancy or fluctuation conveyed.

In the present stage of my inquiries, I have come to restrict myself to two varieties—the glass stethoscope, and the wooden vaginoscope.

There is nothing new in the first. It has been used for a long time; and glass has a single advantage. It is a beautiful conductor of sound, and rather, I think, raises the pitch. At any rate, it enables you to hear more distinctly than with the ordinary wooden stethoscope. Add to this, it is very *readily kept* clean, and does not make any noise when rubbed against the *clothes*. This, as I before said, is a great advantage, and allows it to be used *very modestly*; and when manufactured in the shape I now show you—that

is, long, and with a curve—is very easy of application. These were made by Mr. Maddox of London.

The second variety includes the single and the double vaginoscope. These were made by Mr. Coxeter. You will see that the vaginoscope, as I use it, is nothing more nor less than either the single or double wooden stethoscope, with elastic tube, *to the distal end of which is affixed a wooden speculum*. Occasionally I use a solid tube in lieu of an elastic one, made either of gutta percha or of wood. I am satisfied, however, that, were this manufactured of glass, it would be preferable.

These, then, are the instruments. One word now about their application in diagnosis.

My first business was to ascertain what were the normal pelvic sounds heard in a non-impregnated woman. There may be none. Occasionally, where there is much uterine congestion or vaginal inflammation, you may hear a regular pulse. This is, however, rare, and is probably the vaginal pulse. Commonly, you hear the gurgling in the intestines, passages of wind like bronchial respiration, and of fluid. These sounds, curiously enough, I have not heard in pregnancy, except occasionally the pulse before referred to. I imagine this to be due to the non-transmission of sounds through fluid, which the uterus contains in the early as well as in the more advanced periods of pregnancy.

The murmur of fibrous tumours presents some resemblance to that of pregnancy. Having, however, so fully developed this subject in my Lettsomian Lectures, I need not dwell here on this subject. Suffice it to say, that the true *fibroid tumour* is tubular, and usually louder, and often accompanied with a thrill, and a single or double cardiac sound, synchronous with the first or both sounds of the maternal heart. The *placental murmur* is intensely vesicular, without thrill, and usually without the cardiac sounds conveyed from the mother, except under very exceptional circumstances.—*British Medical Journal, Nov.* 26, 1864, *p.* 593.

90.—ON THE SICKNESS OF PREGNANCY.

By Dr. James Patterson Cassells, Glasgow.

In the morning, an hour before rising, let the patient drink about three ounces of very strong, hot decoction of coffee, and during the day the food must be liquid, taken in small quantities, and often. If this fails, try in conjunction five drops of tincture of iodine in a teaspoonful of cold water every two hours—highly lauded by Dr. Churchill, I think, at least if my memory serves me; so Dr. J. G. Wilson, of this city, told me when recommending its use in a case which resisted every other mode of treatment, but which was cured at once by a few doses.

Should everything fail to give relief, and all food taken be instantly rejected, what can be done? Nutrient enemata, very concentrated and frequently used, afford the only chance of quieting the irritable stomach. Such a plan may be used for many weeks, and in my hands has never failed to soothe the irritable stomach and continual nausea. After a period of from three or four days to as many weeks, food may again be very cautiously partaken of, and will in the majority of cases be retained without any troublesome symptoms. I have followed the latter plan of treatment in three or four extreme cases, and had no cause to regret doing so, the patients and children not having suffered any injury; while I am positive one patient at least would have died had she not been kept up by the enemata alone for sixteen days, nothing but a small quantity of ice having been swallowed during that time.—*Lancet, Dec.* 17, 1864, *p.* 707.

91.—ON SUPPORTING THE PERINEUM AS A PREVENTIVE OF LACERATION AT CHILDBIRTH.

By Geo. K. H. Paterson, Esq., Balbeggie, Perthshire.

In my own practice I have invariably acted on the principle of giving support immediately before and at childbirth, during more than twenty years, and have never regretted doing so, but rather am fully convinced that had I not given such support, I should have had a good number of lacerated perineums to witness and attend to. How the perineum can undergo less risk of being torn by withholding than by giving support to it previous to and during the expulsion of the head of the infant, I must confess I am as yet unable to understand. It must be allowed that laceration of the perineum at childbirth is equally to be expected amongst the wives of the Scotch peasantry as anywhere else; and having attended many of these during past years, of stout make, and of ruddy colour, the infants being generally large, it has always appeared to me that due and proper support prevented laceration of the perineum, even when it seemed most likely to take place; especially where the patient had been left unaided, and when the perineum had been long stretched, owing to the unyielding and unadapted state of the vaginal appendages and head of the fœtus, while the expulsive pains were violent and recurred frequently; and it is oftenest in such instances, I presume, that the obstetrical practitioner may look for a lesion of the perineum as likeliest to occur, unless prevented by means of support applied with the bare flat of his right hand when the fœtal head is resting *firmly* on the perineum, and coming forth under the continued expulsive uterine pains. When the propriety of affording support at this juncture of the case is convincingly felt by the medical attendant, his hand should be so applied as to compress and include within it the vertex of the fœtal head, and also to pilot it if anything a little forwards; and while it is emerging quickly from the vulva, he should keep the hand steadily in apposition, and at the same time depress backwards gently the perineum over the face and especially the chin of the infant as it is being expelled into the world. There is often a difficulty and danger to the perineum in certain cases, and far more so in my mind when unaided, in getting it safely clear of the chin when towards the sacrum; and no better method have I found than the above so as to prevent laceration from occurring. In the married and unmarried country females and out-of-door workers, consisting of those of stout make, and also of little stature, who bear children very frequently, this mode of giving support is undeniably of great advantage; but I make no exception, as I apply it in all my cases of head presentation. And I do think in regard to supporting the perineum, that its advantages ought to be more generally appreciated, as in the rapid and violent expulsive pains, accompanied with large and hard or ossified head and rigid perineum, vagina, and vulva, I am confident by keeping with the hand well applied the head of the fœtus as often as possible back a little, instead of hastily delivering when pressing strongly on the perineum, that I have saved several lying-in women from inevitable laceration; and such a procedure it is reasonable to suppose allows the perineum, &c., to expand more gradually, and renders the risk referred to less to be apprehended in consequence, even when most likely to take place. But truly I have never found it to occur when such preventive practice has been followed.

Although it is said that no labours are so easy, or so soon recovered from as those of the wives of wandering mountaineers in barbarous countries, still their whole lives are so dissimilar to civilized life in this country, that little good seems derivable from a comparison of lying-in women under such different circumstances; and I do not think that the non-occurrence of rupture of the perineum among the former should be an argument for our withholding support to the perineum among our own patients.—*Edinburgh Medical Journal, Nov.* 1864, *p.* 419.

92.—ON THE ADVANTAGES OF A MORE EXTENDED USE OF VERSION IN CASES OF DISPROPORTION.

By Dr. Andrew Inglis.

[The increased use of turning is one of the principal means by which the employment of craniotomy has been and may be still further diminished. It is best that in all head presentations, in which the forceps will not succeed, turning should be attempted, even though there is no hope of extracting by means of it alone.]

The advantages of such a use of version seem to be the following:—1st, That, by enabling us to ascertain definitely the proportion between the size of the child and that of the passages, it may prevent recourse being had to unnecessarily severe operative procedure, which otherwise might be resorted to from ignorance of that proportion. 2nd, That in cases which, without turning, would require craniotomy, the application of forceps to the head, *after turning*, will often be sufficient to effect delivery with a better result to the mother, and frequently without injury to the child. 3rd, That, even in the cases where version does not enable us to avoid craniotomy, its previous employment seems to render that operation easier of performance without adding in any degree to the risk.

There can be no doubt with regard to the first advantage, viz., its usefulness as a means of diagnosis, enabling us to avoid needless severity of operation, because it is manifestly the only method by which the *relative* size of the child to the passages can be determined; and, in the absence of such evidence, mistakes, proving serious both to mother and child, have often occurred even in good hands. I may mention, in illustration of this, that in cases of turning, where nothing else than craniotomy could have otherwise been performed, the child has often been unexpectedly extracted alive. Moreover, there have been cases in which craniotomy has been performed, although the deformity was so great as to detain the breech of the child, and the result has been, as far as I have been able to discover, uniformly fatal. Some cases also have been recorded where, after craniotomy had failed, the Cæsarean operation had eventually to be performed. Now, in these cases, had turning been attempted, as a means of diagnosis, it would have been ascertained at once whether the breech could pass or not, and thus a great amount of time would have been saved, and unnecessary injury avoided, and the propriety of Cæsarean section at once discovered.

The second benefit gained I have stated to be, that the use of forceps with turning as a preliminary, may often enable us with advantage to avoid craniotomy. Such a use of forceps has never found its way into general practice, although there has been good authority for it. Smellie was the first to propose it, and when he did deliver by this method, his results were highly satisfactory. Dr. Granville of London has followed his example, and found the forceps so useful in such circumstances, that he has twice recorded a series of successul cases. I have also met with others who, from their success in this mode of operating, strongly advocate it.

The third advantage which I have stated to be derived from turning, is that, should the forceps fail to procure delivery, or appear inadmissible, the operation of craniotomy will be more easily performed *after turning*, and with probably less risk than if the head were presenting. I have been led to form this opinion principally from the good recoveries cases are generally reported to have made where craniotomy has been performed after extraction of the body, and also from the invariable shortness of the description given of the operation. Dr. Ramsbotham, in speaking of craniotomy under such circumstances, says, that perforation alone is generally sufficient; and in only a very few cases have I heard of the head requiring to be further broken up. It has often happened that, after turning, the extraction of the head entire has been found impossible; but the description of the operation required to complete delivery has, in every report I have been able to find, been very concise, leading to the inference that no great difficulty was experienced. As an instance, I may refer to the first volume of the Transactions of the London

Obstetrical Society, where the description of a case runs as follows:—"The occiput was perforated, the brain washed out, the cranial bones crushed inwards, and the head extracted. The placenta followed, the bleeding ceased, and the patient recovered without an ill symptom." The late Dr. Ramsbotham met with a number of cases of the kind, and yet in recording them, he gives us no reason to suppose the operation to have been rendered either more difficult or more dangerous by the malposition of the head. Smellie also seems to have found it an easy operation, if we can judge from the contrast between his lengthy details of the ordinary form of craniotomy, and his concise and simple accounts of the use he made of his crotchet after the birth of the body.

On the other hand, when craniotomy is done on the head while presenting, great difficulty is often met with, and many hours have often been spent on the operation, even where the pelvis was so wide as easily to permit the subsequent passage of the body. In addition, I would point out the fact, that cases have been related where craniotomy alone having failed to effect delivery, turning has succeeded in procuring extraction with such ease that we must conclude that, though the head had not been sufficiently broken up to permit its propulsion through the pelvis, it had yet been broken up much more than was required to allow its being drawn through after the body.

From all this I infer that, instead of adding to the danger of the operation of craniotomy, turning must diminish it, by enabling us to dispense with an amount of breaking up of the head which, *per se*, is much more likely to do harm than the "operation" of turning. There are many who, from their experience of version under all circumstances, assert that there is at least very little danger to be apprehended from the operation when properly performed, even without chloroform to quiet the uterus. Dr. Ramsbotham states, that he never saw any bad results from even the most difficult cases of turning; and his employment of the operation has been considerable. I may also refer to Dr. Figg's well known views and experience. Moreover, Mr. B. Hicks has improved greatly on the old method of operating, which had already been considered highly satisfactory. I have turned frequently for malpresentation, and find that, where the pelvis was normal, the mother has recovered much better than after an ordinary presentation. In some, where the pelvis was so contracted as to prevent their having a live child without turning, the results have been quite as good. Also, in the few cases I have had where the deformity was such as to require traction so great, that one or two of the bones of the pelvis and trunk of the child gave way, the recovery has been invariably as rapid as after an ordinary labour. In such cases I have never hesitated to use a great deal of force in drawing the body through, with the full confidence that, even if it came to the worst, the head would be more easily and safely extracted in that position than if acted on when presenting in the usual manner. The reason so much less fracture is required after turning than before, seems to be, that, as the occiput is the greatest obstruction, and requires to be fractured as near the neck as possible, when craniotomy is performed on the head presenting, one or both parietal bones, the greater part of the occipital, and often the frontal, have to be cut away to allow of that point being reached; whereas, after turning, it is the nearest point, and perforation alone should almost always be sufficient to allow the bones to fall in as required. If, on the other hand, more than simple perforation be necessary, the hold afforded by the body should be a great assistance. Besides, it must be recognised as of the utmost importance that, in delivering the head, the force applied is *traction* at the centre of the resisting part, while in cases where the head presents, it is *propulsion* by a hook inserted behind the head, involving either the employment of a much greater amount of force or a much greater extent of fracture. An idea of the comparative merits of the two operations may be obtained by the following illustration. If we take a hollow india-rubber ball and try to *press* it through a round aperture a little smaller than its diameter, we shall find that it will flatten out and thus *increase* in diameter; and, if we make an incision in the part protruding, it will flatten *out still farther*; but if, on the other hand, we attempt to *pull* it through by *means of a string* attached to it, the ball will elongate, and so *diminish* in di-

ameter; and, if incised, will collapse, even although the aperture be a small one, without farther limit than is imposed by the substance of the ball.

I have now given reasons for holding that preliminary turning lessens the danger of craniotomy by rendering the operation less difficult, prolonged, and extensive. I have further to state, that it lessens the danger by enabling us to perform the operation at a much more favourable time. In all cases where the forceps are unsuitable, or have failed to procure delivery, if turning is had recourse to, it will be performed at once, and thus craniotomy, if necessary, will be demonstrated to be so, and consequently resolved on and performed at a much earlier, and to that extent more favourable period of the labour than would be the case if the head were allowed to remain presenting.

Besides the danger resulting from exhaustion, &c., caused by the delay both previous to, and in the performance of craniotomy on the presenting head, there is, it must be noted, some risk of rupture of the uterus before delivery by the operation in question. If the uterus ruptures during labour, the lesion always takes place after the os is pretty well dilated; and a very large proportion of the cases of rupture have met with the accident either while the operator was waiting till the labour was sufficiently advanced for the performance of craniotomy, or during the operation. If, on the other hand, turning is applied to such cases, it will almost certainly be performed before the labour has advanced so far as to render a rupture of the uterus at all likely; and the body being once delivered there is no risk of such an accident during the extraction of the head, however long the operator may delay in breaking it up.

From these considerations, I would therefore strongly recommend turning to be attempted in all cases of disproportion too severe to permit of delivery by the forceps, unless the necessity of Cæsarean section be plainly indicated, with the full assurance that, if further instrumental procedure be required, instead of complicating, the turning will be found to render extraction much more easy.

Moreover, I think, that those who admit that in cases of difficulty the head can be more easily extracted by version, on account of the advantage gained by the improved position, combined with the increased power of adaptation consequent on that position, must concede that, if the power of adaptation be increased by subsequent crushing, the assistance derived from version must also in these cases be increased.

Finally, even though great improvements in the manner of breaking up the head should be introduced, still the use of version as a preliminary will continue to be a great assistance, and its advantages as a means of determining the cases in which the necessity for craniotomy exists must also continue to be of the utmost importance.—*Edinburgh Medical Journal*, *Dec.* 1864, *p.* 503.

98.—ON THE APPLICATION OF EXTREME COLD AS AN ANODYNE IN THE PAIN ATTENDANT ON PARTURITION.

By Dr. J. Mortimer Granville, Bristol.

[The author has formed the following conclusions regarding the nature, duration, and intensity of the pain accompanying labour.]

1st. The actual pain (as distinguished from the sensation of forcing) experienced by the parturient woman bears no definite proportion to the force and efficiency of what (for want of a better name) we term "the pains" of her labour. 2nd. The sensation of pain is not invariably synchronous with the occurrence of uterine contraction, the effects of the latter being perceptible to the accoucheur not unfrequently, during examination, before his patient experiences the former.

From these circumstances, taken in connection with others well known to careful observers, it may be inferred that the pain attendant on labour is more directly related to the effects of uterine contraction (perhaps mechanical force operating on nervous tissue) than to the specific muscular act itself.

The reference of the pain to some region more or less remote from the con-

tracting uterus or the dilating external passages (in which the seat of pain might have been supposed to be located) appears to support the last inference, whilst it clearly points to the conclusion that the pain attendant on labour is neuralgic in its character.

Acting on this theory, the author has tried the effect of extreme cold as a topical anodyne, and the result has been so constant that, after repeated experiment, he feels confidence in bringing this new method of seeking to alleviate the pains of labour before the society, and suggesting its adoption by the profession. The method is applicable in all cases, independent of the condition of the patient, cold being already in use as a powerful means of exciting the uterus to contraction in cases of extreme exhaustion. It is perfectly free from the dangers so inseparable from the most cautious use of anæsthetics. It is perfectly manageable, and may be safely confided to a nurse; and its *rationale* is in strict conformity with the well-known principles of physiology. It is not intended to remove, but to alleviate, the pain attendant on parturition. It will be found that the power of the uterine efforts is generally increased, without any corresponding increase in the attendant suffering, the patient being not unfrequently under the impression that the pain has ceased when the presentation continues to evidence the continuance of the expulsive force. This has been very remarkable in some of the cases, the pains appearing to be shortened when examination has proved them to be prolonged. The actual pain is sensibly diminished, the patients expressing themselves variously, but to the general effect that they are relieved of half their suffering.

The method of the application is of the greatest importance. The cold must be extreme, or it is of little, if any, service, and it must be persistent.

An apparatus was exhibited, consisting of a flat tin box, which is filled with a freezing mixture and applied to the seat of the pain.—*Obstetrical Transactions, Vol.* vi. 1865, *p.* 105.

94.—DESCRIPTION OF THE "GUIDE-HOOK," A NEW OBSTETRIC INSTRUMENT.

By Samuel Newham, Esq., Bury St. Edmund's.

This invention is simply a blunt-hook, with the addition of a prow or guide.

The addition of the guide to the hook is for the purpose of favouring the adjustment of the hook into many positions in which a hook is required. It is not intended by this instrument, however, to supersede the use of the ordinary blunt-hook, for the blunt-hook may be used under circumstances in which it might not be safe to use the guide-hook. It is intended to supplement many purposes which the blunt-hook is not constructed to carry out.

The purposes for which the guide or prow will be found useful are:

1st. The insertion of the hook into any opening, whether a natural one, or one effected by the previous use of any other instrument.

After perforation of the head, not only is the hook with the addition of the guide more easily introduced, but the larger guide itself may be used as a means to dilate the opening and break up the bones, and thus freely to discharge the contents of the skull, obviating in this way a prolonged use of the perforator.

In comparatively undilated conditions of the os—in very contracted pelvis—or in any position of the fœtus where it is necessary to introduce the hand far within the mother and there is probability of the bones overlapping after perforation, or of the skin overlaying the artificial opening, a slight use of the perforator and free use of the guide-hook is much less liable to the production of accidents to the uterus or vagina than the sole use of the perforator and crotchet, both which latter instruments, having cutting edges, are liable to prove hurtful to both patient and operator. The guide-hook, having no cutting edges, is free from this liability.

2nd. After opening the head at or about the vertex in ordinary presentations, the guide-hook may be made use of to extract by the insertion of it into the foramen magnum, and also by a little force and rotation, by its insertion

into any of the bones of the base of the skull. For this purpose the smaller end is most suitable, and by a repetition of this boring and breaking process the base of the skull may be so disintegrated that extraction becomes a matter of ease. By thus piercing, seizing hold of the basilar portions of the skull and drawing down, the contents of the skull are much more easily ejected than by seizing the calvaria by the crotchet.

In cases where turning has been accomplished, after first perforating any of the basilar portions of the skull, the guide-hook is of great value in breaking up the arch and strength of the bones. In many instances it is hardly necessary to use the perforator. By first using the smaller, and then the larger end, almost any part may be entered and broken up.

3rd. In cases where it is required to perforate at the roof of the mouth, or in thrusting through the orbits, the guide-hook is of valuable assistance—preceding the use of the larger end by first inserting the smaller.

4th. In cases of a symmetrical pelvis the guide-hook is of great service in turning the direction of the head into a more convenient axis—that is, in directing the occiput to the wider side—the guide, for this purpose, being thrust into the base of the skull, and then rotated.

5th. In perforating or thrusting into the thorax, whether before or after the removal of the arm, the guide-hook is also of value.

6th. In cases of evisceration through artificial openings, or by insertion of it into the anus, the guide-hook much facilitates every proceeding.

Of course there are circumstances in which it is necessary to use a hook in which the guide might be an encumbrance, or even an obstruction, but the care which an adept operator would always exercise in the use of any instrument would reduce these circumstances to very few in number. In many instances a hand or fingers may be used in advance of the guide, and no fear of accident need be dreaded.

I feel great satisfaction myself in possessing so useful an addition to my *armamentum obstetricum*, and confidently advise every accoucheur to add the guide-hook to his collection of instruments.

P. S.—I have had also constructed a handle or key to fix on the smaller end of the guide-hook, to enable the operator to act with force when the larger end is inserted in any part which is very solid or difficult to move.—*Obstetrical Transactions, Vol.* vi. 1865, *p.* 7.

95.—A RARE CASE IN MIDWIFERY PRACTICE.

By George Rigden, Esq., Canterbury.

On the 2nd of August of the present year, Mrs. G., aged about twenty-five

years, of a leucophlegmatic temperament, and residing in this city, was taken in labour with her second child at about five o'clock in the evening. The pains were reported to have been frequent, but not very strong until about eight o'clock, when my attendance was requested. Upon examination, the forehead was found presenting, and partly entering the brim of the pelvis, with the os uteri moderately dilated and dilatable. The patient was extremely anxious in consequence of having experienced a very protracted labour at her first confinement; but there appeared no reason to doubt but that upon this occasion it would proceed more favourably. The pains continued frequent and strong, but with scarcely any effect upon the progress of the labour, until half-past nine o'clock, when they almost suddenly ceased; and not returning by half-past one in the morning, the forceps were applied, and the labour quickly terminated by the birth of a full-aged living child. A broad bandage was immediately applied round the lower part of the abdomen, and the right hand passed between the thighs to the region of the uterus, which was distinctly felt, and moderately contracted. After about half an hour's interval, the uterus was again felt in the same position, and in about the same state; and, as the placenta could not be reached through the vagina, the patient was directed to make a slight expulsive effort, which immediately changed the whole aspect of affairs. The uterus passed away from the hand above the pubes, and was found to have descended through the vagina, and to be completely inverted between the thighs of the mother, with the placenta still attached to the greater part of the larger and more dependent part. No traction had been made at the cord, and the inversion seemed due entirely to the moderate expulsive effort of the mother. The placenta was immediately and quickly with the ends of the fingers peeled off from the uterus, and the latter at once, and without the slightest difficulty, replaced in its natural position. After retaining the hand in the vagina for a few minutes, the uterus was felt contracting, and was also distinctly perceptible above the pubes. The patient was directed to remain in the same position for several hours, and not on any account to make an expulsive effort.

It is satisfactory to record that the uterus remained in its natural position, and that the patient made an excellent recovery without any other bad symptom. She was desired to be particularly careful of herself for several weeks. It is now more than a month since the confinement, and she does not experience any bearing-down or other unpleasant effect from what cannot but be considered as having been a very dangerous complication.—*Lancet*, *Dec.* 17, 1864, *p.* 689.

96.—AN ENQUIRY INTO THE BEST MODE OF DELIVERING THE FŒTAL HEAD AFTER PERFORATION.

By Dr. J. Braxton Hicks.

In introducing the subject of this paper, the author said that, notwithstanding the employment of premature labour and version, cases would occur in which it was either necessary or desirable to perforate. He pointed out that the subject had of late years not received the attention it deserved. He alluded to the disputes which arose upon Dr. Osborn's case of E. Sherwood, when that physician asserted he could draw a child's head through a brim having an inch and a-half antero-posterior diameter, by tilting the base of the skull sideways, and concluded that Cæsarean section might be done away with. The disputes which followed were so acrimonious that the valuable points elicited by Drs. Hull, Hamilton, and Burns were, to a certain extent, lost sight of, at least as far as they were calculated to give any rule in practice. Dr. Burns in particular deduced from his experiments that the calvarium of the fœtal head being removed, the base of the skull could be drawn down easier face foremost than in any other direction. With this the author's experiments entirely agreed. And he pointed out further the advantage of the chin pointing anteriorly during the descent. He further instituted a comparison between the opposing diameters when the face is made to

present, and the other modes of drawing down the base of the skull. He then proceeded to answer the inquiry: If in cases of extreme lessening of the antero-posterior diameter, it is best to cause the face to present; and if, after simple perforation, it is best to continue vertex presentation, at what degree of reduction of the size of the head do the two presentations cause equal obstruction? This he answered by the results of experiments, which might be thus concisely stated. That, as is acknowledged by all, vertex presentation in natural labour is the best; and that after perforation and evacuation of the brain up to the extent of one-fourth, this rule holds good; yet if the evacuation of the brain and collapse of the calvaria by this means, or by more or less fracturing the bones, be carried to a greater degree, we find that the facial presentation affords the easiest mode of delivery, provided that the mentobregmatic falls beneath the bizygomatic diameter. And, further, that if we remove the whole calvarium, leaving merely the base, and then induce face presentation, taking care that the chin, as it descends, points anteriorly, we diminish to the smallest possible amount, short of wholly breaking it up, the opposition of the head, leaving only from one to one and a-half inch in depth to oppose the conjugate diameter of the brim, and from three to four inches at the outside to oppose the transverse. The author, as practical deductions from these facts, recommended that in cases where simple perforation failed, to allow the descent of the head in cases of obstruction—say above three inches antero-posterior diameter—to break up purposely and carefully the bones of the calvarium, and remove at least a portion, preserving the scalp as protection to the edges, and then to induce face presentation. That when the diameter was under three inches, then to remove all the calvarium, and then to induce face presentation, taking care to bring the chin forwards, if not already in that direction. Dr. Hicks then pointed out the facility of doing this with a small blunt hook, which could be readily, and without chance of injury, passed up to the orbit. The chin he had found, had a tendency to point anteriorly upon being drawn down. He then entered upon some useful details, and compared this mode of craniotomy with the cephalotribe. He remarked that by this means, in deciding upon whether craniotomy or Cæsarean section should be performed, the head was not so much to be considered as the size of the body in cases of brim obstruction. The paper was illustrated by eight cases of craniotomy, six of which were required for contraction of the conjugate, and two for obstructions in cavity. In all the induction of face presentation was attended by instant and complete passage through the obstacle. In some of the cases the shoulder and pelvis of fœtus gave more difficulty than the head.—*Medical Times and Gazette, Jan.* 28, 1865, *p.* 103.

97.—DESCRIPTION OF AN APPARATUS FOR THE PERFORMANCE OF TRANSFUSION.

By Dr. Graily Hewitt, Physician to the British Lying-in Hospital; Assistant-Physician-Accoucheur and Lecturer on Midwifery at St. Mary's Hospital.

The apparatus is constructed to carry out the operation of transfusion in the following manner:

The blood to be transfused is received into a glass syringe, and by means of this thrown into the vein of the patient requiring the blood. The method adopted allows of the almost instantaneous transference of the blood from one individual to another, and the exposure of the blood, which, as is well known, tends to produce rapid coagulation, is reduced to a minimum. This is effected by making the syringe itself the vessel into which the blood is received. For this purpose the piston of the syringe is removed, and the barrel of the syringe is held in an inverted position over the orifice of the vein, in close apposition with the surface of the skin, but not pressed too firmly, otherwise the blood will not flow. The blood then flows into the syringe, and when a sufficient quantity is collected therein the syringe is suddenly removed and the piston inserted, the blood being prevented from escaping by inserting the little plug into the escape-pipe. The syringe is constructed to hold two ounces.

Prior to filling the syringe with blood the arm of the receiving patient must be prepared. For this purpose the arm is to be uncovered, the vein selected which is largest—usually the median-basilic or the median-cephalic—and a longitudinal incision, one and a half inch long, made through the skin immediately over the course of the vein. The vein itself is then to be dissected out, and completely bared for nearly one inch of its course. The vein is then to be seized with the forceps and a slight incision made in it by means of the scalpel. This incision should be made obliquely, and just enough to admit the canula with its plug. The canula, at the extremity of which the rounded plug projects, is slightly curved. The next step is to introduce this canula, with its plug, into the vein; the plug is then withdrawn, and the escape of a minute quantity of blood shows that the canula is properly inserted. The arm is then to be carefully and steadily held by an assistant, with directions to prevent the escape of the canula from the vein. The operator next procures the supply of blood in the manner previously described, being careful not to proceed to this stage of the operation until everything is perfectly ready for the injection of the blood. Experience has shown that the operation will most frequently break down from want of attention to this rule. The actual injection of the blood is performed as follows:—Taking the syringe with its contents to the bedside as rapidly as possible, the operator removes the little plug from the canula, and adjusts the syringe to the latter. The adjustment can, owing to the construction of the apparatus, be effected instantaneously, and by gently depressing the piston the blood flows into the receiving vein. The quantity thrown in at once should not exceed about one ounce or one ounce and a-half, and the injection of this quantity should occupy about one minute. It may be necessary to throw in two or more such quantities, according to circumstances. If more than one syringe-full is required, the second or following supplies must be procured and injected in a manner similar to the first.

The glass syringe, with its adjuncts, admits of being very rapidly and easily cleansed. The "Transfusion Case," made by Messrs. Savigny, of St. James's Street, contains, together with the syringe, two canulas with plugs, a small pair of dissecting forceps, a sharp-cutting scalpel, and a common lancet. The bulk of the whole is very inconsiderable, and the case, containing, as it does, everything possibly required for the performance of the operation, can be carried in the ordinary "obstetric bag," being thus always at hand in case of emergency.

For the performance of the operation it will be found necessary to have the assistance of two individuals—one to hold the arm of the recipient, the other to manage the arm of the person supplying the blood. The incision into the vein, in order to procure blood, should be a tolerably large one, to ensure a rapid flow of blood into the syringe. It is not necessary, as was formerly supposed, to heat the syringe, or to take steps for maintaining the blood at a high temperature; the syringe should, however, be placed in a basin of clean water slightly warmed, in order to render the motion of the piston easy.

The object kept in view in the construction of this apparatus is the rapid transference of blood from one person to another. Experience has shown that, while the rapidity with which coagulation occurs varies in different persons and at different times, two or three minutes may be always calculated on before coagulation will occur. The various steps of the operation must be carefully considered before undertaking it, in order to allow of the transference being effected within this interval of time. The tendency to coagulate is materially diminished by the method here adopted of receiving the blood, whereby the exposure to air and contact with foreign bodies is reduced to a minimum.—*Obstetrical Transactions, Vol.* vi. 1865, *p.* 136.

98.—ON A MEANS OF ARRESTING FLOODING AFTER LABOR AND ABORTION.

By Dr. Robert Barnes.

[The following remarks were part of a criticism of a paper on puerperal fever,

by Dr. Snow Beck. The question was as to the best means of checking the hemorrhage which so much predisposes to subsequent absorption of putilage.]

As a means of preventing the loss of blood he had found nothing of equal efficacy to the injection of a solution of perchloride of iron into the uterus after clearing out the cavity of placental remains and clots. He had used this plan for several years, and in a large number of cases after labour and abortion, and had always had reason to congratulate himself upon the result. The perchloride of iron had the further advantage of being antiseptic. He had certainly saved several women from death from flooding, and believed these and others had been rendered less liable to puerperal fever. The plan he adopted was to carry in his "obstetric bag" a saturated solution of the salt. Of this solution he used one part to eight of water. But he thought that even a weaker solution was sufficient. It instantly coagulated the blood in the mouths of the uterine vessels.—*Lancet, April* 1, 1865, *p.* 341.

99.—ON THE CONSTITUTION OF THE PREGNANT WOMAN AS A CAUSE OF PUERPERAL FEVER.

By Dr. Robert Barnes, President of the Obstetrical Society of London, &c.

The conditions of the puerperal woman predisposing to fever are constitutional and local. The constitutional conditions arise out of, or are the culminating expressions of, conditions beginning with pregnancy. The local conditions are chiefly the immediate results of labour. I propose, first, to trace rapidly the constitutional conditions of the pregnant woman.

Pregnancy often induces a profound alteration in the quality of the blood, and this alteration is necessarily attended by modifications of nutrition and innervation. This primary fact has always been recognised. Hippocrates and Galen saw in the blood of pregnancy such impoverishment as to lead them to regard it as approaching to a pathological condition. Mauriceau called pregnancy a disease of nine months. Sauvages, in his Nosography, classifies pregnancy amongst the cachexies. Boerhaave said, "Fœmina plurimis afficitur malis ex solâ graviditate oriundis." Baillie looked on pregnancy as "a state analogous to inflammation;" the decidua he likened to false membrane, and the liquor amnii to effusion of serum. The Italians of the present day bleed pregnant women, simply because they are pregnant.

The changes wrought in the blood in consequence of the parasitic maintenance of the embryo are partly the result of organic or chemical operations, and partly of chemical pressure. It is probable that the first, or organic, operations act very early in pregnancy. At least, I have seen convulsions with albuminuria as early as the fifth month, before the uterus was large enough to exert any material pressure. The first blood changes, then, take place under the influence of the nutrition and development of the embryo. The circulating fluid, the circulatory apparatus, and the excretory organs, have double work to do. They must suffice for two organisms. The blood must nourish both; it must purify itself of the degraded materials resulting from the molecular changes going on in both. For two reasons the rapidity of the circulation is increased. First, the quality of the blood being lowered, it must be sent round more quickly in order to compensate for its lesser value. Secondly, there is the increased demand; the more active *cis à fronte* caused by the new organism. There is a local stimulus and attraction in the growing uterus, causing enormous enlargement of the vessels more immediately supplying this organ, analogous to that which John Hunter observed to attend the growth of the antlers of the stag. There is every reason to suppose that so powerful a local action, even physiological, cannot go on without affecting the chemical properties as well as the volume and circulation of the blood. The pulse is quickened. The heart has to labour harder to maintain this increased rate of circulation. What is the consequence of increased functional activity in a muscle? Hypertrophy. That there is a normal, a physiological hypertrophy of the heart in pregnancy analogous to the physiological hypertrophy of the gravid womb, has

been demonstrated by Larcher (1859), Guillot, and others. As pregnancy advances, a third cause of hypertrophy of the heart comes into play. This lies in the increased mechanical hindrances to circulation from the diminished capacity of the chest, and the local impediments in the abdomen and pelvis. Associated with this hypertrophy of the heart is the exophthalmos, and enlargement of the thyroid gland, first described by Parry (1825), by Graves (1843), since by Stokes, Basedow, Beau, Natalis Guillot, Laycock, and others, and of which I have myself seen several marked examples. The peculiar condition of the blood is further marked by the tendency to osteophytic deposits in the cranial bones, and on the decidual surface of the placenta.

Thus we have two new conditions in the pregnant woman: degradation of the blood, and increased size and activity in the heart. These conditions influence every part of the system. Let us review the alterations in the blood and some of their consequences. Andral and Gavarret found that from the first to the sixth month the fibrin was below the physiological mean, and during the last three months above the mean; at the same time there is a diminution of globules. Becquerel and Rodier say the density both of the defibrinated blood and of the serum is diminished; that the water, the fibrin, and the phosphorized fat are increased, while the globules and albumen are diminished. Probably if organic chemistry were more advanced there would also be found various excrementitious substances. It is the opinion of Mr. Simon that the fibrin is excrementitious. The general result is hydræmia—a condition related to chlorosis or scurvy. The watery state of the blood is revealed in various ways. Jacquemier says he found a bruit de souffle at the heart in one out of four cases of pregnancy. Dubois, however, found it in only one per cent. Caseaux attributes the abdominal souffle, usually considered placental, to alterations of the blood similar to those of chlorosis, and partly to pressure. Against this theory, however, I must set the fact that I have several times heard a distinct souffle on one side and an ordinary arterial sound on the other.

The primary and apparently physiological alterations in the blood readily pass into pathological excess, and may give rise to, or be associated with, severe diseases.

The liver may be overpowered. It may prove unequal to the eliminative strain thrown upon it. *Icterus gravidarum* may ensue.

The kidney may be unequal to its work; hyperæmia, albuminuria follow. Sugar is not uncommonly found.

I believe that the heart may become morbidly hypertrophied, and soften from fatty degeneration. Palpitations are common.

I have also observed that the lungs, having excessive decarbonising or other eliminative work, and being irritated by excrementitious matter brought to them, may become affected with bronchorrhœa, or a form of chronic bronchitis.

The veins in distant parts of the body become liable to dilatations, and thus we have local retardations of the blood, and sometimes thrombosis. Œdema is another result. This is certainly not always due to pressure, and may occur independently of albuminuria. It usually increases with the advance of pregnancy—that is, with the increase of spanæmi, and of pressure upon the lungs. Hence with the increased effort to drive the blood through the pulmonic system, there must be compensating retardation of the systemic circulation. This is relieved by œdema.

Some diseases of the circulation and respiration resulting from pregnancy have been elaborately described by Dr. de Cristoforis (Annali Universali di Medicini, 1863). Perhaps he attaches too much importance to the mechanical pressure exerted by the gravid uterus upon the principal arterial and venous trunks. His views, nevertheless, are to a great extent well founded, and are highly suggestive. Certainly the influence of mechanical pressure in aggravating the effects of altered quality of the blood cannot be overlooked. The following are some of the propositions stated by Dr. de Cristoforis:—

1. The gravid uterus presses upon the principal arterial and venous trunks. *There results* an obstacle at the point of pressure, in consequence of which

the arterial blood accumulates in the upper parts, and can only pass with difficulty to the lower parts of the body. This results in mechanical *superior arterial hyperæmia*, whilst the venous blood stagnates in the veins of the lower limbs, producing *inferior œdema*.

2. Circumstances which increase the volume of the uterus, as twins, dropsy of the amnion, augment the pressure and the results.

3. The consequences of superior arterial hyperæmia are—passive plethora, visceral congestions, which in their turn induce special morbid essences (as eclampsia), or render chronic and acute diseases more severe (as pneumonia, bronchitis, phthisis), whilst the liver becomes voluminous and fat, and the spleen soft.

4. By the continuous effort to which the left ventricle is excited, there arises hypertrophy, and this in its turn, conjoined with the superior arterial hyperæmia, dispose to pulmonary and cerebral apoplexies.

5. The impeded respiration induces defective hematosis; this impoverishment of the blood increases the disposition to œdema of the lung.

Thus we have all the disorders of the circulation, and all the alterations in the quality of the blood, traced to mechanical pressure. That this is not altogether true is proved to me from having often witnessed serious impairment of the blood long before the uterus was large enough to exert any material pressure. But in the latter months, I have certainly seen all the effects of pressure described by Dr. de Cristoforis; and there can be no doubt that the double and mutually aggravating disorder of the blood and disturbance of the circulating organs will, in many cases, induce serious constitutional affections, and predispose to puerperal fever.

Growing out of, and adding by a vicious circle of reaction to, these evils, is a disposition to physical languor and inertia. The secretions and excretions are all depraved or impeded; and the system suffers all the ills that spring from want of exercise.

Of course the summary here given of the disorders of pregnancy would form an exaggerated picture of the condition of the majority of pregnant women; but we may safely draw from it the conclusion that, in most cases, the cause of these disorders—pregnancy—existing, the effects will follow in greater or less degree.

But before labour comes, the constitutional conditions arising out of pregnancy have already developed a high susceptibility to the invasion of febrile and infectious disorders. I think I may state this proposition with confidence—namely, that the pregnant woman is far more liable to be attacked by small-pox, scarlatina, and typhoid, than the non-pregnant. It is also no uncommon thing for women received into lying-in hospitals some days, or even weeks, before labour to catch the prevalent puerperal infection. And the fact may be explained in the following manner. I am well satisfied, from repeated personal experience and observation of others, that persons in ordinary health coming within the range of zymotic poisons do frequently imbibe them. They are saved by two protective agencies. First, in their systems these poisons have previously undergone their specific fermentation. In the next place, their excreting organs, being in good working order, have the faculty of casting out the poisonous germs before they have time to do more than accomplish an imperfect stage of fermentation. But still they make their presence felt. Just like mineral poisons, they are carried to the liver, to the kidneys, to the intestines, to the skin, to the lungs. There they produce more or less irritation, and are eliminated. I am conscious of having taken on several occasions doses of choleraic, scarlatinal, varioloid, and other poisons, which have produced shivering, and then been carried out of the system by diarrhœa. The germ perishes and will not propagate. In like manner many offensive odours act. The odours imbibed by the breath are reproduced in the stools, in which they may be perceived for days afterwards. There seems, then, reason to believe that the security against renewed fermentation of a zymotic poison in the same body is not obtained simply by a previous fermentation. I am disposed to think that the capacity of the system to throw off morbid poisons is increased by being frequently called into exercise. In this manner we may account for the comparative

immunity enjoyed by medical practitioners under repeated exposure. There must, in addition, be integrity or sufficient power in the eliminative organs. Now it is precisely this eliminative power which fails in the pregnant and puerperal woman. Her excreting organs are overworked by the task of getting rid of her own and the embryo's excrementitious matters; and her blood is in consequence so degraded by its own impurities that it is incapable of dealing with the new poison received from without. Hence the foreign poison again finds a favourable medium for fermentation; and variola, scarlatina, or typhoid again run their course—even in a person who has had these diseases before. I think I am in a position to affirm that such is really the case; and that in pregnancy the susceptibility to take zymotic disease is so exalted that second and third attacks are more common, as they certainly are more fatal, than in other persons.

There are means of lessening this danger and of facilitating the discharge of these poisons. These consist mainly in generous living, so as to support the blood in the utmost efficiency; in exercise in the open air, which compels us to inhale a larger proportion of oxygen, which, coursing through the blood, and adding to the energy of our excretory functions, especially of the lungs and skin, converts and throws off the noxious elements; in strict regard to cleanliness, especially bathing, which secures the activity of the skin. I believe it to be a grievous error for a medical man to indulge in a close carriage. To use a form of expression, which is hardly a metaphor, by riding in the free air he may actually blow away the germs of disease which he has inhaled. If this be true as applied to persons in health, similar means are far more necessary to the pregnant woman. Active exercise she may be incapable of taking; but avoidance of close rooms and exposure to free air are almost always possible. The rules of hygiene necessary for all to observe who would have health are infinitely more important for the pregnant woman. She should live well; bathe once a week, to give the skin the power of compensating for the deficient activity of the internal excreting organs; take moderate exercise; secure good ventilation within doors, and be as much as possible in the open air.

No more striking idea can be formed of the abruptness and extent of the change undergone by a woman in passing from pregnancy to child-bed than by stating the following fact determined by Gassner:—

By the act of delivery the pregnant woman loses one-fifth part of her weight. This is probably a larger proportion than is lost by the patient who undergoes amputation at the thigh. It is true that things have been preparing so that this sudden fission, as we may call it, of the parent body may be effected with the least possible disturbance. The embryo has attained a stage of growth rendering it capable of independent life. But still up to the moment of separation, nearly all, if not all, its nutritive and excretory functions are performed through the mother's blood. Part of this loss of weight consists of the child, placenta, and liquor amnii. The combined weight of these may average twelve pounds. All the remaining loss must come directly from the mother's system. First there is the hemorrhage, more or less copious, which follows the expulsion of the placenta. In ordinary cases, where the uterus contracts fairly, the blood that so follows is, I believe, nearly restricted to that which was contained in the vessels of the uterus, and which is now squeezed out by the contraction of the organ. It is doubtful whether much of this uterine blood is driven back into the maternal vessels. It is superabundant, and is therefore cast out. But another, and a lage part of the gross loss is thrown out gradually; that is, within a few days. It is this which puts the integrity and powers of the system to the test. The rapidity is marvellous with which the uterus passes from being the largest muscle in the body, weighing twenty ounces or more, to a weight of nine or ten ounces. The heart, too, loses weight, and an excess of blood and other fluids, and probably of solids, has to be got rid of. Now the organized solids, such as the muscular tissue of the uterus, can only be discharged by being in the first instance converted into soluble matter, which can be taken up into the circulation, and then carried to the excreting organs. Here, then, is an amount of work thrown upon the system unknown before. All that superabundant solid

structure called into existence to build up the pregnant uterus has to undergo fatty metamorphosis, and the proceeds are rapidly absorbed into the blood. Thus the blood undergoes a new and extensive deterioration, to remedy which the utmost energies of the excreting organs are exerted. Hence we have commonly a greatly increased activity of the skin, manifested by perspirations and a peculiar odour, which shows that the skin discharges not water only, but effete organic matter; the kidneys also act freely, and not uncommonly an abundance of urate of ammonia and urea shows that these organs are discharging more than the usual excreta. The quantity of carbon and of watery vapour exhaled by the lungs is also greatly increased; and here again the peculiar puerperal odour, the "gravis odor puerperii," reveals the fact that the lungs too are called upon to assist in throwing out the useless organic matter.

In addition to this active work of elimination, there is a new work of formation. The secretion of milk is taking place. It is probable that in the performance of these two functions there is some amount of reciprocal aid. Some of the materials resulting from the fatty conversion of the uterus may serve in the formation of milk. Certainly involution goes on best under the influence of lactation. If the woman do not suckle, the activity of absorption is impaired; the uterus remains bulky; and I am persuaded that it is to the neglect of this natural duty that many mothers may trace the origin of those uterine engorgements and hypertrophies which are developed many months afterwards. Indeed, Gassner appears to have reduced this proposition to actual proof. He found the loss of body weight was less in non-suckling than in suckling women.

Dr. M'Clintock, who has made valuable observations on the pulse in childbed, says that in a large proportion of cases of normal convalescence from parturition the pulse scarcely deviates at all from the standard of health. Where this is the case, it may be inferred that the excreting organs are doing their work fairly. Dr. F. Winckel found the temperature of lying-in women slightly higher than during gestation; and he, as well as Dr. V. Grünewalt and Dr. Traube, observed a marked and rapid rise in the temperature of the body whenever any inflammatory or febrile condition supervened. It is very rarely that things are so happily balanced in childbed that the establishment of lactation is effected without some degree of febrile movement. On the second or third day there is commonly increased frequency of the pulse and some heat of skin, sometimes even a slight rigor, attending the rapid filling and tension—a state often verging on inflammation—of the breast. This may be held to indicate a disturbance of the balance between the absorption of now superfluous matters and of their excretion. It is not, therefore, a "milk fever," as it is commonly called. Nor is it, as some German authors describe it, a "wound fever," the result of the uterine lesion. It is more strictly, I believe, a fever resulting from the accumulation of effete noxious matters in the blood, which cannot be kept down by the combined agency of excretion and lactation. This is, perhaps, *the simplest form of puerperal fever*. It might be called "*Excretory Puerperal Fever*." It is strictly autogenetic. Under favourable circumstances, lactation is fairly set up; the child stimulates and maintains the diversion to the breasts; the excreting organs, relieved by the assistance of the mammary glands, recover their efficiency; the blood is kept free from the in-pouring adulterating matters; and the fever subsides in three or four days. But let one of three things happen—that is, throw into the system any extraneous poison, as that of a zymotic fever, or the miasm arising from the herding of three or four lying-in women together, thus increasing the burden weighing upon the excretory organs; impair the efficiency of the excreting organs by bad ventilation, cold, excessive heat, poor diet, want of cleanliness; or take away from the excreting organs the assistance of the breasts.—and the seeds of fever will burst into active germination, delirium set in, involution be arrested, and you may have to deal with puerperal fever in one of its most formidable shapes.

And there is yet another source of danger. The local violence sustained by the uterus and surrounding tissues by the act of labour constitutes a serious element in the puerperal state. It is this local injury which many emi-

ment physicians abroad regard as the most potent amongst the causes of puerperal fever. The condition of the blood as resulting from the disturbed balance of absorptive, excretory, and milk-forming functions upon which I have dwelt, has attracted but little attention. Let us examine the immediate effects of labour upon the parts concerned in parturition.

The child expelled and the placenta cast, what is the condition of the internal surface of the uterus? Some—as Cruveilhier, Fergusson, Simpson, and Hesch—liken this inner surface, especially that portion to which the placenta adhered, to a surgical wound, such as that made by amputation. The muscular layer of the uterine wall, they say, is bared; large vessels are torn asunder; there is a breach to be repaired, vessels to be closed. The area first denuded by the fall of the placenta is almost instantly greatly reduced by the vigorous contraction of the uterus. This reduction in superficies may be generally stated as being from sixty to nine square inches. Still there remains a considerable lesion to repair. On the other hand, more minute observation tends to lessen the points of apparent analogy between the uterine wound and the wound made by the surgeon. Robin, Kilian, Dr. Matthews Duncan, and others, show that the muscular coat is not entirely exposed; that a preparatory formation of new mucous membrane takes place during gestation between the old decidua or mucous membrane which has merged in the placenta and the muscular coat, so that when the placenta is shed there is a new fine mucous covering ready to protect the muscular coat. But notwithstanding this view,—in the general correctness of which I concur,—it must be admitted that the detachment of the placenta and membranes does not leave a perfectly smooth and clean surface. The old decidua adheres in shreds; clots of fibrin and blood project into the cavity, and here and there, as I have often ascertained by microscopical examination of aborted ova, of mature placenta, and of the uterus itself, the process of avulsion is so violent that not only decidua, but even muscular fibres, are torn away. We must, then, admit that there is a foundation for the comparison of a woman recently delivered to a patient who has undergone a surgical operation. Still I believe a too exclusive importance has been attached to the condition of the inner surface of the uterus. Other structures have undergone changes of considerable moment. I have never examined the body of a woman who has died within a few days after labour without seeing striking marks of the violence which, even under ordinary circumstances, attends that process. These effects are mainly traced in the cervix uteri, the os uteri, in the cellular tissue connecting the cervix uteri with the bladder, and in the cellular tissue of the broad ligaments. The expansion of the cervix is, in most cases, effected partly by the forcible protrusion of the child's head and body. The parts are violently stretched. The tissues undergo severe bruising, and even laceration. Small vessels give way. Ecchymoses, sometimes large effusions of blood—the uterine hematocele of Dr. M'Clintock—take place in the mucuslo-fibrous tissue of the cervix. For days after labour the flaccid lips of the os uteri, and the mucous membrane of the cervix, exhibit a deep slate-blue colour, almost black. In the peri-uterine cellular tissue I have always observed the following conditions:—The tissues loose; the interspaces large; and the whole infiltrated with serum, and sometimes containing ecchymoses, and even considerable collections of blood. Under certain conditions this state may pass into pelvic cellulitis. In addition to this there commonly remains great hyperæmia, and sometimes phlebectasis and venous thrombosis. The effused serum generally is quickly absorbed; but in the process it adds to the degradation of the blood. Here, then, we have a further local injury predisposing to local inflammation and general febrile reaction.

The enumeration of the sources of blood-contamination is not yet complete. Two factors remain to be considered. First, there is the shock of labour itself. This is both physical and emotional. It must be supposed that in the majority of instances the nervous impressions attending and following labour are beneficial. But in some cases it is not so. The shock to the nervous centres may be so great as to cause universal paralysis and sudden death. The effect of lesser degrees of shock in perverting the blood is undoubted. Sudden emotion is often the starting-point of puerperal fever. In

other cases heart or arterial thrombosis is traceable to it. But a more constant result of labour is the tissue-waste attendant upon severe and protracted physical exertion. This cause of blood-adulteration has not been sufficiently appreciated. Great mental tension and long-continued exercise of nerve-force entail rapid expenditure of the chemical elements of nerve-tissue. Dr. Bence Jones has shown that abundance of phosphates are formed in the urine after great mental exertion. In like manner great muscular exertion is attended by enormous expenditure of the chemical elements of muscle. Mr. Paget and others affirm that muscular action is always followed by the discharge of urea, carbonic acid, and water. Helmholtz shows that the muscles themselves, after long-repeated contractions, are changed in chemical composition. Dubois-Reymond has shown that the muscular juice, which, so long as the muscles are in a state of quiescence, possesses a neutral or faintly alkaline reaction, becomes, after they have been violently exercised, decidedly acid. Lactic acid is thus formed, and gets into the blood. Professor Ludwig again shows that arterial blood carried through a muscle in a state of contraction is almost completely deprived of oxygen. Dr. Edward Smith also has demonstrated that during great exertion the quantity of carbonic acid exhaled by the lungs becomes five times as great as it is during a state of repose during the day, and ten times as much as in repose at night. Brown-Séquard shows that the irritability of the muscles is extinguished by exertion. Thus in overdriven cattle, animals hunted to death, soldiers killed in battle, rigidity is early and short-lived, and putrefaction soon begins. It seems to me probable that the violent action of the uterus is the normal preparation for its involution. In this supreme effort the muscular tissue is wasted, softened, and prepared for liquefaction and absorption.

There are few occasions in life in which so much nervous and muscular energy is expended by a woman as in her first labour. She must then suffer in a high degree from the rapid conversion of nerve and muscle into waste matter, and endure from this source a further strain upon the excretory organs. I conceive this to be what takes place under the violent physical exertion of labour. A large proportion of water is thrown off from the blood; hence there is inspissation. There is accumulation of carbonic acid in the blood; hence there is hurried circulation in order to make up for the imperfect action of the lungs. Aëration cannot keep pace with the progress of deterioration. Under these circumstances the conservative powers are easily overcome, and the heart circulates a fluid deprived of vital stimulating properties, and charged with effete irritating matter. The result is a condition analogous to that state of exhaustion which issues in typhus. The system trembles on the verge of fever. I have known fatal pneumonia, pleurisy, and pericarditis ensue upon excessive physical exertion.

How important, then, is it to *shorten labour!*—that is, to cut short the waste of nerve and muscle, and to prevent unnecessary adulteration of the blood. Thus physiology emphatically condemns the old rule to let the head lie four or six hours in the pelvis before we interfere to release the patient from the torture of suspense and the agony of fruitless exertion. There can be no shadow of doubt that the timely use of the forceps may often save a woman from puerperal fever. I have also some reason to believe that in some cases chloroform, by removing the dread of pain and eliminating hurtful emotion, materially lessens the evils of excessive nervous action, and may thus indirectly lessen the danger of puerperal fever. It must not, however, be supposed that chloroform confers immunity against this disease.

To sum up the connexion between puerperal fever and the local and constitutional conditions derived from pregnancy and labour:—

1. Pregnancy induces a degraded condition of blood; throws an excessive burden upon the excreting apparatus; impedes the freedom of the circulation; causes hypertrophy of the heart.

2. Labour adds to the condition left by pregnancy; shock; extensive local injury; an enormous waste of nerve and muscle, the consequence of physical exertion; greater degradation of blood from this conversion of nerve and muscle, and also from the proceeds of the involution of the uterine tissues.

3. Hence a greatly exalted stress upon the excretory organs, and a general deterioration of the solids and fluids, inducing feeble physiological action,

and therefore tendency to fall into pathological action; that is, to generate what in the strictest sense may be called Puerperal Fever.—*Lancet, March 18 and 25, 1865, pp. 279, 307.*

100.—ON THE PROPAGATION OF PUERPERAL FEVER BY ACCOUCHEURS.

M. GRISAR has recently brought before the Brussels Academy of Medicine the subject of the communication of puerperal fever through the medium of the accoucheur. Although there is nothing new in the fact, it is of importance that it should be reiterated from time to time, in order to caution those who, unwarned, may prove curses instead of blessings to patients who seek their services. In place of recalling the testimony of the most celebrated accoucheurs on this point, we may cite M. Grisar's personal experience. In December, 1842, he delivered a woman with the forceps who died of puerperal fever on the second day; and between then and the 19th of the following March, 16 out of 64 women delivered by him were attacked by puerperal fever, 11 of the number dying. As he found the disease did not prevail in the practices of other physicians, he came to the conclusion that he had been the means of communicating the contagious principle of the disease, and therefore took every possible precaution. Until the end of 1862, therefore, for twenty years, he did not meet with another case; but at the end of that year he had a fatal one; and between December 5 and January 26 following, of 9 women delivered by him 8 became the subjects of the disease, and of these 4 died. He had taken every precaution as respects ablutions, clothing, &c., but it was not until after he had suspended practice for a month that the disease ceased to appear among his patients. M. Guerin, commenting upon the above facts, observes that Chomel always called in Baudelocque to his wealthy patients, who, unattached to any maternity, he had found to be the only accoucheur of his day in whose practice puerperal fever did not appear; and more than one practitioner, renowned for his knowledge and talent, has acquired among the public a terrible reputation for the calamities which attended his presence. M. Guerin adds that he himself, as well as various other practitioners, have, during the prevalence of epidemics of puerperal fever, experienced symptoms which could only be explained by the presence of a poisonous miasm, and which may doubtless be transported, notwithstanding ablution, change of clothing, &c. Such persons suffer from general uneasiness, fœtal breath, eructations of a peculiar odour, and somewhat loose and strong smelling stools. They should observe the greatest cleanliness, freely breathe the fresh air, and repeatedly purge themselves. When the fever occurs in their practice they should make it a solemn duty to abstain for a while from attending other cases.—*Medical Times and Gazette, Dec. 10, 1864, p. 625.*

101.—ON EXCORIATED NIPPLES.

[There have been many replies in answer to a query in the 'Lancet' as to the best treatment for sore nipples. Dr. MAC CORMAC, of Belfast, writes]:—

My remedy, then, simply is the application—every time after the child has left the nipple—with a fine camel-hair pencil, of gum arabic in an impalpable powder. This powder, so applied, is perfectly bland and free from all irritative effects. It shields the nipple from atmospheric influences from the moment it is applied, and seems to secure the nipple from the excoriating effects attending the act of sucking. I shall be glad if other persons derive from it the good effects which I experienced in the case adverted to.

[Another gentleman uses the following ointment:]

Take of semivitreous oxide of lead (litharge), half a drachm; acetate of lead, ten grains; spermaceti ointment, half an ounce. Mix.

[*Dr.* EASTLAKE writes:]

The moment an abrasion or fissure is perceived around the nipple, I recom-

mend the *immediate* removal of the child from both breasts, and the application of the following simple ointment to the sore:—Honey, white wax, of each one ounce; almond oil, two ounces. Should the abrasion heal with sufficient rapidity to enable suckling to be continued *tant mieux*; if not, I believe it is far better to desist from nursing altogether, for by so doing we not only allow the sore nipple to heal, but we prevent the almost inevitable result of mammary inflammation.

[Mr. STORER, of Salisbury, recommends the following formula]:—

Pure India-rubber, cut in thin pieces, one drachm and a half; muriate of morphia, six grains; pure chloroform, six drachms. Mix by shaking the whole in a phial, and let the nipples be painted over with the application night and morning.—*Lancet, Nov.* 26, *and Dec.* 3, 1864, *pp.* 623, 652.

102.—ON FIBROUS TUMOURS OF THE UTERUS TREATED BY SURGICAL MEANS.

By L. BAKER BROWN, Esq., Senior Surgeon to the London Surgical Home.

[Two papers on this subject have previously been published by Mr. Baker Brown, (see *Retrospect*, vol. xliii, p. 242) and in which he gives an account of seven cases treated by the plan recommended by him. Of these seven, five were cured, one relieved, and one died. In the second paper he said "that the treatment only applied to intra-uterine fibrous tumours of the non-pedunculated form, growing from the inside of the uterus by a broad base." In his concluding remarks in the same paper he said,]

"1st. That intra-uterine tumours of the non-pedunculated form may be destroyed by a surgical operation, viz., by cutting into and destroying a portion of their tissue; in other words, by gouging a portion out." "2nd. That this operation may be performed successfully and safely. Out of the dozen or more cases in which I have practised it, I have lost but the one which I have narrated in the present paper—a circumstance explicable as an accident." "3rd. That the preliminary division of the os and cervix uteri, in order to reach the internal tumour, has in itself the very important advantage of nearly or quite arresting the uterine hemorrhage, which is the most serious symptom attendant on uterine tumours."

Since the reading of this paper, some hundreds of professional men have witnessed my public practice at "The London Surgical Home;" and one gentleman, Dr. Routh, has lately stated in his 'Lettsomian Lectures' that he has successfully adopted the practice. My friend Dr. Dawson, of Newcastle-on-Tyne, has done the same.

My object in bringing this subject before the Society for the third time is to confirm by fourteen cases my previous good results, and, at the same time, to show that I have modified my surgical treatment; for whereas I uniformly divided my operative treatment into two parts, viz.—1st, preliminary incision of os and cervix; and 2nd, the gouging or breaking up of the tumour; I now find, by increased experience, that the first step is frequently all sufficient; and that even when gouging is required, a less severe operation than that previously adopted is effectual; and that, consequently, I never now use "Harper's instruments," but simply a pair of blunt-pointed scissors, finding that the slightest destruction of the capsule of the tumour is enough to ensure its disintegration.

I have been much gratified to find that Dr. M'Clintock, in his admirable memoir 'On Diseases of Women,' has confirmed the truth of my proposition, "that incision of the os and cervix does arrest the hemorrhage so frequently attending these fibrous tumours," and he gives two illustrative cases in his own practice.

Mode of Operation.—Let me draw the particular attention of the Fellows of this Society to the mode of performing the different steps of the operation, as on it I lay great stress both as to efficiency and safety.

Firstly, as to incising the os and cervix. The patient having being placed in the lithotomy position, the perinæum is well depressed by an assistant, by

means of Bozeman's speculm. The anterior lip of the os uteri is then seized by a pair of vulsellum forceps, and held by the left hand of the operator, who then introduces the hysterotome, and, whilst firmly holding the forceps to the left side, cuts the os and cervix laterally to the right. Then reversing the hands, he holds the forceps towards the right side, and cuts out towards the left. The cut surfaces are then plugged with oiled lint, and the vagina also thoroughly filled with lint or cotton wool, so as to ensure most perfect pressure and exclusion of atmosphere.

When this last stage is well carried out, there is never any serious hemorrhage; but, on the contrary, an unequal or inefficient plugging is often followed by hemorrhage, which may become in itself dangerous, or may be followed by inflammation of the uterus, and even peritoneum.

Hence it will be seen that the risk of this part of the operation is materially lessened by careful attention to the above directions.

Secondly.—Where it is considered advisable to perform the second step of the operation, viz., gouging the tumour (which should not be done *sooner* than a fortnight after incision of the os and cervix), the patient is again placed in the lithotomy position; the index-finger of the left hand of the operator is inserted within the inner os, and a pair of long-handled, blunt-pointed, curved scissors are passed up to the tumour, and gently pressed through the capsule; the blades are then so opened as to break up the tumour. It is not necessary or advisable to break up much. Oiled lint is then passed into the opening thus made by the scissors, by means of a uterine sound, and the whole vagina thoroughly plugged, as previously described. These dressings should not be removed under forty-eight hours, and, when removed, the vagina should be well syringed out twice a day with a lotion made of Condy's fluid, in the proportion of two drachms of the fluid to half a pint of water.

An opiate is administered to the patient by the rectum, a bland milk diet given, the most perfect rest and quiet enforced, and the urine drawn off every six hours by the nurse.

As I have already said, increased experience has proved to me that this second operation is frequently not required, for that incision of the os and cervix not only stops the hemorrhage, but also arrests the development of the tumour; in some cases the tumour decreases, and when small it will often entirely disappear. This last fact is more particularly proved where the tumours are of recent origin.

This is of great practical importance, and should induce us to wait for the full benefits of the first before we proceed to the second operation; and, indeed, unless the tumour be sufficiently large to interfere with the functions of the bladder, bowel, or uterus, and presuming that the hemorrhage has ceased, I would never advise any further surgical interference, but I do recommend that a steady medical treatment should be pursued, especially the use of the bichloride of mercury and bromide of ammonium, having often seen the tumour go on decreasing, and even undergo the process of calcification, so well described by Rokitansky.

[Dr. ROUTH spoke in highly complimentary terms of Mr. Baker Brown's paper, and observed:]

He believed that fibroid tumours of the uterus, whether they were out of the pelvis or within it, required different treatment. But *neither* should be meddled with except vital functions were interfered with—above the pelvis, if the heart or lungs were pressed against; below the pelvis, if the defecation and micturition were interfered with; or in both cases, if the losses of blood were excessive and otherwise irremediable. But the operation of Mr. B. could not often be safely practised in supra-pelvic tumours, because, if the finger could not give proper indications, you might mistake the side on which the wall was fibroid, and cut through into the peritoneum, as had occurred in one case he knew of. The operation for such tumours, and especially if fibro-cystic, was gastrotomy, and in these cases it was best to remove veins as well as arteries, because risk of hemorrhage from a ligature on the cervix or vagina was so much less.

In regard to pelvic fibroids of the uterus, Mr. Brown had shown incontestably *that* a tumour, if cut through, would gradually absorb away. In two

cases before recorded the tumours were partially projecting out of the os, and could be seen and felt quite distinctly. Now, ocular inspection in these showed that, after complete bisection, the tumours being left *in situ*, they disappeared altogether. The practice was therefore established to be proper in other cases, although the tumours were not visible. The opening of the os, as a preliminary step, both because it checked bleeding and enabled the operator to get more readily at the tumour, was wise, and, and under the restrictions laid down by Mr. Brown, quite safe. He (Dr. Routh) invariably practised it.

[Mr. BROWN subsequently remarked :]

As to the *rationale* of the operation, he had avoided the question for fear of giving too wide a field for discussion. He thought the action was twofold. The tumours were of very low vitality, and the slightest interference with them stopped their growth. Thus, in the first place, the initiatory blood-vessels were cut through by incising the os and cervix; and, secondly, the tumour was grasped more firmly by the contraction of the uterus, which always takes place after the os is incised; and thus the tumour was confined, could not grow, and often died.—*Obstetrical Transactions, Vol.* vi. 1865, *p.* 22.

103.—CAULIFLOWER EXCRESCENCE OF THE UTERUS.

[At a meeting of the Obstetrical Society of London, a discussion took place as to the relative merit of different instruments.]

Dr. ROUTH said that, like his compeers, he, too, had used the wire-rope écraseur. Once, in using that of Dr. Hicks, he had found himself unable to remove the tumour by it, and had been obliged to have recourse to the chain écraseur to finish the operation. The same thing had happened to him in using Weiss and Co.'s improvement on Dr. Hicks's écraseur. The instrument had bent, and he could not cut off the tumour, and was again obliged to finish it by the chain écraseur. This, in private practice, was a great drawback. Some tumours were unusually hard, and required an unusually powerful instrument. It was right, however, to add that he had since, by an experiment, tested the relative powers of the double-action chain écraseur of the French and Weiss's wire-rope instruments, upon a cow's womb, previously hardened in spirit for days. Here the instrument of Weiss had the decided advantage. The part was cut through with comparative ease by this, and with the very greatest difficulty by the French instrument.

Dr. J. BRAXTON HICKS, in answer, said that he had used nothing else but the instrument at Guy's Hospital ever since he had constructed it. All of his were made by Durroch, of St. Thomas's-street, and he could say he had never failed except in one case, where the polypus was five inches across, as large as could exist in the vagina. The failures, when they occurred, were rather owing to the fault of the makers, who, in imitating the original, either overlooked the principle on which it acted or endeavoured to effect what they called improvements. The most common fault was to leave the eye of the shaft sharp-edged instead of perfectly smooth; this was why the rope snapped over heavy work. Another, which had been alluded to, was its not cutting through in large firm growths. This was owing to twisting the rope too evenly and firmly, which gave it also stiffness. If the strands were rather irregularly twisted, then it would rather cut and tear as one side was drawn down. Another point was the best mode of tempering the wire. Durroch had taken much trouble on this point, and he believed that his make could be relied upon. Dr. Hicks did not intend his instrument to compete in strength with the chain écraseur, although it was nearly as strong; but it was constructed to be used in places where the chain could not be used—at the fundus of the uterus, or on the cervix *in situ*, &c. It was an important point not to use the rope a second time over the same amount of resistance; the cost of a new one was but small.—*Obstetrical Transactions, Vol.* vi. 1865, *p.* 104.

104.—ON OVARIOTOMY AND OVARIOTOMISTS.

By Dr. CHARLES CLAY, late Senior Surgeon of St. Mary's Hospital, Manchester.

[The following brief portion from a longer paper on the subject by Dr. Clay gives the result of his operations. Dr. Clay it must be remembered bore the brunt of the contumely that was for many years levelled against the operation, unsupported and alone. He still considers his plan of a large incision the best, as by it unseen mischief is less liable to be done.]

My operations were begun on the 10th of June, 1842; and I include in the following table all up to the termination of the year 1864, being a period of twenty-two years. And I am anxious to impress on the reader this fact, that all the cases were for private practice, and therefore not so advantageous in many points of view as those from hospital practice.

From 1842 *to December*, 1864.

	Cases.	Recoveries.	Deaths.
Complete ovarian extirpation	111	77	34
Incomplete ovariotomy, cutting down on the tumours, lessening their bulk from within, and setting up the ulcerative process.	4	4	0
Exploratory incisions, discovering uterine tumours, not proceeded with.	2	2	0
Entire removal of large fibroid uteri with both ovaries through the abdominal parietes.	3	1	2
Cæsarean section done too late to save mother, but save children.	2	0	2
Total	122	84	38

Add to the above six cases of tapping and injection with strong tincture of iodine, four of which never refilled, and though occurring years ago, remain well. Two refilled, and were afterwards operated upon; both recovered, and are included in the above table. These complete my abdominal surgery for the period.

In the table just given is shown the result of the large incision in every case, except the two exploratory incisions where uterine disease was suspected, and the operation of extirpation not proceeded with. The incision was *in these two cases* smaller, but exceeded four inches, and was as difficult to treat afterwards as the cases of complete extirpation. I may also remark that these cases occurred in my early practice; and if they were now offered to me in *their then state*, I should not hesitate to extirpate both uterus and ovaries, both being excellent prospective cases. And here I may further state that I have not had the mortification of closing the abdomen over an *ovarian tumour* which I could not extirpate in consequence of extensive adhesions.

In reference to the large incision, the more I have practised it, the more I am convinced of its superiority over every other plan proposed, as the least liable to do unseen mischief, which once done can never be repaired. Such was the operation of ovariotomy when I first took it up, and the table above shows my position in the question to the end of the year 1864. During the above period, but particularly of later years, many new operators have entered the field, and added their experience. To one and all, when desired, I have answered every question as to mode of operation and treatment. I have kept nothing back that could add to their success.—*Lancet, Feb.* 25, 1865, *p.* 202.

105.—ON OVARIOTOMY.

By Dr. W. TYLER SMITH, Physician-Accoucheur to St. Mary's Hospital; Examiner in Midwifery in the University of London.

[At a meeting of the Obstetrical Society of London, Dr. Tyler Smith related *eight* additional cases of Ovariotomy, which with the twelve related in a

previous paper, make a total of twenty. His plan is to transfix the pedicle by a double silk ligature, to cut the ends of the latter off, and allow the whole to drop into the pelvis.]

Of the twenty cases of ovariotomy now and formerly related to the Society, sixteen have recovered, or 80 per cent. If any one should consider that Case 16, in which the patient died of bronchitis three weeks after the operation, ought to be omitted from the list of recoveries, the cures would still amount to 75 per cent. I have now returned the pedicle into the pelvis in ten cases, and I see no reason to discontinue the practice, but am strongly disposed to recommend it to others. As regards the state of the patients at the time of the operation, I would observe that in all the poor women the disease was so far advanced as to disable them from the performance of their ordinary duties, while in those of better circumstances the condition was such that the operation could not have been delayed longer without risk to life. All the poor patients were operated on in private lodgings.

Mr. SPENCER WELLS said that the question raised by Dr. Tyler Smith as to the mode of securing the pedicle was one of the most important which could be considered by the Society. He had followed Dr. Tyler Smith's practice in five cases. From the first no conclusion could be drawn, as the patient would probably have died had the pedicle been secured in any other manner. Two of the other four had recovered more quickly than they probably could have done under any other proceeding, although in both there was more evidence of sthenic peritonitis than was often seen where the clamp was used. In two other cases the patients died of diffuse peritonitis, and he (Mr. Wells) thought they might have done better if the clamp had been used. In one no post-mortem could be made. In the other the ligature and the tissue it enclosed were surrounded by a sort of capsule formed by two coils of small intestine, adhering together and to the pedicle by recent lymph. It was a matter of some doubt whether such a condition might lead eventually to intestinal obstruction, and be more objectionable than the adhesion to the abdominal wall which followed the clamp. His (Mr. Wells') own impression of the three modes of dealing with the pedicle was, that when the pedicle was long the clamp was the safest and the best mode; but that when the pedicle was short, it was better to cut off the ligatures short, and return the tied pedicle, than to leave the ends of the ligature hanging out through the abdominal wall.

Dr. J. BRAXTON HICKS considered the perfection of ovariotomy would be nearly gained by doing without any ligature at all. He believed it would be possible, by firmly compressing the pedicle for an inch or so by a serrated clamp or some such apparatus, to condense the tissues so firmly as to completely prevent bleeding. The pedicle could then be dropped into the pelvis. He threw out this suggestion because he had found, in examining tissues after the use of the écraseur, that it was nearly impossible to unravel them; and he thought if the width of the compressor were increased no bleeding would result.

In reply, Dr. TYLER SMITH stated that he had every reason to be satisfied with the results of the plan of dropping the pedicle into the pelvis after securing it by a ligature, and he could with confidence recommend it as being probably safer and certainly more convenient than the other plan. He should be very glad to have an instrument such as that suggested by Dr. Hicks for dividing the pedicle if it could be relied on for preventing hemorrhage.—*Obstetrical Transactions*, *Vol.* vi. 1865, *p.* 66.

106.—ON A NEW METHOD OF SECURING THE PEDICLE IN OVARIOTOMY.

By I. BAKER BROWN, Esq.

[The following article is an abstract of a paper read before the Obstetrical Society of London.]

The Author observed that hitherto there had been three distinct methods of securing the pedicle: first, by ligature, allowing the ends to hang out as

practised by Dr. Clay, of Manchester (the pioneer of ovariotomy in this country, who had steadily led us to our present successful results), and by Lane (the first surgeon in London who performed this operation) consecutively; secondly, by clamp, as first suggested by Hutchinson, and followed by many others; thirdly, by cutting off the ligature short and closing the wound, as first successfully practised by Rogers, of New York, in 1829, by Dr. Bellinger, in America, in 1835, by Dr. Siebold, of Darmstadt, in 1846, and recently by Dr. Tyler Smith. The first three gentlemen's cases were all successful, and the last-named gentleman has also had great success. Mr. Brown said that his objections to the first method had been the length of time required for the ligature to come away, which varied from nine days in his own practice to a month in that of others; to the second, the frequent severe pain caused by the dragging of the pedicle, or the pressure of the clamp itself; to the third, the unsuccessful results in his hands following its use. Having repeatedly used the actual cautery of late, employing Dr. Clay's instruments, in burning adhesions off the omentum and elsewhere, he had been gradually led to the conclusion that the actual cautery might be employed in treating the pedicle itself. Consequently, on December 28, 1864, he tried it upon a patient of Dr. Burchell, of the Kingsland-road, a lady, forty-seven years of age, who had had three children, the youngest twenty-one years since. The disease was first discovered by Dr. Burchell in August last, and so rapidly increased, as to lead Dr. Barnes and Dr. Tanner to recommend extirpation some short time before he (Mr. Brown) saw her. As the abdomen then was very large, the skin shiny, and the general health rapidly suffering, he performed the operation by Clay's large incision. There were many adhesions laterally and posteriorly, the bleeding from which was checked by the actual cautery; and finally, the pedicle, being secured by a clamp, whilst a very large multilocular mass of cysts was removed, was thoroughly seared by actual cautery and allowed to drop. The wound was then closed in the usual way, and it healed in a week, the patient being convalescent in a fortnight. Mr. Brown thought that if this plan was found by repetition to be successful, it would very materially lessen the dangers of the operation, and consequently ensure a greater number of recoveries.

Dr. Routh stated that Mr. Brown's previous and successful experiments in the removal of the omentum by a red-hot iron would prove the best reply to Dr. Tyler Smith, as to the probable conduct of the peritoneum where a pedicle was removed in the same manner.

Mr. Brown, in reply to several speakers, said that the objection urged by Dr. Tyler Smith, of the slough being injurious to the peritoneum, had been answered by the questions put by Dr. Routh and replied to by Dr. Greenhalgh: that he (Mr. Brown) had, for four years past, repeatedly used the actual cautery in burning adhesions and arresting hemorrhage, and in no one of the cases so treated had he had a death; and he thought that the objection ought not to deter others. He did not allude to the écraseur, because he thought there was not sufficient time in ovariotomy to use it safely; and he did not think it probable that it would ever come into use on that account. In answer to an objection that white heat might be detrimental, Mr. Brown said he did not go quite so far as to use white heat, but he stopped just short of it. To the objection of Dr. Parsons, that there was fear of hemorrhage in case of sickness after the use of the cautery, Mr. Brown replied that whilst it was well known that many patients had died from hemorrhage where the ligature was used, he did not think, judging from his past experience and the results of Veterinary Surgeons in spaying the sow, that there was any probability of hemorrhage where the cautery was steadily and thoroughly applied.—*Medical Times and Gazette, March* 25, 1865, *p.* 320.

107.—ON THE OPERATION OF OVARIOTOMY.

By Thomas Bryant, Esq., Assistant-Surgeon to Guy's Hospital.

[The patient should be in the half-sitting posture as it is comfortable, and still convenient to the operator. There is also less risk of the escape of the

ovarian fluid into the peritoneal cavity. In all cases it is better to commence the operation by making a short abdominal incision; but in certain examples, sooner than apply force in the removal of the tumour it is better to increase the length of the wound upwards.]

When the incision has been made, and there is no evidence that adhesions exist between the cyst and the anterior abdominal walls, it appears to be an unnecessary practice, if not one of considerable danger, for the operator to introduce his hand into the abdominal cavity with the view of learning whether there exist any peritoneal adhesions. If adhesions do exist in front they must necessarily be broken down, but I believe it to be the best practice to separate all others as they manifest their presence during the removal of the tumour, and by following this practice it will be rarely necessary for the operator to introduce his hands into the abdominal cavity at all. When adhesions exist they can frequently be broken down by the finger of the operator, introduced at the wound's margin, for as the cyst empties, these adhesions are necessarily brought forward, and when they do *not* exist the peritoneum escapes even the touch of the hands of the operator. In complicated and exceptional examples of this operation, it is clear that this practice cannot be followed, but in the more ordinary and simple cases it is most applicable. Several of the examples above recorded prove the value of this advice, for in them the peritoneum was touched only by the knife and needle.

Treatment of the pedicle.—How to fix the pedicle with safety and ease, so as to prevent hemorrhage and avoid undue irritation and traction upon the pelvic viscera, is a problem which yet remains for surgeons to decide. The clamp and the ligature are at present indispensable for the carrying out of these purpores; but they are still imperfect, and it is hoped that other means will soon be found which will be free from the objections to the above, and yet possess their advantages. The practice which Dr. Tyler Smith has so boldly introduced, of tying the pedicle and of dropping it with its shortened ligature into the pelvis, is doubtless a step in the right direction; but such treatment has been hitherto applied only to the narrow pedicle, and the treatment of the broad yet remains untouched, although there is no reason why, in these cases, the same practice might not be carried out with an equally good result.

In the examples before us I applied the clamp or ligature according to circumstances. When the pedicle was long, and there was, therefore, no traction upon the uterus, I used the clamp, for its application is easy, and a more rapid convalescence appears to follow its use than that of the ligature. When the pedicle was short I employed the ligature, for it allows the pedicle to retract, and thus has a tendency to prevent that traction upon the parts which must be considered injurious. In other cases, in which the pedicle was very broad, the ligature was used.

Sponging out the pelvis.—The propriety or impropriety of sponging out the pelvis after the operation is a point in practice which experience has not yet definitely decided. Different operators have adopted either method, and apparently with equal success.

In the cases I have related, this practice was not followed, for in no instance was there any escape of blood of any importance into the peritoneal cavity, nor of any semi-solid ovarian matter, for, under these circumstances, the practice of sponging out the pelvis appears to be a sound one, but under any other it scarcely appears to be required. In a healthy patient, that is, in one who is likely to recover after such an operation as ovariotomy, the effusion of a little blood into the peritoneal cavity cannot be regarded in a very serious light, nor can the escape of the limpid or mucoid contents of an ovarian cyst into the abdomen be looked upon with greater apprehension, for both fluids may and will, in all probability, be absorbed without difficulty; but the irritation of such a foreign body as a sponge upon exposed peritoneum and intestines must be looked upon as being more or less injurious, and, as a consequence, unless a strong necessity compels, the practice of sponging out the pelvis may be considered as being of a questionable nature.

We know from experience that blood may be extensively effused into a joint and no evil result follow, as it may into the cellular tissue of a part,

and yet be absorbed; and obstetricians will readily recall cases in which blood has been effused into the peritoneal cavity without, of necessity, any serious result. We gather also, from the above cases, the fact that the contents of an ovarian cyst may escape into the peritoneum by rupture or by a tapping without any evil influence.

In the last case I have recorded, the cyst had evidently been discharging its mucoid contents into the peritoneal cavity for many weeks without the slightest bad effect.

Treatment of the wound.—In every case but one I have employed the silver sutures; but I have *not* done so from the belief that they are less irritating than the silk, for general experience in both has told me that such is not the fact, but because they allow a more accurate adaptation of the parts than do other kinds of suture.

I have also included the peritoneum in my sutures, with the abdominal muscles.

The after-treatment.—The after-treatment of all my cases has been very simple. In nearly all I administered a good opium suppository, as ten grains of the compound soap pill, directly after the operation, and when any pain appeared the suppository was repeated; but in other examples it was administered twice a day, or only at bedtime, according to circumstances. The beneficial effect of this practice in many of the cases was most marked, and in all it was good. It is by far the most effectual way of giving opium in these ovarian operations, as it is in all others involving the intestines, pelvic viscera, or perineum, and in my hands it has almost entirely taken the place of every other mode of administration. By such a practice we appear to secure all the good effects of the administration of an opiate, without its evil, and at the same time we acquire some additional advantages, for it certainly tends to neutralize the evil influence of chloroform sickness, it gives the stomach rest, and frequently sends the patient off at once into a quiet sleep, the value of which is indisputable.

In the majority of cases this opiate suppository was the only piece of special medical practice requisite, for, in all the above cases which have recovered, convalescence went on from the beginning, and no bad symptoms made their appearance.

The patient's room was kept cool and airy, as in any other case of operation; for the first two or three days beef tea and arowroot formed the chief diet, but after that fish, meat, and wine, were carefully administered; indeed, the same principles of action were adopted in these cases as have been proved of value in other operations in general surgery; the main object being to keep up the powers of the patient, so as to enable nature to complete the cure.—*Obstetrical Transactions, Vol.* vi. *p.* 56.

108.—ON THE ADMINISTRATION OF CHLOROFORM IN OVARIOTOMY.

By Dr. PARSON, York-street, Portman-square.

Since the subject of administration of chloroform has been so recently and so ably discussed by the Committee for that purpose of the Medico-Chirurgical Society, and as the capital operation of ovariotomy is being almost daily performed, the results of my experience in the large number of cases performed by Mr. Spencer Wells at the Samaritan Hospital for Women, where I have had the subsequent care of the patients, as well as in many of Mr. Wells's private cases, may possibly be of some use to others in similar subsequent operations.

The apparatus I have used of late has been "Skinner's," consisting, as most people know, of a piece of domet flannel spread in a tent-like manner over a wire frame, the base of which is somewhat oval, so as to include the nose, mouth, and chin of the patient under this tent-like expansion. The handle is a piece of stout wire about seven inches long, bent into a loop, and *can be moved* and fixed at any convenient angle with the frame that supports *the domet*. Hence, the handle balancing the frame, the latter will generally *lie easily* wherever it is placed on the patient's face. This allows the right *hand to hold* the bottle of chloroform, enabling the left hand to feel the

patient's pulse. The bottle containing chloroform holds about two ounces, and allows the chloroform to escape drop by drop through a very small tube. This is Skinner's inhaler and dropping bottle, slightly modified for the Samaritam Hospital by Weiss and Son.

Its employment is equally simple; the tent-like inhaler is allowed to lie over the lower part of the face, and the chloroform is sprinkled drop by drop over it, and rarely does more than one minim of chloroform at a time escape from the bottle.

The advantage of this mode of giving chloroform appears to be that as only one minim of chloroform escapes at a time, and as the successive minims are distributed over the expanded domet, it ensures a much more regular dilution of the vapour with the air inspired than is usual with almost any other apparatus I have seen, except Clover's, whose methodical dilution is certainly the safest of any, and I know of no one who has presented anything as yet so decidedly accurate as his; but the incumbrance and appearance of the large inflated bag, and the intricacy of the apparatus, will always, I fear, be an obstacle to its general adoption.

Now, the domet inhaler is possibly the next nearest approach to perfection in the simplicity of its use and regular dilution of the vapour, for it follows that if the drops fall regularly, and the inspirations be about the same in frequency and depth, that a regular dilution must be ensured, and hence a nearly equable inhalation, and these I believe to be very important facts for the safety of the patient.

The time usually required to produce anæsthesia thus, varies from ten to fifteen minutes, differing in different persons; but I observe in ovariotomy that the slower its administration the less is the subsequent distress after the operation; that a state of collapse rarely occurs, and the patients generally awake tranquilly and collected soon after the operation; and there is much less vomiting and retching (in no case is solid food allowed before the operation); the preliminary stage of excitement is also very much less; and lastly, there is rarely any spasm of the larynx, which, by the bye, does sometimes occur in hysterical women. The patients generally go quietly into a profound sleep without a struggle. Now, this infrequency of vomiting and retching is of immense importance to the ovariotomist, for vomiting tends to force the small intestines out of the abdomen through the wound, unless the wound be firmly grasped by an experienced assistant, or another assistant presses his hand upon the epigastric region to prevent the descent of the diaphragm, and the operator has to wait till the vomiting is over. And also after the wound is closed by the sutures and strapping, any effort of vomiting is exceedingly painful for the patient, by stretching the abdominal wound, straining the pedicle, and disturbing the small intestines and mesentery that have been more or less irritated by the necessary manipulation in the removal of the tumour and the subsequent sponging and wiping out the cavity of the peritoneum when necessary. Hence the utmost quietude of the abdominal viscera greatly increases the patient's chance of recovery after ovariotomy.

Cutting through the skin of the abdomen is certainly the most painful part of ovariotomy; next to this appears to be the sponging out of the pelvis; and hence the greatest amount of insensibility is required at the very outset of the operation, and very little chloroform is required whilst the adhesions are being broken down at the time occupied in tapping the cyst or cysts. During the sponging out the pelvis of any fluid that may have collected there, there is almost always a spasmodic action of the legs, which I believe is due to the direct pressure of the sponges over anterior crural nerves, and hold that chloroform should not be given to such an extent as to destroy the irritability of those nerves, and that an assistant should always be placed at the patient's knees to keep them down during this part of the operation. Possibly some of this spasmodic action of the legs may be reflex, and as the patient is unconscious of it, it is much better that those motions of the legs be restrained by an assistant than have the patient prostrated by chloroform. I have also observed that this very sponging the peritoneum has a very depressing effect on the heart's action; frequently the pulse sinks not only in

force, but becomes small and slow. In one case I counted it only 30, whereas before it was beating at the rate of 68 in the minute. This woman, aged thirty-two, had a mitral murmur and a large heart, and accompanying this failing of the pulse a death-like sweat broke out over her face; and as I have several times observed this, I fancy the hypogastric plexus has been pressed upon by the sponge, and that this dangerous state is through the medium of the sympathetic; and hence in those cases it becomes the more important not to have the circulation much reduced in power when there is this depressing ordeal to go through.

The chief drawback to chloroform in ovariotomy seems to be that the enormous distension of the abdomen so presses up the diaphragm that the apex of the heart is, I often find, tilted upwards and towards the left axilla—not unfrequently found beating under the third or fourth rib—and the lungs so compressed upwards and backwards, that the power of making a deep and full inspiration is utterly impossible. I believe. the muscular power of the diaphragm to be almost for the time paralysed, comparatively speaking; and hence in almost every case of ovariotomy there is an exceedingly feeble inspiratory power; and as ovarian cases are almost all greatly reduced in muscular power and tone, the state of the respiration during the operation is also a cause of great anxiety, for I have found that when the patient's mouth is shut, and she breathes solely through the nostrils, that the inspirations are less frequent and less deep than when the month is kept open; in fact, in some very feeble old women with ovarian dropsy of some years' standing, one can scarcely see that they inspire at all; and, further, I believe this defective inspiration to be greatly embarrassing to the right side of the heart, for I find that when the inspirations are shallow, the heart beats with less force and less frequency; and in those ovarian operations more attention should be paid to the respiration than even to the pulse; and hence I have of late, when insensibility has been produced, slipped one of my fingers between the patient's teeth, and throwing her head slightly backwards and to the right side, by this means the tongue does not fall backwards, and the stertor, if it has existed, is generally immediately prevented. I find also that where there is a tendency to spasmodic inspirations, too, and closure of the rima glottidis, it is very effectual in affording a free inspiration.

When the circulation is feeble, notwithstanding that air enters the chest freely, gently rubbing the region over the apex of the heart is sure to increase its action; so, also, is rubbing the neck from the ear to the clavicle over the course of the phrenic and pneumogastric. But I find this sometimes provokes vomiting: also a cold hand on the hot skin, or a rough, cold towel, has always been sufficient to stimulate the flagging circulation in even protracted operations. Occasionally, when the tumour is very large, immediately the abdomen is emptied the circulation has lost its accustomed pressure, and a temporary fainting occurs. Now this is best avoided by immediately removing one or two of the pillows that have been propping up the patient's head and shoulders, and allowing her to lie perfectly horizontal; and this has to be done whilst the operation is going on, for before removing the mass in the abdomen the patients cannot breathe at all if they are placed horizontally—their diaphragm cannot push down the mass in the abdomen unless their shoulders are raised, and the tumour allowed to gravitate away from the thorax. Mr. Spencer Wells, in a recent case to avoid the chance of syncope, tapped the patient whilst on the table with a small trocar and canula, and emptied the largest cyst, and thus greatly equalised the pressure before any incision was made into the abdominal parietes.

The average amount of chloroform used is about three drachms. The objection that the chloroform thus administered affects the bystanders I have found to have been of very little importance, and is certainly more a theoretical than a practical objection.

Lastly, I beg to recommend operators *not* to have their patients too profoundly narcotised; for, however pleasant it may be for them, it is highly *unpleasant* for him who has the life of the patient in his more immediate *charge*, and I know of no cases so critical as those who are the subjects of *ovariotomy*.—*Medical Times and Gazette*, Feb. 10, 1865, p. 157.

109.—MULTILOCULAR OVARIAN CYST—OVARIOTOMY—RECOVERY.

By Dr. George Scott, Southampton.

[The patient was a married woman, twenty-three years of age, delicate looking, and slightly made. The enlargement had taken place very rapidly.]

When I first saw her the abdomen was already very large, more so than would be that of a woman in the last month of pregnancy. It was equally distended all over, and smooth on the surface, except towards the left hypochondrium, where a tumour of considerable fleshy hardness, of irregularly-oblong shape, and of about the size of an orange, was felt one inch below the false ribs. This swelling was quite painless—it could not be moved out of position, and the abdominal wall could be freely moved over it. She could give no clear account as to when it first became perceptible, but it had certainly been there a month or six weeks before I saw her. The remaining part of the abdomen seemed to be distended only with fluid contained in one large sac, for the wave of succussion was felt passing uninterruptedly from one side of the belly to the other. The liver was pushed up high under the ribs in the right hypochondrium. The line of demarcation between the latter organ and the tumour was not very distinct, the dull, hepatic sound passing almost imperceptibly into the dull percussion sound of the tumour. She experienced a considerable amount of pain in the abdomen now and then, but her general health did not appear to have suffered much since the appearance of the swelling. She could walk about very well, notwithstanding the weight and size of the tumour. Her appetite was very good. Her bowels were remarkably confined—sometimes she had not a motion for a week together. She mentioned that she had all her lifetime been of costive habit. Some years ago her bowels were not moved for nine days, and nevertheless she continued to eat heartily the whole time.

The treatment adopted at this time was the administration of warm water enemata every day, with occasionally a caster-oil one to clear out the bowels, and steel and other tonics internally to keep up the strength. The enemata were continued every day for some weeks, and brought away for a considerable time an astonishing quantity of hard, offensive fæces.

She was ordered to drink porter, and to have a generous diet, with plenty of meat, &c.

[In December the enlargement had very much increased, and on the 2d of January two and a quarter gallons of thick, purulent, offensive fluid were drawn off. She was again tapped in March, and the fluid was not so offensive as at the previous time, but her health was evidently breaking down, and the propriety of ovariotomy began to be seriously discussed.

She was getting thinner and weaker every day,—and the expedient of tapping was certain to be required at shorter and shorter intervals each time it was resorted to as a mere measure of temporary relief. To draw off so much fluid so frequently from the system was a most exhausting process. Moreover, she was leading a life of constant misery, suffering always more or less severe pain in the abdomen. If nothing were done beyond using palliative measures, it was evident that her life could not be prolonged many months, and those would be months of continual and severe suffering. Under these circumstances, therefore, it was pointed out to her that the operation of removing the tumour afforded her a chance of complete recovery to perfect health, although, of course, at the imminent risk of her sinking from the effects of the operation itself within a few days or even hours of its performance.

It was put to herself to decide whether she would remain as she was, using only palliative measures for the relief of urgent symptoms, with a certainty of dying a painful death within a few months at farthest, or whether she would brave the danger of ovariotomy, which afforded her a chance of complete recovery.

She made no difficulty in choosing to run the risk of the operation, observing that she would rather die than continue to suffer the misery she was then enduring. By means of the uterine sound it was ascertained that there were

no extensive adhesions of the tumour to the uterus, as the latter was pretty freely moveable all round. It was very much anteverted, being evidently pushed downwards and forwards by the mass of enlarged ovary above it.

On April 27 last, at 3 p. m., I proceeded to perform the operation. I was kindly assisted by Professor Aitken, of the Army Medical School, Mr. Mott, Mr. Norcott, Mr. J. Reeve Shorto, who administered chloroform, Mr. Ware, and several other gentlemen. When the patient was well under the influence of chloroform I made an incision in the middle line about four inches in length, beginning just below the umbilicus, through the whole thickness of the wall of the abdomen down to the outer surface of the tumour. I then plunged a very large trocar into the large cyst and drew off fully two gallons of thick, viscid, perfectly bland, purulent-looking fluid. Thus having reduced very considerably the size of the tumour, I enlarged the wound in the abdominal wall downwards to the extent of about six or seven inches altogether, and then passed my hand between the tumour and the wall of the abdomen, or rather endeavoured to do so. I found here the adhesions of the tumour so very strong that it was some time, and not without using very considerable force, before I was able to insinuate my hand between the tumour and the abdominal wall so as to commence separating the former from its adhesions. After separating the anterior part of the enlarged ovary, I proceeded to tear away the adhesions from the sides of the same, and then from the posterior part. I was prepared to have to separate a certain amount of connexions to the surrounding parts, but I certainly did not anticipate such firm bands of adhesion and such difficulty in separating them as I actually met with. The whole surface of the tumour was strongly adherent to the surrounding parts, and it was only by using really great force and tearing violently that I succeeded in separating the tumour sufficiently to draw it out of the abdominal cavity. This was, however, done eventually, and the tumour held up bodily over the opening in the abdomen. A loop of the small intestine was then seen to be adherent to the tumour close to its root. This was torn away by the hand without difficulty, and the whole mass left attached to the body only by a thin, fibrous pedicle. The clamp was now applied around the pedicle as closely as possible to the tumour, and the latter cut away just above the clamp. The pedicle was sufficiently long to allow the clamp, when drawn out as far as possible without dragging at the parts to which it was attached, to lie outside the wound of the abdomen.

The tumour was of large size. Its weight when removed was 11¼ lbs. Fully two gallons of fluid were drawn off before the removal of the more solid part, so that the weight of the whole structure when entire and in the body of the patient must have been upwards of 30 lbs. It was multilocular, being made up entirely of larger and smaller cysts, varying in size from that of a pea to that of the large sac containing fully two gallons of fluid. The contents of the cysts varied very much, being in one sac serous, in another viscid and glutinous, in another purulent-looking, in another very flocculent, and so on. Some cysts of considerable size were growing, as it were, on the outside of the wall of the very large cyst. Several of these smaller sacs, having very thin walls, were ruptured during the operation from the great force required to accomplish the separation of the surrounding adhesions. The contents of these sacks sometimes serous, sometimes purulent, sometimes glutinous, thus escaped in considerable quantity into the cavity of the peritoneum. This matter, however, being perfectly bland, was not likely to set up much irritation in the peritoneal cavity. As much as possible of it, and of the small quantity of blood necessarily effused into the peritoneum during the operation was sponged out, and the wound was then closed by means of eight hare-lip needles. A pledget of lint was put over the wound, and a broad bandage round the body so as to cover all, and the patient put to bed. The operation took altogether nearly an hour in its performance; scarcely any hemorrhage occurred, and not a single vessel required to be tied.

The patient vomited a little after recovering from the effects of chloro*form.* Some raw brandy was then given. *At bedtime she was ordered a draught containing half a drachm of sol. mur. morphiæ, and a suppository of*

two grains of extr. opii. was administered. Ice and iced drinks were allowed her freely to allay the thirst; enemata composed of about two ounces of beef-tea with half an ounce of brandy were ordered to be given every two hours during the night.

April 28, 11 a. m. Feels tolerably well; says she is suffering no pain; slept a little during the night; vomited at seven this morning, but not since. Pulse 128. Ordered to swallow small pieces of ice every now and then to relieve the thirst and vomiting. A little beef-tea was given several times during the night, but it was rejected again immediately. After the first night, therefore, for about a week after the operation, nothing but cold drink, such as iced-water, toast-and-water, orange-juice and water, &c., was given by the mouth. The patient was kept alive by the beef-tea and brandy enemata continued every two or three hours during the day and most of the night. These were generally well retained. She had a considerable amount of vomiting during the first four days after the operation, but the vomiting seemed to do her good; she always felt relieved by it; there was never much straining with it. 2 p. m. Pulse 120; a little vomiting since 11 a. m.; feels comfortable. Rep. hâc nocte suppositorium extr. opii et haustus sol. mur. morph.—*Med. Times and Gazette, Nov.* 19, 1864, *p.* 536.

110.—ON THE REMOVAL OF UTERINE POLYPI.

By Dr. J. Marion Sims, Surgeon of the Woman's Hospital, New York.

In a former paper (vide the 'Lancet,' Oct. 22nd,) I said the écraseur was a most valuable instrument for the removal of uterine polypi; that I preferred Chassaignac's to all modifications of it; that the chief objection to it was the great difficulty of applying it, and that I had overcome this by rendering the chain rigid. Where the polypus has descended into the vagina Maisonneuve's wire or Dr. Braxton Hicks's cord of wire answers very well; but where the polypus is intra-uterine, with a contracted cervix, we ordinarily fail in their application, just as we do with the chain of Chassaignac.

I have added to Chassaignac's instrument a porte-chaine, which may be described as a pair of dilating forceps with spring blades, which render the chain stiff, so that it may be passed straight into the vagina or into the cavity of the uterus, as easily as we would a sound or a sponge probang. After which the chain is expanded by the blades of the forceps or porte-chaine.

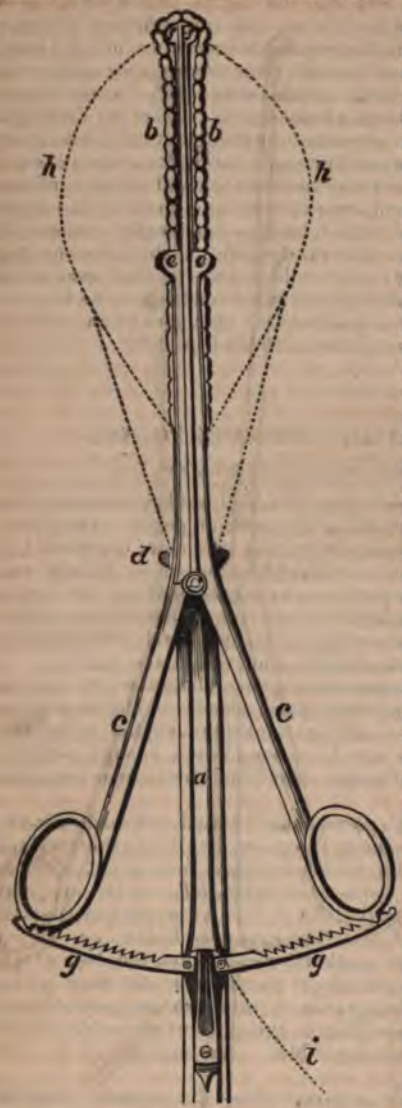
Fig. 1 represents the écraseur and porte-chaine united and ready for use. If we have to deal with an intra-uterine polypus we first dilate the cervix, if need be, with a sponge tent. On its withdrawal the instrument is to be passed into the cavity of the uterus, when the chain, *b b*, is to be expanded in the direction of the elliptical dotted lines, *h h*, by approximating the handles, *c c*, towards the dotted shaft, *a*. The expanded blades, *e e*, *f f*, are fixed at the desired extent by the rackets, *g g*. When the loop of the chain is placed securely around the pedicle of the tumour, the shaft of the instrument is pushed down on the chain just as if there was no porte-chaine. Then to get rid of this latter we have only to spring the right racket to the dotted line, *i*, when we pull back the right blade, and immediately the left comes away almost spontaneously.

Fig. 2 represents the porte-chaine separated from the écraseur, with the two blades apart to show their mechanism and the facility with which they may be detached. The distal extremities of the blades, *a a*, are of watch-spring temper, and have clefts for holding the chain.

Let me illustrate the principle of its application by a clinical observation. In February, 1863, Dr. Morpain, of Paris, invited me to operate on a patient of his who had a polypus as large as a goose's egg projecting partly from the cavity of the uterus.

The patient, on a table, was placed in the left lateral semi-prone position, and when the speculum was introduced, it elevated the perineum and posterior wall of the vagina, and brought completely into view the tumour.

There is great temptation under such circumstances to seize the projecting portion of the polypus with a strong vulsellum or tenaculum, and pull it towards the os externum. But this is not the best thing to do, because it will close up the mouth of the vagina, and obstruct both sight and manipulation; for the mouth of the vagina, even in favourable cases, would hardly be forced open more than an inch and a half from the urethra back to the perineum, and we need all this space for operating.



Here a small tenaculum was hooked into the tumour at *a*, and by it the polypus was pushed gently downwards and forwards against the anterior wall of the vagina. It was held firmly, while the stiffened chain of the écraseur was passed along the upper or posterior surface of the tumour from *a* up to the fundus uteri at *c*. This done the tenaculum was removed, and the chain of the écraseur opened out in the cavity of the uterus to a sufficient extent to allow the tumour to pass through it. This was effected by hooking the tenaculum, and raising the end of the tumour up towards the posterior wall of the vagina, at the same time that the écraseur was pressed in the opposite direction. This movement placed the middle portion of the chain parallel with the anterior face of the tumour, while its loop or distal portion still remained stationary. It was thus made to embrace the pedicle, and it only remained to remove the porte-chaine at the same moment that the shaft of the instrument was pushed down on the chain, which was tightened closely around the pedicle. The operation was then finished as easily as if the tumour had been wholly outside the body, and that, too, without the least strain or traction on the uterus or surrounding organs.

This operation was done with the assistance of Dr. Morpain, Sir Joseph Olliffe, and Dr. W. E. Johnstone. Since then (February, 1863) I have had every reason to feel satisfied with the porte-chaine, whether the polypus was in the uterus or simply in the vagina.

[Dr. Sims then relates a case in which he was consulted on account of sterility. The uterus felt somewhat enlarged and he introduced a sponge tent with the object of ascertaining the cause of the enlargement. This turned out to be a small polypus which was easily removed by the écraseur. *Before the* use of sponge tents no diagnosis could have been made.]

Now we determine with the minutest accuracy, not only the presence, but *the size*, position, relations, and attachment of all such tumours. Before the

use of sponge tents, if we suspected from rational symptoms an intra-uterine polypus, we could only wait from month to month—sometimes from year to year—for it to grow and to force its way into the vagina, before we could interfere surgically for its removal. But now we no longer doubt and procrastinate; we no longer let our patients bleed till they become bloodless and dropsical: but we ferret out at once the source of mischief, and remove it from its once secure hiding-place. This is a great advance in surgery; and no man of twenty or thirty years' experience can look back on the days of ergot and Gooch's canula, and contrast them with the present time of sponge tents and the écraseur, without a thrill of delight at the progress of our noble calling; and this we owe to British and French surgery—to the sponge tent and the écraseur—to Simpson and to Chassaignac.—*Lancet, Nov.* 12, 1864, *p.* 542.

111.—ON THE TREATMENT OF DYSMENORRHŒA.

By Dr. J. Marion Sims, late Surgeon to the Woman's Hospital, New York.

Dr. Simpson's method of incising the os and cervix uteri is followed by most operators, both in my own country and in this. He places his patient on the side, introduces the index finger of one hand into the vagina, pushes the fundus uteri up if it be anteverted, passes his uterotome into the cervix to the os internum, springs the blade, and withdraws the instrument, cutting open one side of the cervix; then reintroducing the instrument, the other side is cut in like manner, thus making a bilateral incision of the cervix large enough to allow the index finger to be passed to the os internum; and, as before stated, he then passes in to the vagina a large camel's-hair pencil saturated with a solution of the perchloride of iron.

Dr. Greenhalgh has modified Dr. Simpson's instrument by giving it two blades, which cut through both sides of the cervix at once, thus ensuring an equilateral uniformity of section that cannot always be predicated of the single-bladed instrument. His instrument is a masterpiece of ingenuity, and answers well in his practised hands. But I object to both these methods, because they are done in the dark, and too much is left to the execution of a machine instead of the judgment of the surgeon.

Suppose it were necessary to amputate an elongated uvula,—by no means

an uncommon operation,—would it be judicious to run one finger down the throat and guide by it some machine for performing the operation in the dark? Or would it be more surgical and more precise to look into the throat, seize the part with a proper appliance, and amputate it where our judgment would determine to be right and best for the individual case? There are operations that must be done by the touch alone, and in the dark; but we never select this plan if it be possible to aid the manipulatory process by the sight.

Besides the objections already urged against instruments of this class, there is another to which all instruments on the principle of cutting from above downwards are obnoxious—viz., that as the uterus is not fixed, it must necessarily glide upwards to some extent by the mere centrifugal force of the expanded blade or blades, and thus we can never feel altogether certain of the length and breadth of the cut. Whether too much or too little, it is not safely remediable afterwards.

FIG. 1.

The above engraving represents the speculum elevating the posterior wall of the vagina, the tenaculum hooked into the anterior lip of the os tincæ, which has here been divided on one side. The scissors are seen in the act of dividing the opposite side.

The operation, as I prefer to perform it, differs from Dr. Simpson's not in its aim and scope, but merely in its mechanical execution. He and his followers operate in the dark. I bring everything into plain view. They cut from within outwards; I, in the contrary direction, from the os externum upwards to the cavity of the womb. They, as a rule, do not tampon the vagina after the operation; I always do, for the double purpose of guarding against hemorrhage and ensuring an open os.

I place the patient on the left side, as for all the operations in uterine surgery. The speculum is introduced; a small tenaculum is hooked into the central portion of the anterior lip of the os tincæ; the uterus is gently pulled forwards; one blade of a pair of curved scissors is passed into the canal of the cervix till the outer one comes almost in contact with the insertion of the vagina on the side of the cervix, and the portion thus embraced is divided at one blow of the scissors. Then the opposite side is in like manner divided, and the operation is almost finished. (Fig. 1.) It only remains, while the uterus is still held in position by the tenaculum, to sponge away the blood, and pass a narrow-bladed, blunt-pointed knife (at a proper angle with its handle) and divide the small amount of tissue on each side leading from the scissor-cuts up to the very cavity of the womb.

Fig. 2 represents the knife with the blade in proper position for cutting the *left side of the* canal. To cut the right side it is necessary to turn the blade *in the opposite* direction, as shown by the dotted line. The blade may be *fixed firmly* at any angle by the screw at the end of the handle, which drives

a shaft up into little holes, as seen in Fig. 3, where the razor-shape of the blade is also shown. The operation is quickly done, and the judgment of the surgeon determines whether the peculiarities of the case demand more or less cutting. The hemorrhage is usually unimportant, but sometimes it is furious, and I have often seen it come with such a rush that the vagina would be filled before a set of sponges could be washed out. But there is nothing to be feared. Press one or two sponge probangs (Fig. 4) right into the neck of the uterus, but at the same time be sure to keep the organ firmly fixed by the tenaculum; for if the bleeding is profuse it is a very awkward and unlucky thing to let it slip out, particularly if the vagina be lax and deep. Five or six minutes will usually suffice to control the bleeding by the pressure of the

FIG. 2.

FIG. 3.

FIG. 4.

probangs. When that is done, and not till then, the dressing may be proceeded with. Two or three small pieces of cotton, large enough when moistened to fill up the gaping os, are to be thoroughly saturated with water, then squeezed as dry as possible, and afterwards wetted in a mixture of one part of Delau's neutral solution of the perchloride of iron with four or five parts of water, or in Dr. Squibb's liq. ferri persulphatis similarly diluted. Squeeze out the superfluous fluid, and place a bit of the cotton in one angle of the wound, pressing a portion of it up into the cervical canal, and holding it in place with the sponge probang. Apply another bit of cotton similarly prepared on the opposite side, and press it down with another sponge probang. If necessary, another portion of cotton may be placed centrally or over all; then, if there is no bleeding, some cotton a little moistened in simple water may be laid over the neck of the womb, to be covered with dry cotton to the extent of supporting the whole dressing neatly and comfortably in its place. The patient is put to bed, having been perhaps eight or nine minutes on the table. She eats and drinks as usual, but the recumbent posture is enjoined *for a few days.* She may pass water lying, or it may be drawn off. The

only object of the recumbent posture is to ensure the retention of the dressing *in situ*. I formerly allowed my patients to sit up and walk about the room, the day after the operation, but I was so often annoyed by the supervention of hemorrhage that I at length adopted the plan of keeping them down till the spontaneous separation of the intra-cervical dressing.

On the day after the operation the whole of the vaginal portion of the tampon is to be carefully removed, placing the patient in the position as for the operation, and using the speculum, which must be introduced so as not to derange the relations of the dressing. When it is all removed down to the intra-cervical portion, a wad of cotton saturated with Price's glycerine, and large enough to cover completely the cervix and its first dressing, is laid over it, and the patient again lifted into bed. The action of this, as already fully explained, is to induce a profuse watery discharge from the vagina, which keeps the part cleanly drained of all secretions or exudations from the decomposition of the blood contained in the original dressing. This glycerined cotton is to be removed and renewed daily till the suppurative process throws off the dressing from the neck of the womb. This will not be under three or four days. In the meantime, the glycerine, by its detergent and antiseptic properties, keeps everything sweet and clean; and its affinity for water, which by osmosis it extracts from the tissues with which it lies in contact, keeps the parts entirely clear of any secretion that might be re-absorbed and poison the blood, if not thus drained off by the chemico-capillary action of the dressing. No one can thus apply glycerine to the neck of the womb and not be struck with its peculiar power and properties. The intra-cervical dressing will be loosened on the third day or later, and it may then be gently removed with forceps. If it adheres obstinately, let it alone, but cover it and the whole cervix with the cotton glycerole, and at the next dressing it may come away easily. I have frequently provoked bleeding by a little impatience in removing it prematurely. When it is once safely out, then the cervix is to be plugged with a small bit of cotton glycerole, and the whole covered as before with the same. This dressing is to be renewed daily till the parts have entirely healed, which usually takes from twelve to seventeen days, or perhaps till the recurrence of the next menstrual period. And this reminds me that the operation should always be performed within from three to five days after a menstrual epoch, so that we may have time enough for the healing process to be wholly accomplished before the recurrence of the next.

There is sometimes great trouble in keeping the mouth of the womb sufficiently open. It never remains just as we cut it. The tendency of all cicatrizing wounds to contract as they heal is wonderfully illustrated here. I have often been amazed to find the os contracted in a month to one-fourth of the size of the original incisions. I have frequently seen it cut open large enough to admit the index-finger up to the os internum, and then close in a few weeks to such a degree as not to admit a No. 4 or 5 bougie, and this in spite of persevering efforts to prevent the contraction. This is the case where there is great induration of the cervix, with deposit of white fibrous tissue. I have frequently been compelled to repeat the operation, and I remember several patients upon whom I have operated as often as three times in the course of a few months, and even then the result was not wholly satisfactory. These may be called exceptional cases, but it is well to know that they are not very rare. Even when the os tincæ remains open enough, we may have some trouble in keeping the contracted portion above of normal dimensions. This may be the case if there is much of a flexure, particularly anteriorly. And here I would recommend the occasional passage of a bougie after the first week. Dr. Emmet is in the habit of using the sound as early as the third day after the operation, passing it into the cavity of the womb, and pressing it pretty firmly first against one side of the canal and then against the other in withdrawing it. I have in a few cases followed his example, but with a little timidity. Dr. Greenhalgh uses a self-retaining intra-uterine stem, which is very ingenious, and answers well in his hands.

This operation often cures dysmenorrhœa. Sometimes it only modifies it. And again, I have seen cases where it produced no beneficial effect whatever.

The first menstrual flow after it is usually ushered in without the premonition that had so long harassed the poor sufferer, and she may pass through the whole period with comparative comfort; but I think it advisable for such patients to take very good care of themselves at each return of the flow, and to avoid all unnecessary exposure or fatigue. If there is pain enough to lie down, I direct an anodyne by the rectum, and for this purpose McMunn's elixir of opium is the very best. It is less apt to nauseate or to produce headache than crude opium or any of its alkaloids. It is more efficacious by the rectum than by the mouth, because it is more immediately in conjunction with the nerves of the affected part.

Morphine taken into the stomach is a very poor remedy for sciatica, but we all know that it exerts not only a temporary soothing effect, but a wonderful and rapidly curative action when endermically injected along the course of the affected nerve—a boon to suffering humanity for which we owe an everlasting debt of gratitude to the honored name of Alexander Wood, of Edinburgh. It is upon this principle of applying the remedy as near as possible to the seat of suffering that the anodyne for dysmenorrhœa is far more efficacious by the rectum than by the mouth.—*Lancet, April* 1, 1865, *p.* 338.

112.—ON DILATATION VERSUS DIVISION OF THE CERVIX UTERI IN DYSMENORRHŒA.

By Dr. G. T. Gream, Upper Brook-street.

[Dr. Marion Sims as will be seen by the preceding paper repudiates dilatation of the os uteri as dangerous in all its aspects, and declares that division of the cervix is as safe as dilatation is hazardous. This opinion Dr. Gream contests.]

It is now many years since this operation was proposed by certain practitioners as a cure for dysmenorrhœa and sterility; and from that time I have repeatedly been consulted by women who, at different dates antecedent to my seeing them, had undergone the operation; and, except in one solitary instance, I have met with no one in whom pregnancy had supervened upon this mode of treatment; indeed, their application to me was consequent upon the failure of the plan which had been adopted.

The exceptional case was this. A lady to whom the birth of an heir was most important found herself with child, and sought my attendance when she had arrived at about the fourth month of pregnancy; telling me—without, however, attributing her pregnancy to the operation—that the uterus had six years before been operated upon; and so complete had been the division of the cervix that the finger could readily be introduced into the uterine cavity, and the membranes of the ovum could be touched, as they can be sometimes during the last days of gestation. The patient was an old friend of a physician whose name it is only necessary to mention to give assurance of his ability to judge of these matters; and I begged that his opinion might be asked in reference to the chance of a continuance of the pregnancy. His opinion corresponded with my own, and, according to our prognostications, abortion resulted but a few weeks afterwards, from the inability of the uterus to retain its contents. I need not say that the hopes of this patient are irreparably gone.

I could relate cases of cellulitis, of pelvic abscess, and of chronic as well as of acute metritis, depending entirely upon the performance of this operation; but the following case will show some of its ill effects. A lady arrived in London in a deplorable condition, the effect of an operation upon her uterus which had some time before been performed. She was most desirous to reach home, and had been permitted to do so only upon her urgent entreaties, because her general condition was such as to give alarm lest she might not accomplish her journey. The physician who had been attending her, whose advice was sought after the operation had been performed, and when its bad effects became apparent, wrote to me, begging me to give her my most serious attention. It was desirable that she should be often seen;

and Mr. Morgan, of Sussex-place, kindly undertook the charge of her, living as he did near to her own residence; and he and I conjointly attended her for months during the formation of an extensive pelvic abscess, which ultimately discharged itself into the intestine, and she recovered, but with this result: her uterus is permanently fixed to the left side of the pelvic cavity, and any chance of future conception is totally at an end.

I could relate cases of alarming hemorrhage immediately following division of the cervix, and others in which the lamentable consequences were deferred, and in which death ensued; and I confess I was surprised to find that Dr. Marion Sims had been assured by some of the promoters of this plan of treatment that they had never met with hemorrhage; for I fancied that my memory carried me back to evidence of a contrary nature. But of this I will say no more.

The treatment of sterility is but seldom called for, except by those in the more wealthy classes of society, as the birth of children to persons amongst other classes is comparatively of less importance; and hence experience is gained in mature private practice rather than in hospitals. It seems incumbent on those, therefore, who have had the opportunity of treating cases of this kind, to make public their experience; and I am sure that those of your readers to whom I am known will give me credit for stating the truth, when I say that there are numbers of women in the upper classes of society who would sacaifice half their lives to become mothers, but who, from having undergone the operation of division of the cervix uteri, have lost all chance of being pregnant.

I agree with Dr. Marion Sims that dilatation may prove hurtful, indeed may be dangerous to life; but not when properly performed. I have myself seen the uterus enlarged to the size of the fifth month of pregnancy by the undue means taken to dilate the os. I have known instruments so rudely forced through the canal that acute symptoms have followed, requiring most active means for their suppression. And I have myself found pieces of metal fixed in the uterus, which had remained there, in some instances, for days, and in others for longer periods; while inflammation was set up, and abscess resulted. I have known repeated dilatations followed by chronic as well as by acute inflammation; and this in single women. In one case—a case ultimately in charge of Mr. Turner, of Sussex-gardens, who asked me to meet him in consultation concerning it,—there had been no less than sixty instrumental introductions with the view to remove dysmenorrhœa; and this in an unmarried woman. But these are not the cases exemplifying mischief by dilatation; for common experience shows that the uterus will not admit of the permanent introduction of foreign bodies without bad results, and prolonged repetition of dilatation is in no case called for. I entirely discard, therefore, all such cases as arguments against dilatation, as well as others mentioned by Dr. Marion Sims; and I repudiate, with all the power that words possess, the tampering with the virgin uterus under any but the most urgent suffering.

I remember when I was a pupil of Sir Benjamin Brodie, at St. George's Hospital, that great surgeon introduced the practice, when extracting a calculus from the female bladder, of dividing in two or three places the mucous membrane of the urethra, throughout the whole canal, by means of a bistorui caché, in order that, as cicatrization proceeded in these wounds, the urethra might contract, and thus prevent the incontinence of urine which usually followed in such cases.

Now it appears to me that there is no difference in the behaviour under cicatrization between the mucus membrane of the cervix uteri and that of the urethra. Hence this operation becomes rather an impediment to than a cause of pregnancy. And when it is remembered that we are acting upon the most dilatable part of the whole body—so dilatable, indeed, that the fœtal head even escapes through it by dilatation,—why should we seek other methods for enlarging the os uteri than dilatation, when it alone is the process especially pointed out as fitted to the nature of the part under treatment?

I regret exceedingly to differ from Dr. Marion Sims; but when I have

been witness for many years past to the cure of dysmenorrhœa in married women through dilatation of the os uteri, and when I have seen pregnancy repeatedly and often immediately supervene upon the same operation, it has occurred to me that I have some claim to your indulgence and that of your readers for considering the two questions of incision and dilatation in an opposite point of view to that entertained by Dr. Marion Sims.

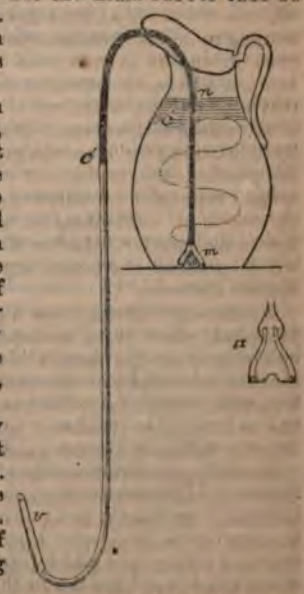
It is far from my wish to be supposed to suggest to so experienced a practitioner as Dr. Marion Sims appears to be the adoption of a mode of treatment which differs from his own; but he has evidently taken his objections to dilatation of the os uteri from cases which in my view were evidences, not of dilatation, but of maltreatment of the uterus. If, however, he will try the experiment of introducing a very small flexible metal bougie, such as is used for the male urethra, about two inches into the os uteri, and follow this by others gradually increased in size until the canal is dilated to the size of a common pencil; or if he will use a dilator such as was mentioned by Dr. Priestley, I think, in the 'Lancet' a few months ago, or one which was made for myself, and which was described by me in your journal,—he will find it more successful in cases of sterility than slitting the cervix uteri, and thus inflicting a permanent injury on that organ.

The whole process of dilatation may be performed in two or three operations at the most.—*Lancet, April* 8, 1865, *p.* 381.

113.—A SIMPLE INSTRUMENT FOR VAGINAL INJECTIONS.

By Dr. Adolph Rasch, Physician to the Eastern Dispensary of the German Hospital.

The instrument I have the honour of bringing before the Society consists of a long india-rubber tube (six to seven feet long, and about as thick as a lady's little finger), made heavy at one end, with a hollow, bell-shaped lead, the base of which has large grooves, to allow the entrance of the fluid from the very bottom of the vessel. It is essential that the india-rubber tube be strong and well made, to prevent its collapse. An elastic thick bougie, with several holes in the bulbous end, and not less than five inches long, is fastened at the other end of the tube.



To use this instrument, place some vessel on a high place (mantle-piece, chest of drawers, &c.), plunge the metal end of the instrument along with about two feet of the tube into the fluid to be injected (the dotted lines show the tube in this stage), and allow the immersed part to fill, which is done in a few seconds; then compress the tube with two fingers over the surface of the water, and lift the *filled* part of the tube over the brim of the vessel and *lower* than the surface of the fluid in the vessel. By this little trick we establish a current in the syphon, and make all contrivances for suction, &c., unnecessary.

The drawing needs little explanation. By straightening the filled tube, *c* will be at *c'*, that is to say, lower than the surface of the fluid, *n*. The dark part shows the tube filled before the compressing fingers have been withdrawn. *a* represents the metal (the under surface of which is grooved) to which is attached the long flexible tube.

The fluid will, of course, be propelled out of the vaginal tube the more forcibly the higher the vessel with the fluid is placed. When using the instrument the patient should sit over a basin to receive the fluid from the vagina.

Greatly as I rely on injections in the treatment of diseases of the vagina and uterus, I have considered it a duty to bring my simple contrivance before the Society. Having no complicated construction, every one can put it together himself. It cannot get out of order, and deserves a trial.

As the quantity of fluid for a douche should be large, it is advisable to order the ingredients in powders or concentrated solutions to be mixed with a jugful of water. As alum, zinc, lead, &c., are very cheap, there can be no objection to this, even in the practice among the poor.

The instrument is made by Mr. Evan Lewis, 31, City Road.—*Obstetrical Transactions, Vol.* vi., 1865, *p.* 224.

114.—ON THE SURGICAL TREATMENT OF VESICO-VAGINAL FISTULA.

By Dr. Awly P. Banon, Member of Council of Royal College of Surgeons, Ireland; Surgeon to Jervis street Hospital, &c.

[In the first three or four cases operated on by Dr. Banon he followed Boxman's plan, and with success. After shortly describing this operation, he passes on to observe:]

For very small fistulæ, particularly when situated high up in the vagina, I have myself contrived an instrument which I shall endeavour to describe, and which I found of much use in fixing and bringing within easy reach of the knife the edges of the aperture. The instrument is a curved steel sound or probe, provided with four elongated points projecting backwards. It may easily be forced into the smallest aperture, and on withdrawing it will grapple the margin of the fistula on every side, and enable the operator to divide it with the greatest ease. Its introduction will be much facilitated by previously passing a sound through the urethra into the bladder, by which the membrane can be made tense and raised towards the operator. The necessity of some such instrument suggested itself to me in August, 1863, in the case of Mrs. Quanby, which is one of those I am about to detail. In her case the vagina was contracted, and the fistula, a mere pin-hole, situated very high up, the instrument proving of much use in bringing it within reach. I subsequently used it, and assisted Drs. Kidd and Cronyn, who recently operated in cases of this form of fistula, and found it greatly to facilitate their operations. In my earlier operations, after pairing the edges and introducing iron or silver wire sutures, I followed the plan of Bozman, of using leaden plate perforated with a double row of holes corresponding to the number of sutures intended to be used, and cut to the proper size, on which the sutures were twisted; but though I found it to answer well, I have latterly omitted it as unnecessary, and have followed the more simple plan of Marion Sims, of twisting the silver sutures down on the divided parts, having, of course, previously brought them accurately into apposition. I had the advantage of seeing Dr. Sims operate when in Dublin, two years since, and had several conversations with him, and was so struck with the simplicity of his method that I have since then, to a great extent, followed it. The position recommended by Sims is the semi-prone, which is much less harassing to the patient as well as to the operator than that on the hands and knees recommended by Bozman. This latter will, however, in some cases be found convenient, as will also that of the lithotomy position recommended by Baker Browne. Chloroform may be advantageously used in the semi-prone or lithotomy positions, but not in that resting on the hands and knees. After the operation the patient should be kept quiet, and the bent or syphon catheter introduced and left in the bladder, changing it every day, or oftener if required, until the 8th or 9th day, when the sutures may be removed, and the patient allowed to pass urine by her own efforts. I am aware that some surgeons do not consider this course necessary, and dispense with the catheter altogether. Of the results of this practice I have no experience, having *never tried it*, but knowing that all strain must be kept off from the recently

divided parts by the retention of the bent catheter in the bladder, and that it is generally borne without much discomfort, I should be slow in giving up its use.—*Dublin Quarterly Journal, Feb.* 1865, *p.* 30.

115.—CHILD CROWING—THE MOST EFFECTIVE REMEDY EXPOSURE TO DRY, COLD WINDS.

By J. Robberton, Esq., Manchester.

It may be said, and with truth, that various authors among their remedies for this disease recommend change of air. It is not, however, mere change of locality that I advise, but the free exposure of the infant out of doors, for many hours daily, to a dry, cold atmosphere; and if the air be dry the colder the better. The following, from the pen of the late Dr. Marshall Hall, is worthy of attention; and I refer to his opinion regarding atmospheric exposure in child-crowing on account of his acknowledged eminence both as a writer and a practical physician:—

"The baneful influence," says he, "of the north-east wind, and the curative influence of change of air, and especially of the sea breezes, are not less marked in this affection than in whooping-cough." Again: "All inclemencies of the weather being avoided—for heat, cold, and the north-easterly winds are alike injurious—the child should be much in the open air."

The five following cases, selected out of a number in which I have been consulted since the publication of my paper several years ago, and all of which have been treated on the same principle, are instructive, especially as showing, contrary to Dr. Marshall Hall's opinion, that easterly and northerly winds are not injurious but curative. Were these winds injurious it would indeed be unfortunate, since, owing to causes I cannot account for, every instance of child-crowing that I have seen has commenced in the winter or spring, as the following list of cases (of which I possess notes) proves:—In November there was one, in December four, January seven, February eleven, March thirteen, and in April four; in all forty cases.

In the way of preparation for change of air the following particulars demand attention:—

1st. Careful scarification of the gums, so as to make and keep bare the tips of the advancing teeth.

2nd. Correction of the alvine secretions, which are nearly always of a pale colour, by small doses of mercury with chalk shaken up in a little syrup.

3rd. Free sponging of the body every morning with cold water.

4th. Warm clothing, suitable to the season of the year.

5th. If the child is in part or altogether handfed, careful attention to the diet; since excess of food is found to aggravate the crowing and other symptoms.

Case 1.—January, 1856.—E. H., a girl twelve months old, with only two lower incisors cut, and suffering from the gums over the upper incisors, and crowing spasms very frequently, and while in them was stiff and struggled severely. The thumbs were turned into the palms of the hands, and there was blueness always remaining under the eyes and round the mouth and nose. Was taken in January to Alderley, but the weather being damp and close, she grew rapidly worse while there, having as many as thirty-two attacks of crowing in the twenty-four hours. At the end of a week she was brought back to Manchester, and then taken without delay to the shore below Liverpool. There the weather was found to be intensely cold, and a strong and bitter east wind blowing. Sometimes the nurse who carried baby about could hardly stand with her in her arms, so strong was the wind. Baby was a month at the shore, being out in all weathers for about six or seven hours a-day. From the end of the first week there was evident improvement, and by the end of the third week the crowing spasms had ceased, nor has she ever suffered from a return of them. She was sponged with cold

water night and morning; had careful attention paid to the diet, especially as to the quantity she ate; and before the two upper front teeth (which caused her much suffering) came through, her gums had been lanced eleven times. She is now a strong healthy child.

Case 2.—February, 1865.—H. P., a boy ten months old, had for some weeks been feverish and starting in consequence of cutting his side teeth; the gums were frequently scarified, giving relief at the time. About a week before "crowing" was heard he had two convulsions, and for some days was seriously ill. "One morning," as the mother wrote me, "on going up early to his room the nurse drew my attention to his hands—the thumbs were drawn into the palms; and when dressing him I found his feet were also drawn, so much so that I was unable to put on his boots. The nurse told me she had been alarmed by the child making a peculiar noise as though struggling for breath. I remarked also a blueness about the mouth and nose, which made me fear he had been convulsed during the night. I had heard you describe the symptoms of 'crowing inspiration,' and therefore lost no time in sending for you. Your order was to take him off without delay to Blackpool or Fleetwood. On account, however, of the severity of the weather, and his having a cough, I sent him with his nurse to Southport. In a day or two I went to see him, and found him very little better: his hands and feet were not so much drawn, but the 'crowing' was more frequent. On my reporting this, you considered Southport not sufficiently bracing, and you wished me to take him to the bleak shore at Fleetwood, which I accordingly did. There had been a heavy fall of snow, and a north-east wind prevailed more or less the whole time at Fleetwood. Every day, wet or fine, he was wrapped up warmly and drawn out in a perambulator from morning till evening. He improved daily; the fits of 'crowing' became gradually fewer and less severe, and at the end of three weeks he returned home perfectly well."

Case 3.—January 11, 1857.—Visited Mr. W.'s baby, a male, aged fourteen months, suffering from the incisor teeth, and from fits of crowing with clenching of the thumbs. The gums were scarified, the bowels regulated, care enjoined as to diet, and it was intimated that he must be removed immediately to the sea side. In about a couple of days an attack of crowing terminated in convulsions, and as the child's grandfather happened to live near Disley, at the foot of the hills, the family, with my approval, had him carried thither. On reaching Disley, as the nurse was lifting him from the carriage, he had crowing followed by convulsions. The weather was dry, the cold intense, and the country deep under snow; nevertheless, he was carried daily to the top of a neighbouring hill, and afterwards in a perambulator was wheeled about in the snow the most of the day, greatly to the surprise of the country people, who thought the nurse must be out of her mind. A man said, "Are ye mad, lass? ye'll kill that child." She replied, "The doctor has ordered him to be carried on the hills." "I don't care about Doctors," said he, "ye'll kill him." The crowing was rapidly mitigated, and I was assured by the babe's aunt, who accompanied the nurse, that the crowing and other symptoms had disappeared by the end of a week.

Case 4.—February 4, 1857.—Was consulted regarding a fine, vigorous male infant, aged eleven months, suffering from the incisor and biscupid teeth, with crowing and drawing of the thumbs. After dividing the gums, minding the bowels, the diet, and the clothing, I advised a removal to the sea side. The baby's mother, however, having occasion to visit a relative, about ten or twelve miles north of this, in the country, took him thither for change, but with little or no benefit, and on her return, the crowing being frequent and of alarming violence, I advised his being taken forthwith to the dry, breezy shore of Blackpool. His mother wrote me from Ulverston as follows:—"We have come here after a month at Blackpool, and I am thankful to be able to give you a satisfactory report. You will remember that the day we last brought baby to you his thumbs were in; we went that afternoon to Blackpool, and before night the thumbs were out, and have only been in once since for part of a day, which was about the tenth day after we got there. We were not able to carry out your system fully for the first

ten days, on account of a sharp cough, the weather being at the time most severe, but as soon as the cough moderated we had him out all weathers—for the first week from five to seven hours daily, the second week from seven to eight hours, and for the last week he has been out between ten and eleven hours each day. We took him out both when sleeping and waking, in hard frost, in the bitterest east wind, in a north-westerly wind that I could scarcely stand against with him in my arms, in hail-storms and in snow, and yet he never suffered from cold, but on the contrary benefited visibly in health, whilst the crowing gradually diminished. We wrapped him up well certainly, and in that inclement weather he was the only one of the party to come in warm. I have strictly followed your directions; the diet is plain, and the meals given with regularity; the bowels are in order, and he ails nothing but the occasional crowing, which has now ceased to be alarming. His eye tooth and four double teeth are coming, yet it is wonderful how little they affect his health and spirits; he has twelve teeth through, and is now 13¼ months old." On returning from Ulverston after two or three weeks, the crowing and the other symptoms had ceased.

[Mr. Roberton, after relating the fifth case, which was of a similar character, says:]

I have, in conclusion, to observe that in the numerous cases of stridulous crowing that have come under my notice, painful dentition has been the sole cause. I have never seen or heard of one instance of the disease before or after dentition—always during the teething period; and the opinion advanced by some that child-crowing is owing to morbid enlargement of the glands of the neck, or of the thoracic absorbent glands, has no support from facts.—*Medical Times and Gazette, Jan.* 14, 1865, *p.* 32.

SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE PRECEDING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

AFFECTIONS OF THE SYSTEM GENERALLY.

ACUTE RHEUMATISM.—*Treatment by Blisters.*—Those cases answer best to the treatment by blisters, freely placed over the inflamed joints, in which a great number of joints are simultaneously affected, and when by setting up a large amount of discharging surface in the proximity of the inflamed parts, a large proportion of the materies morbi may be evacuated at one *coup*. The urine diminishes in acidity during this treatment, and in 10 cases out of 43 became positively alkaline. (Dr. Davies, p. 39.)

FEVER.—Let the aliment chiefly consist of milk and water, butter-milk, rice and milk, arrow root, and beef-tea, sometimes with a little bread, where it is relished. This must in great measure be left to the patient's own feelings, but milk diet should in all instances form the staple nourishment. Neither any medicine nor alcoholic stimulants should be given as matter of routine, they should be directed entirely to emergencies and casualties. (Dr. W. T. Gairdner, p. 21.)

The use of stimulants in disease is not now so much in vogue in many of the London Hospitals as was formerly the case. In the treatment of typhus and typhoid fevers, although a little wine may be very useful occasionally, it is rarely necessary, for alcohol be it remembered is not an antagonist to the fever. (Dr. Wilks, p. 17.)

GOUT.—There are two ways in which an excess of uric acid may occur in the blood and textures: 1st, from an excess of animal or vegetable albuminous food entering the system; and 2nd, from an arrest of oxidation. It is evident therefore that there are two modes of preventing the gouty diathesis: first, by diminishing the amount of albuminous food; and secondly, by promoting oxidation. In other words, the smallest amount of food, and the greatest amount of air are the antidotes for the gouty diathesis. (Dr. Bence Jones, p. 86.)

Chronic Gout treated by dilute Hydrochloric Acid.—It is better to direct our treatment in cases of chronic gout to increase the efficiency of the assimilative process, and so prevent the formation of lithic acid, than simply to aid its elimination by the administration of alkalies after it is formed. Hydrochloric acid with cascarilla is more likely to be useful than potash and lithia water. Several interesting cases are related, in which most marked success followed the judicious carrying out of this plan of treatment. (Dr. James F. Duncan, *Dublin Quarterly Journal, May* 1865, p. 302.)

MALARIOUS FEVER.—Quinine may be given in scruple doses whether there are head symptoms, delirium, coma, or pain in the liver. Whether it be in the hot stage or cold, quinine is not only safe for all forms of malarious fever, but its certain cure; and in cases where there is danger to life, the earlier and the larger the doses of quinine which can be given to the patient the better. Vomiting and full cinchonism should be the only checks for continuing it in full doses. (Mr. E. Hare, p. 31.)

The action of quinine when administered in large full doses, say of half a

drachm followed by a scruple every two hours, is that of a powerful nervine sedative. It has almost a paralysing action on the brain. In some cases even dangerous symptoms are produced: deadly coldness, arrest of the pulse, and paralysis of the heart. In smaller doses it produces the excitement usually spoken of. In sufficient doses it is however a specific, and with water to wash out the bowels, and apply to the head, no other medicine is wanted for the successful treatment of the worst forms of tropical fever. It is well always to mix calomel with the quinine when the patient is a child; its effects are marvellous in these cases, even when no other visible effects are produced. Five grains of quinine is a sufficient dose for a child, but without calomel double this dose is necessary. (Mr. E. Hare, p. 36.)

Rheumatic Fever.—*Alkaline Baths.*—No local application will relieve the pain so much as spongio-piline soaked in a warm saturated solution of nitrate and carbonate of potash, and applied to the joints. By the use of an alkaline bath in which the whole body may be immersed, an attack of rheumatic fever may be arrested in limine. The bath should contain about twenty-four gallons of water at 100°, two pounds of bicarbonate of potash, and one pound of nitrate of potash. The patient should remain in it at least ten minutes. (Mr. B. G. Everett, p. 42.)

Scarlatina.—*Bisulphite of Soda.*—The sulphites, so much recommended in cases of catalytic disease, as scarlet fever, act as preventives if taken when the person is exposed to infection: but when the catalytic action has once taken place, as evidenced by the presence of the rash, no sensible effects are produced by their administration. In severe cases the salt should nevertheless be administered in doses of ten grains every one, two, or three hours; for the chief danger is the tendency to secondary inoculation of the blood which the sulphurous acid, set free when a bi-sulphite is decomposed by the acid of the stomach, tends to prevent. It may not only be used in scarlet fever but all infectious febrile diseases. (Dr. W. J. Cummins, p. 38.)

Typhus Fever.—In a large proportion of cases, typhus fever left to its natural course, and treated with abundant milk diet and without drugs or stimulants, will have its natural crisis before the twelfth day. No other food can be depended on than milk, or buttermilk; to give wine, whisky, and beef tea while withholding milk is to diminish the chance of the patient's recovery. Large doses of alcoholic stimulants are positively injurious, as is also the deprivation of a plentiful supply of fresh air. Our guide to the progress of a case of fever is, to watch carefully and constantly the rate of the pulse, for it gradually rises in frequency up to the acme of the fever, and then as gradually declines, so that the crisis can be accurately ascertained. This is of immense importance as regards the prognosis, for the other symptoms may continue as formidable as ever, but if the pulse is gradually declining in frequency, the case will do well. (Dr. W. T. Gairdner, p. 21.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Chorea.—*Aniline.*—Sulphate of aniline has a powerful sedative influence on the nervous system when taken internally. When taken in medicinal doses, say of two to three grains thrice a day, it is an excellent remedy in cases of chorea, in some cases rapidly diminishing the muscular irritability. After a time it produces blueness of the lips and face: this, however, is only temporary. It is also liable to produce great depression. Its use must then be left off for a short time. Aniline is a distinct thing from nitro-benzine, which is a product similar to glonoine (produced by the action of nitric acid on glycerine) and to gun cotton (produced by the action of nitric acid on cotton). Nitro-benzine has very noxious properties. Of the metallic tonics iron is the best, arsenic next. Cod-liver oil is frequently of service by improving the general health. The use of the shower-bath and of electricity are well known; but the shower-bath occasionally proves injurious from the fright it may occasion; its use therefore requires caution. Chloroform and

preparations of opium are not only useless but frequently injurious. (Dr. J. Turnbull, p. 50.)

SCRIVENERS' PALSY.—Entire rest from the occupation that has produced the disease is the one essential point in its cure. If it has been recognised early, two months' entire rest will arrest its progress; at the end of that time strychnine is of service, but it is injurious when given in the early stage of the disease, and before rest. The health should be improved as much as possible. Fresh bracing air is of more service than all the tonics in the world. (Mr. S. Solly, p. 42.)

WASTING PALSY.—Wasting palsy, or progressive muscular atrophy, is that disease which is now generally regarded as primary idiopathic granular or fatty degeneration of the muscular fibre, analogous in all essential features to fatty degeneration of the heart. It is recognised as being a partial wasting of muscle, forming a marked contrast to that general wasting which occurs after paralysis, and by the muscles not being selected in those groups in which they would be were the disease the result of some lesion in a nerve centre. As long as any muscular structure remains, the will is capable of acting upon it, the weakness being posterior to the wasting in point of time. The treatment must consist in Faradisation, in kneading, and suitable localised movements to be used every day. The general treatment must be tonic, good diet and stimulants being allowed. The best medicine is perhaps the syrup of iodide of iron. The disease is very liable to occur in the hand, arm, and shoulder. (Dr. C. B. Radcliffe, p. 44.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

CARDIAC DISEASE.—*Digitalis.*—Digitalis will stimulate and strengthen a weak heart, and the weaker the muscular tissue the safer is the administration of the remedy. It will reduce the number of contractions in a weak heart acting rapidly, and quicken the action of a weak slowly acting one. It should be administered in doses of ten drops of the tincture and upwards. Cases of dyspnœa from an emphysematous state of the lungs and sluggish pulmonary circulation are generally most decidedly benefited by the use of digitalis; indeed, in cases of hypertrophy it is productive of no good result, and is better avoided altogether. (Dr. Wm. Murray, *Medical Times and Gazette, March* 18, 1865, p. 277.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

ASTHMA.—*Chloroform.*—During the height of the asthmatic paroxysm chloroform is one of our best remedies. It acts by removing that which is the whole cause of the asphyxial stoppage, the bronchial spasm. It produces almost immediate relief in most cases, and in a few this relief is permanent after the insensibility has passed off; in most it is only co-existent with the insensibility. In some cases it is productive of harm, but these cases are very rare. In some cases it relieves the paroxysm without producing insensibility. No amount of asthmatic dyspnœa or asphyxia is any bar to its use. If given constantly, however, in large doses, for a long period, its use becomes a pernicious habit, and produces effects allied to those of chronic alcoholism. (Dr. Hyde Salter, p. 69.)

PHTHISIS.—*Diagnosis.*—Ordinary cases in which the disease is localised to one lung or to one portion of a lung, are readily diagnosed even in their early stages, but there are cases in which minute tubercles are deposited equably throughout the whole of both lungs, and in which the symptoms so resemble an attack of mild bronchitis as to mislead the medical attendant. These cases of phthisis are usually acute, and are attended with other symptoms of a constitutional character. The case is protracted, and frequent sweats occur unattended with amelioration of the symptoms. To the very last the

percussion sound is normal, although both lungs may be filled with tubercles. (Dr. J. Russell, p. 59.)

PNEUMONIA.—*The first auscultatory sound recognisable* in cases of pneumonia is not the well-known crepitating râle, but an intensely puerile respiration in the affected part, loud and harsh, and contrasting with the natural respiratory murmur of the healthy parts of the lung. (Dr. A. T. H. Waters, p. 63.)

Inflammation of the lungs appears to go through its natural progress so long as the body is not too much exhausted, and the physician as early as possible supports it by nutrients and restoratives. Prostration and weakening complications or remedies not only materially lengthen the period of the disease, but especially prolong the convalescence. As a means of palliating symptoms, and especially pain and dyspnœa, warm fomentations and poultices are the best and safest. In some exceptional cases, a small bleeding may be employed as a palliative to relieve engorgement of the lungs and congestion of the right side of the heart. As a curative treatment the best plan is rest in bed, nutritive drinks, especially good beef-tea, from the first, assisted by port wine (from four to eight ounces) if the pulse becomes weak, and solid nutrients as soon as they can be taken. The elimination of the exudation may be further assisted by salines (acetate of ammonia and small doses of tartar emetic—one sixteenth of a grain), and diuretics (nitric ether), although nature will accomplish this herself if the strength of the body be well maintained. All saline purgatives, contra-stimulants, depressants, anodynes, and lowering remedies of every description should be avoided. This practice only differs from that of the late Dr. Todd in the avoidance of unnecessary stimulation. (Dr. J. Hughes Bennett, p. 64.)

RHEUMATIC BRONCHITIS.—It is not uncommon to have cases of subacute bronchitis at the same time as articular rheumatism. The chest symptoms do not, however, seem to be influenced by the state of the arthritic affection, but run quite an independent course. No medicine especially directed to this form of bronchitis is of much use, but under the use of iodide of potassium, alkalies, and other remedies directed to the rheumatic state, the bronchitic symptoms will disappear. There appears to be two forms of the disease, the one purely rheumatic, the other produced by the modifying influence of the rheumatic diathesis on ordinary bronchitis. (Dr. J. Jones, p. 67.)

TUBERCLE.—*Exciting Cause of.*—There is little or no difference between tubercular infiltration and the low plasma found in the pneumonia of debilitated subjects. The two may almost be said to be identical. In cases of consumption, given the cachexia, the only exciting cause requisite for the production of tubercle is a low form of inflammation. The varieties of tubercle depend upon the acuteness or chronicity of the inflammation, the degree of the cachexia, and the nature of the structure on or in which they are formed. The doctrine of spontaneous generation of tubercle is fraught with danger to the patient by diverting the attention from the immediate exciting cause, and making our treatment one-sided by being directed solely to the constitutional malady. This low form of inflammation is treated with the best results by ammonia salines, with tartarised antimony and morphia, the antimony appearing to control the inflammation without producing any permanent debility. (Dr. James Jones, p. 53.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

CANCER OF THE RECTUM.—*Amussat's Operation.*—The operation for establishing an anus in the left loin is not nearly so serious nor so difficult of performance as is commonly supposed, nor is the inconvenience to the patient so great as is supposed. The bowel must be distended with an injection of linseed tea, passed up by means of a small tube, which may be kept in during the operation and its aperture plugged. A transverse incision four inches in length must be made midway between the crest of the ilium

and the last rib. The different fascial and muscular layers must then be divided on a director, until the subperitoneal fat is arrived at. The gut is reached through this at the bottom of a deep wound. The colon will be found uncovered by peritoneum, and having been drawn to the surface a longitudinal opening should be made in it, and its edges united to the lips of the wound by sutures. It is of the highest importance not to delay lumbar colotomy till the worst evils of cancer are experienced. (Mr. T. B. Curling, p. 157.)

CARIOUS TEETH.—*Carbolic Acid.*—This is an excellent substitute for creasote in cleaning out the carious cavity of a bad tooth. It can be obtained pure, creasote rarely or never can. Creasote never allays toothache, carbolic acid will frequently do so. (Dr. Watson, *British and Foreign Medico-Chirurgical Review, Jan.* 1865, p. 240.)

CLEFT PALATE.—The operation of staphyloraphy may be perfectly successful so far as joining the two sides of the cleft goes, but the patient rarely or never speaks any better on this account, because the ability to articulate must remain an impossibility so long as the velum and pharynx are unable to meet, and this after staphyloraphy they are rarely able to do. The substitution of an elastic artificial appliance to fill up the gap is, in skilful hands, capable of far more satisfactory results than have hitherto been derived from any other source. An obturator perfectly adapted to the muscles against which it is to lie, flexible, durable, and free from danger of misplacement, may be made. For description of this instrument see a full account at p. 151. (Dr. N. W. Kingsley, p. 147.)

ENLARGED TONSILS.—In most cases the use of very strong caustics is preferable to any cutting operation for removing the tonsils. Dr. Mackenzie uses what he calls "London Paste"; it consists of equal parts of caustic soda and lime. A piece of ivory slightly roughened and hollowed at the end, with a good light and a little care answers very well. A little of the paste should be placed in the hollow, and applied for about five seconds. From five to ten applications suffice to remove the enlarged tonsils. (Dr. P. C. Smyly, p. 156.)

EXCISION OF THE TONGUE.—The most essential thing to the success of an operation for the removal of the entire tongue, is to preserve the power of voluntary deglutition, the loss of which results from depriving the os hyoides of the power by which it is drawn forwards. Instead, therefore, of cutting through all the muscles of the os hyoides, the mylo-hyoidei and genio-hyoidei must be untouched, merely the attachments of the genio-hyo-glossi being divided. Chloroform should not be used. The details of a successful operation will be found at p. 140. (Prof. Syme.)

GASTRODYNIA.—*Oxide of Manganese.*—In cases of pain in the stomach following ingestion of the food, the black oxide of manganese is more efficacious in allaying the hyperæsthetic state of the mucous membrane than is even nitrate of bismuth. Manganese does not constipate the bowels. It has been tried in several hundred cases and the results were most satisfactory. It is also highly useful in pyrosis, generally removing first the watery discharge, and afterwards the pain in a short time. In certain irritable states of the stomach it is also of much service. In one remarkable case in which a woman had been affected with vomiting for many months, it was very successful in quieting the stomach. The purified oxide of manganese may be given in doses of five grains to half a drachm. The salts of manganese are not so suitable for therapeutical purposes. (Dr. A. Leared, p. 79.)

HARE-LIP OPERATION.—*Instrument for controlling Hemorrhage in.*—A pair of forceps, each blade having two separate points distant about an inch from one another, and deeply grooved to hold the lip firmly, is of great use in the operation of hare-lip. By it the hemorrhage may be completely controlled, and the surgeon may give his undivided and deliberate attention to the accurate adaptation of the denuded edges of the lip. (Mr. T. Smith, p. 151.)

ILIUS.—*Galvanism.*—A case of ilius is related in which no relief could be procured until action of the bowels was induced by the use of galvanism. The pipe of a syringe was introduced for some distance into the bowel, and

at the same time galvanism was applied rapidly by means of a sponge connected with the positive pole of a battery, over the whole abdominal surface. The sponge connected with the negative pole was placed on the anus, and even introduced into the rectum. A large quantity of fluid fæces was got rid of. Each time the sponge was applied to the abdomen, violent action of the abdominal muscles ensued. (Dr. Stokes, Mr. J. M. Finny, p. 81.)

Nasal Cavity.—*Diseases of.*—When one side of the nasal cavity is entirely filled through one nostril with fluid by hydrostatic pressure, while the patient is breathing through the mouth, the soft palate completely closes the choanæ, and does not allow any fluid to pass into the pharynx. Hence, by forcibly injecting water into one nostril it comes out in a stream at the other, washing it out, and ejecting any plugs of hardened mucus or pus, or any foreign body which may have obtained entrance. In ozœna beneficial medicated solutions may thus be applied, as solutions of alum, nitrate of silver, and sulphate of zinc. These must be weak at first, and strengthened afterwards if desirable. Pure warm water produces in most persons a very disagreeable sensation, and causes a copious discharge of watery mucus from the nose. To avoid this it is better to employ solutions of common salt for rinsing the nose (one ounce to the pint of water). The temperature of the solution must be rather lower than that of the blood. A dilute solution of permanganate of potash is very useful to remove the fœtor which exists in cases of ozœna. An apparatus for the convenient application of fluids to the nasal cavity has been invented and is described fully at p. 141. (Dr. J. L. W. Thudichum.)

Ozœna.—Syringe water freely through the opposite nostril to the one affected, and the liquid will come out at the affected nostril. It is astonishing what large plugs of congealed muco-pus will sometimes be removed. A teaspoonful of the following deodorant tincture, added to a pint of water, may sometimes be used with advantage:—Tincture of camphor, tincture of myrrh, of each three drachms; soap liniment, two drachms; glacial acetic acid, twenty minims; oil of pitch, one drachm. (Dr. T. Skinner, p. 146.)

Tape-worm.—*Male Fern and Kamela combined.*—Neither male fern nor kamela given separately produce very satisfactory results in the treatment of tape-worm, the head being seldom brought away, and the disease consequently recurring. But if they are given in combination, in a draught containing a drachm and a half of the extract of male fern, and two drachms of the powdered kamela, with a little mucilage and cinnamon water, half taken at bedtime, and the rest at 2 a. m., the result is pretty certain. (Dr. J. Brunton, p. 83.)

Trichiniasis.—*Benzine.*—Benzine may be obtained by distilling benzoic acid with lime; it is however usually procured in the preparation of coal naphtha. It possesses the power of destroying trichinæ, and when given with care does not prove poisonous to the animal infested by the parasite. ʒ ij. of benzine must be mixed with liquorice juice and mucilage of gum arabic, of each ʒ i., and peppermint water ʒ iv. Of this mixture a tablespoonful must be given every one or two hours. It is well borne in this form, but it kills the trichinæ still in the intestinal canal, and so prevents the propagation of the parasites. (Dr. Mosler, p. 193.)

AFFECTIONS OF THE URINARY ORGANS.

Acid Diathesis.—There is some relationship between the acid diathesis and diabetes; both are diseases of suboxidation; both arise from a check to the chemical actions of the body. The acid diathesis is, however, a less degree of error than is diabetes. Much may be done by the diet in the treatment of the acid diathesis. Fatty substances often produce irritation and heartburn. Sugar and butter should be left off; this alone will suffice in many cases. In severe cases of acidity small quantities of lean animal food,

including fish, flesh, fowl, game, and eggs, and brandy and water constitute the best anti-acid treatment. All fluids containing sugar should be avoided; and alkaline gaseous mineral water, natural or artificial, will neutralize a portion of the acidity which imperfect combustion continually produces. The free action of the skin by baths, especially Turkish baths and vapour baths, or by strong exercise (the best because implying a freer respiration), removes acid from the system. By increasing the action of the kidneys by means of pure water and nitre, and mineral waters, we may most effectually promote the cure of the case. There is great difference in the neutralising power of different alkalies, ten grains of lithia being equal to thirty-three of potash, and the carbonate of lithia is about double the neutralising power of the corresponding potash salt. (Dr. H. Bence Jones, p. 88.)

Bright's Disease.—First endeavour to relieve the anasarca, but not by irritating diuretics which injure the kidney, nor by strong purgatives which produce great debility, but by the frequent use of the vapour-bath. A vast surface is brought into secreting operation, and yet little besides water is eliminated. The diet should be such as to supply the waste of albumen; it should consist of eggs (especially the white) uncooked, and of plenty of juicy and underdone meat. Later in the case chalybeates and tonics are required, with the object of renovating the exhausted supply of red corpuscles. Pure air is also essential. The amount of albumen discharged may be arrested by astringents, such as gallic acid and tannin. (Dr. A. H. Hassall, p. 110.)

Calculi in the Kidneys or Bladder.—*Solution of.*—The urine may be kept alkaline for weeks together without injury, by the administration of large doses of citrate or acetate of potash. The administration of alkalies in this way is as harmless as the taking of so much sugar. Treatment of this kind is only of use where the stone is small and of the uric acid kind, when the urine is not alkaline from disease. (Dr. W. Roberts, p. 175.)

Passage of a Calculus from the Kidney.—Chloroform inhalation should be continued for hours, but only to the extent of producing slight unconsciousness. The remedy second in importance is the hot-bath at the temperature of 100° Fahr. continued for hours unless it causes faintness; the patient may afterwards be wrapped in warm blankets and in a few hours the bath may be repeated if necessary. Nothing weakens like pain. Opium may be used externally on a poultice, and it may be administered internally by subcutaneous injection. Half-a-grain of acetate of morphia in solution injected into the loin will act potently on the pain within a few minutes. (Dr. H. Bence Jones, p. 101.)

Diabetes.—The two great ends to be gained by medicinal treatment in diabetes, are, to promote the oxidation of the sugar, and to compensate the system for the loss of saccharine fuel by promoting the supply of the oleaginous fuel. The latter end is gained by the administration of vegetable and animal oils and fats. Of these cod-liver oil and cream are most frequently used. Of all the medicines which can be given for the promotion of the oxidation, whether of sugar or fat, in the body, iron and alkalies are the most energetic. Hence, iron, or the ammonio-citrate of iron with excess of ammonia, or with other alkalies, are the best medicines for diabetes. Alkalies without iron promote oxidation. M. Miahle has even stated that alkalies are the specific for diabetes. The carbonates of soda or potash or carbonate of ammonia may be given in any gaseous mineral water. (Dr. H. Bence Jones, p. 113.)

Hydrocele.—Tap the hydrocele in the usual way, and then introduce through the canula a silver probe, previously coated for an inch of its length with nitrate of silver. The probe must be prepared by heating the extremity to a dull-red heat, and then placing it in a little finely powdered nitrate of silver; it must then again be subjected to heat, so as to form a smooth coating. This plan of treatment is most quick and effectual. (Dr. W. Jollie, p. 176.)

Acupuncture.—When properly done acupuncture will frequently cure hydrocele without any other treatment. The needle used should be either triangular or spear-headed; a hare-lip pin will do very well. From three to six

punctures should be made, and the tumour will be gone in a day or two. (Dr. J. Thompson, p. 175.)

LITHOTOMY.—Be careful not to let the knife slip from the groove of the staff, otherwise it will cut too much backwards and run between the bladder and the rectum. The rectum must always be carefully emptied, by means of an injection, before an operation. As the arteries of the perineum are very irregular the first incision must not be very deep; the fat and subcutaneous tissue will readily yield to pressure. The more the prostatic portion is dilated by the finger, and the less it is cut, the better. (Mr. Holmes Coote, p. 162.)

Straight versus *Curved Lithotomy Staffs*.—There can be no doubt but that a curved staff is not the form best adapted to convey a cutting instrument where the eye of the operator cannot follow it, but the straight staff of Mr. Key has two important defects. It is too slightely curved at its extremity, so that there is risk that in the hands of an indifferent assistant its point may escape from the bladder; this may be easily remedied. The second defect is that the groove is continued around the curve at the extremity, whereas by its being made to terminate just before the curve begins in an abrupt stop the point of the knife is here arrested, and there is no risk of wounding the bladder beyond. The operator should always hold the staff himself with his left hand whilst he penetrates the bladder. He must be careful to depress the handle of the staff at the same time, in order to raise the bladder and prostate from the rectum. (Mr. J. R. Lane, p. 171.)

LITHOTRITY.—Great attention is requisite to minute details, in order to procure a perfectly successful result in cases of lithotrity. No sitting should exceed three minutes in duration, and the lithotrite must not be introduced more than twice at one sitting. A large fragment must never be withdrawn by it. Preliminary injections are not required, nor are subsequent washings out to remove debris, and moreover they are absolutely injurious, for much harm results from rapid and forcible distension of the bladder by means of injections. The operator requires no other instrument than the lithotrite, and in so short and unirritating a proceeding he requires no choloroform and no preparation. The sitting may be repeated every third, fourth, or fifth day. It is necessary to use a powerful and fenestrated instrument for the first crushings of a large stone. Large fragments result at first, but so far from desiring to remove them, it is important to discourage them from passing at first. By keeping the patient in bed for a day or two after the first sitting, desiring him to pass urine only in the recumbent position, irritation is reduced to a minimum. It is better that only a little debris should pass at first, until the flat-bladed lithotrite has been used to pulverise the fragments. The debris will then pass readily enough. (Mr. Henry Thompson, p. 166.)

Lithotrity in Cases of Recent Stone in the Bladder.—Lithotrity should not at once be performed in cases of stone which has only existed a very short period in the bladder. It should be allowed to remain a few months in order to accustom the bladder to mechanical contact, and the urethra must be inured to the passage of instruments by the occasional passing of a bougie. (Mr. Henry Thompson, p. 171.)

Lithotrity in Cases of Paralysed Bladder.—There are cases of stone accompanied with paralysis of the muscular coat of the bladder, or more correctly atony, the result of over-distension of the viscus. And there may or may not be at the same time obstructive enlargement of the prostate. This state does not by any means contraindicate lithotrity, and the operation is even more frequently attended with success than in the ordinary run of cases. The urethra has become so accustomed to the passage of a catheter, which such patients have probably been compelled to use for months, that the debris may be removed by the lithotrite with impunity. (Mr. Henry Thompson, p. 170.)

Lithotrity Instruments.—The usual large fenestrated instrument is necessary at the first crushing of a stone, but after this is accomplished, the proper instrument is a lighter flat-bladed one. The blades of this are shorter and unfenestrated, and the edges rounded. The blades do not quite meet, so that the fear of crushing the mucous membrane of the bladder is reduced

to a minimum. It is used solely to pulverise the fragments, and as it is withdrawn it may be made to withdraw more or less of the powder, according to the capacity of the urethra. (Mr. Henry Thompson, p. 168.)

Lithotrity, when unsuitable.—Lithotrity is suited almost exclusively to cases of men in middle or advanced life, of gouty habit, and with a deep perineum, in whom, after intervals of a few months, a small uric acid calculus drops from the kidney into the bladder, the kidneys being in their general structure healthy. The lithotrite should under no circumstances be used for the purpose of crushing an oxalate of lime calculus, the character of which is generally recognised by its sharp ringing sound. The fragments are always sharp and irritating to the bladder, and it not uncommonly happens that the lithotrite fails to act on the great mass of the stone. (Mr. Holmes Coote, p. 163.)

Oxalic Acid Diathesis.—The best medicinal treatment of the oxalic acid diathesis is that recommended by Prout, viz., the use of mineral acids. It is better to use each acid separately, according to the properties which each possesses, than to use the nitro-muriatic acid in one invariable form, as is generally done. As hydrochloric acid is especially the acid of the gastric juice, it should be preferred where there is feeble digestive power, and given before each meal of animal food. Nitric acid promotes secretion and perhaps oxidation, and it should be given an hour or two before meals on an empty stomach, in cases where secretion is defective. (Dr. H. Bence Jones, p. 96.)

Phosphatic Gravel and Calculi.—These may arise from four causes, all requiring different treatment. When there is retention of urine the cause of this must be removed before anything else can be done. When the stone is not due to any vice in the general system, but to a purely local cause, as where phosphates augment a small uric acid calculus, all tendencies to the uric acid deposit having long since passed away, then the stone itself must be attacked. Sometimes there is an excessive secretion of phosphates from the kidneys, in other words, a genuine phosphatic diathesis: here the vice in the tissues must be got rid of. Lastly, there may be simply a deficiency of the normal acidity which is essential to the retention of the earthy phosphates in solution until its emission from the bladder. Of all calculi the phosphatic are the most soluble, hence it is better worth while to try to procure solution of the stone in this than in the other varieties of stone. (Dr. G. Harley, p. 103.)

Renal and Urinary Affections.—*Tincture of Perchloride of Iron.*—The tincture of iron possesses most eminently the properties of an astringent, tonic, and styptic, coagulating blood or albumen with which it is brought into contact, and constringing the vessels and tissues to which it is applied. Hence arises its value in the albuminuria of Bright's disease, and in hemorrhage from the kidney, bladder, or urethra. We can only conclude, however, that it acts by its tonic influence on the system, for by analysis it may be proved that neither the iron nor the acid escape by the urine. (Dr. A. H. Hassall, p. 112.)

AFFECTIONS OF THE BONES, JOINTS, ETC.

Exposure of Articular Cartilages in Wounds.—It is generally believed that the exposure of articular cartilages in wounds is dangerous, this cartilage being liable to inflame and prevent the healing of the parts. This is entirely incorrect. Entire bones of the tarsus may be removed, leaving a wound lined with articular cartilage, and yet no bad symptoms occur, and the wound will heal soundly, though of course rather slowly, owing to the nature of the wound. It may be reasonably alleged as probable that a wound in which no bony tissue is exposed is more likely to heal kindly and soundly, and is less liable to pyæmic infection than one where a large surface of cancellous bone is laid bare, with all its cells cut open. Such bone frequently softens and becomes carious, and has to exfoliate; and if this is the case *with* healthy bone, when exposed and divided in operation, it is still more

often the case with bone already softened by low inflammation. (Mr. T. Holmes, p. 132.)

FRACTURE.—*The Immediate Treatment of.*—Out of ninety of the principal surgical hospitals of Great Britain and Ireland, in seven only is the custom of early putting-up of the fracture by starch or other fixed material resorted to. This is no doubt owing to the constriction of the limb produced by the starch bandage as ordinarily employed. This, however, may be avoided by making the starched linen splints in two halves. Let a piece of wet lint be accurately adapted to the outer side of the leg, and over this a series of pieces of bandages diagonally, reaching from the inner edge of the tibia to the inner edge of the calf, or as far as can be reached, the patient lying quiet with the outer side of the limb uppermost. These pieces of bandage must be smeared on their inner surface with a mixture composed of the whites of eight eggs and half a pound of flour. The instep must be accurately covered, also the toes, sole, and heel, not a wrinkle or crease being left. The limb must be left uncovered until next day, by which time the half splint will have become dry and extremely firm and light. The patient must now turn upon the other side and expose the inner side of the limb, and the proceeding must be repeated, care being taken that the lint lining is in advance everywhere of the plastered strips, to prevent adhesion of the two halves. When the second half has dried the edges of the two may be neatly trimmed with scissors, the edges however being allowed to overlap half an inch or so. A roller bandage must now be put on and the splint is complete. Subsequently, when the patients come to exercise and move about, and the size of the limb begins to decrease, chamois leather may be moulded to the interior to produce a softer lining. (Mr. J. Tufnell, p. 120.)

PENETRATING WOUNDS OF THE KNEE-JOINT.—*Irrigation.*—In cases of penetrating wound of the knee-joint, the parts should be kept immovable by being fixed to a back splint sufficiently long, and well padded. The bandage should not cover the joint itself. A continuous dripping of water should now be directed on the part, by means of a few threads of worsted, one end of which is placed in a vessel of water placed three or four feet above the limb. Nothing can be compared to this plan for preventing inflammation of the joint. It may be kept up seven or eight days, and this period may be extended or shortened according to the special requirements of each case. (Dr. W. Newman, p. 129.)

STIFF JOINTS.—*Forcible Extension under Chloroform.*—The chloroform must be given so as to produce complete muscular relaxation, and then, supposing the knee-joint is to be operated upon, let the operator flex the joint somewhat more than it already is flexed, whilst the thigh is held firmly by an assistant. This should be done by a sudden sharp jerk. The adhesions usually give way with a loud noise. The joint must then be forcibly straightened, and if this cannot be accomplished perfectly, a few more sudden jerks may be given, and the extension repeated until the limb is perfectly straight. If, however, it is found that the force to be used is very considerable, it is better not to do too much at one operation. The joint should be firmly bandaged after the operation, and the limb fixed in a good position on a splint. If swelling and heat follow, hot fomentations are the best treatment, but in a large number of cases even this is not required. Strumous cases are decidedly the most unfavourable ones, and a stiff joint is certainly the best result that can be expected. (Mr. W. Adams, p. 135.)

AFFECTIONS OF THE SKIN.

ECZEMA.—The treatment of eczema resolves itself into the treatment of constitutional and local debility. Arsenic is a remedy having a peculiar power in cases of eczema. It is a tonic which gives force to and improves the nutrition of the nervous matter, and [redacted] the actions of the skin, which tend to its nutrition and renovatio[redacted]ith iron, and given in small quantities in the mi[redacted]as there is acute inflammation, water dressing s[redacted]ntly, all

crusts having been removed, the parts should be dressed with strips of lint thickly spread with benzoated ointment of oxide of zinc, which may be kept in place by straps of plaster, or a cotton bandage very evenly applied. Every forty-eight hours it should be unpacked, re-dressed, wiped with a soft napkin to remove crusts, and packed up again for a similar period. If there is much heat the bandage may be sopped with water. If there is much irritability the part should be undressed and washed thoroughly with juniper tar soap, then dried and again dressed as before. At other times pencilling the surface with a weak solution of nitrate of silver in nitric ether is of more service. The chronic forms of eczema owe their thickening and induration to infiltration of the tissues of the skin with serous fluid. This state may be rapidly cured by brushing the part over with caustic potash. (Mr. Erasmus Wilson, p. 181.)

Bichloride of Mercury and Antimony.—Although eczema mostly arises in weak states of constitution, it is in its own essence inflammatory, and therefore, whilst the strength is supported by generous diet, an alterative remedy like bichloride of mercury may be most advantageously given, and even combined with tonics. Antimony is also very useful in some cases; it may be given in combination with the mercury, or, in cases where the skin is heated and inclined to be dry, along with salines. The combination of mercury and antimony will sometimes cure in a week or two, cases of eczema in oldish people in the parts about the anus, which cases frequently will have resisted other treatment for years. One-sixteenth of a grain of bichloride given three times a day with one-eighth of a grain of antimony is a sufficient dose. To infants one-sixtieth of a grain of bichloride should be given three times a day, with a little glycerine, which is a very good demulcent. If given for some months, with occasional intermission, it will mostly cure the complaint. Beef-tea and broth should also be given in addition to the milk, and farinaceous food avoided. External applications must of course be combined with this treatment. (Mr. R. H. Meade, p. 187.)

Eczema of the Eyelids, Conjunctiva, and Cornea.—What is known as ophthalmia tarsi is in reality eczema of the lids, and is frequently associated with eruptions on the skin. Granular lids, lippitudo, strumous ophthalmia, certain forms of simple or catarrhal ophthalmia, keratitis, and strumous keratitis, and certain ulcers on the cornea, are merely varieties of eczematous disease. Unchecked eczema of the eyelids always terminates in lippitudo. The treatment should be directed to eczema, its chief features being a non-stimulating diet and alkaline medicines, with the local use of eczema ointments, and the administration of morphia if there is much photophobia. (Mr. Furneaux Jordan, p. 188.)

Parasitic Skin Diseases.—*Carbolic Acid.*—Carbolic acid is one of the most efficient parasiticides known. It must be used as a lotion after softening the part with warm water. (Dr. Watson, *British and Foreign Medico-Chirurgical Review, January* 1865, *p.* 240.)

Profuse Perspiration.—*Hot Water.*—The application of hot-water to the skin, that is, of water as hot as can be borne without pain, instead of leaving the skin moist leaves it hot and dry. Hence it may be used as a remedy in cases of profuse perspiration. It may be useful to those who though otherwise in good health perspire to a distressing degree in hot weather; also in cases of perspiration of some particular part, as the axillæ, hands, feet, &c. The health in these cases is sure to be improved by free purgation, and by quinine, fresh air, and exercise. In the ordinary night sweat of phthisis it is productive of great relief; but in the true hectic, arising from absorption of decaying pus from some internal organ, it must be confessed to be of no avail. (Dr. R. Druitt, p. 62.)

Scabies.—Boil one part of quick-lime with two parts of sublimed sulphur, in ten parts of water, until the lime and sulphur are perfectly united; during the boiling it must be constantly stirred with a piece of wood, and when the sulphur and lime have combined the fluid is to be decanted, and kept in a well-stoppered bottle. It is to be used for the cure of scabies as follows:—*Rub every part* with it, except the face and scalp, for half an hour, then *wash well* and put on clean linen. A single application will suffice. The

linen must also be soaked in boiling water, and the clothes subjected to a temperature of about 200° Fahr., in an oven for a short time. (Dr. J. F. Nicholls, Mr. T. M. Kendall, pp. 191, 192.)

Tincture of Iodine.—The compound tincture of iodine is the cleanest and surest cure for scabies, when the surface affected is not very extensive. It should be applied once a day with a camel's-hair brush. At the first application the clusters of spots may be painted together, but afterwards it will be necessary merely to touch any fresh ones which may appear. (Dr. J. Lowe, p. 191.)

Oil of Petroleum.—The oil of petroleum instantaneously kills the parasite, while it is, at the same time, a disinfectant against the larvæ which are wont to be found in the wearing apparel and bedclothes. (Dr. Decaisne, p. 191.)

Sycosis.—Foment the chin well with warm water, and then apply a strong alkaline lotion, consisting of an ounce of the bicarbonate of potash to four of distilled water, with a view of dissolving the crusts. The hair must be clipped closely to the level of the skin. Then the parts may be brushed night and morning (after ablution) with the following lotion: R. Glycerinæ, f. 3x.; liquoris potassæ arsenitis, f. 3ij.; M. ut fiat lotio. When the chin becomes red and irritated with the arsenic, it must be discontinued for a time and the alkaline lotion reapplied. (Prof. Laycock, p. 188.)

Creasote Lotions.—The use of a lotion consisting of 50 grammes of water, 50 grammes of alcohol, and 50 centigrammes of creasote twice or three times a day, is a very successful plan of treatment. The amount of creasote in the lotion may be doubled if necessary. Creasote possesses the power of checking the growth of the vegetable cryptogams. (Dr. Masse, p. 190.)

VENEREAL AFFECTIONS.

Chancre.—Both soft and hard chancre are the result of one and the same virus. Whether a soft or hard chancre is produced depends upon the intensity of the virus. If the virus be very intense, a soft chancre results, the intensity of the virus giving rise to so much inflammation as to prevent absorption. The indurated chancre, on the contrary, is the product of a weaker virus, in which case the inflammation developed is not so great as to lay any impediment in the way of absorption into the system taking place. (Dr. D. J. Simpson, p. 216.)

Communicability of Secondary Syphilis.—The question is doubtful, but cases occasionally occur which point decidedly to syphilisation of the female by the seminal fluid without pregnancy, and without any secretion from the skin of the penis. (Dr. W. Allingham, p. 217.)

A husband may communicate syphilis to his wife without himself having at the time a primary chancre, and without the wife being pregnant. For let the parts become diseased, or let them be subject to increased action in a syphilitic person, and then the secretions from those organs have a power which they had not before, and may communicate the disease directly to any part with which they come in contact. So far as we know, however, some increased or diseased action in the organ producing that secretion is a *sine quâ non* to its possessing infecting powers. (Mr. Henry Lee, p. 218.)

Condylomata.—Apply with a brush a solution of twenty-five centigrammes of corrosive sublimate in fifty-two grammes of collodion well shaken together. (Dr. G. Finco, p. 224.)

Gonorrhœa.—*Oil of Red Sandal Wood; and Gurgina Balsam.*—From 20 to 40 minims of the oil of yellow Sandal wood, given three times a day, diluted with three parts of rectified spirit, will usually cause the most marked suppression of the discharge within forty-eight hours. It is equal in efficacy and very often superior to copaiba or cubebs, and is pleasant to the taste. It is obtained by distillation from the wood, which grows in the East Indies. Another remedy of considerable value is Gurgina balsam; it is the product of an immense tree growing in various parts of India. Incisions are made

and heat by fire is applied to the root. It may be given in doses of a teaspoonful three times a day, uncombined. (Dr. T. B. Henderson, p. 225.)

INFANTILE SYPHILIS.—The use of mercurials is quite unnecessary. Chlorate of potash given in two-grain doses thrice daily is all the medicinal treatment required. The nose usually requires well syringing with warm water. (Mr. R. W. Dunn, p. 224.)

INFLAMED SCROFULOUS AND SYPHILITIC GLANDS.—*Iodine.*—Spread laminæ of iodine uniformly on a layer of cotton wadding, and, covering the wadding with a leaf of gelatine larger than itself, apply over the enlarged gland. The circumference of the gelatine should adhere to the skin. The apparatus must be left in its place for from twenty-four to forty-eight hours, and the result will be a phlyctena filled with a thick purulent or bloody serosity. This treatment will generally cause the disappearance of the swelling. (Dr. Prieur, p. 192.)

SYPHILIS.—*Mercury.*—Mercury is of doubtful efficacy in the primary and tertiary stages of syphilis. The pustular syphilides are the least amenable to it, and the scaly, chronic papular, and certain forms of chronic superficial ulceration the most so. For the tertiary stages, marked by diseases of bone and periosteum, unhealthy suppurating sores on the integument, the iodide of potassium is the best remedy. The long array of charges laid against the use of mercury are highly exaggerated. If mercury be exhibited with ordinary care and discrimination, and particularly if it be applied through the skin, there is little or no risk of injury to the constitution. (*Editor of Medical Times and Gazette*, p. 209.)

SYPHILISATION.—During the last eleven years the experiments of M. Sperino, of Turin, have verified the announcement of Auzias Turenne, that the syphilitic taint can be removed by syphilisation. Lately Professor Boeck, of Christiania, has come to the same conclusion. Let a patient suffering from severe constitutional symptoms be inoculated, say on the chest, with matter from an indurated chancre, and be re-inoculated every third day with matter from previous pustules, and the successive crops of pustules will gradually become smaller and at last prove abortive. It is better to use only the matter from an indurated chancre, for it is worked out in fewer generations than matter from a soft chancre. Further, by this means the chance of relapse is reduced to a minimum; for, during the last three years, during which he has confined himself to inoculation from indurated chancre, Professor Boeck has not recorded a single relapse, whereas there was nine and a half per cent. of relapses when matter from a soft chancre was employed. The employment of syphilisation as a prophylactic is revolting, but as a curative means its use is perfectly justifiable. The case of a patient suffering to an alarming extent from symptoms of syphilis and threatened phthisis is related. He was completely cured, and gained three stones in weight. (Dr. D. J. Simpson, p. 215.)

SYPHILITIC ERUPTIONS.—*Their diagnosis.*—The coppery tint of an eruption is frequently fallacious, as this tint is found more or less marked in every case of non-syphilitic psoriasis. Again, syphilitic eruptions do not present it at all at their outset; it is only when they become developed that the coppery tint appears. The absence of itching and smarting is a peculiarity which often aids in the recognition of a syphilide, as does also its tendency to occur in segments of circles, and its preference for the face, occurring frequently at the angles of the mouth, at the inner canthus of the eyelids, in the neighbourhood of the alæ of the nose, and on the forehead near the roots of the hairs. The mixed character of syphilitic eruptions is another means of distinguishing them, pustules, vesicles, and scales being frequently mixed up together. No two diseases are more frequently confounded together than syphilitic lupus and the lupus that occurs in the scrofulous. The presence of cicatrices in the throat will often solve this question. Severe cases of eczema in infants are frequently mistaken for infantile syphilis. (Dr. B. Squire, p. 221.)

SYPHILITIC LICHEN ATTENDED WITH INTENSE PRURITUS.—The opinion *is pretty general* that the absence of itching is one of the most valuable diagnostic signs in syphilis. A case of undoubted syphilitic lichen is related at

p. 223, under the care of Dr. Sisson, at the Royal General Infirmary, in which the pruritus was excessive. It was quite relieved by small doses of bichloride of mercury. (Dr. Sisson, p. 223.)

VACCINO-SYPHILITIC INOCULATION.—The following is well authenticated: Six children were vaccinated from one child. Five out of the six between three and four weeks afterwards had indurated ulcers on the inoculated parts. Each one of these five communicated the disease to members of its own family, so that twenty-three persons were infected from this single source. The mothers and nurses of these children had first a chancre on the breast. The fathers of the children and the fathers of the nurses remained healthy until some form of secondary disease appeared on the private parts of the mothers and nurses, and then the husbands in some cases contracted the disease, which appeared in the form of a primary chancre. (Mr. H. Lee, Dr. Viennois, p. 219.)

AFFECTIONS OF THE EYE.

CATARACT AND ARTIFICIAL PUPIL.—*Dressing for the Eye after Operation for.*—After the operations for extraction of cataract and for artificial pupil the best dressing is a little jewellers' cotton wool placed on the closed lids, and covered with a few turns of flannel bandage. Very gentle compression produces great comfort. (Mr. J. V. Solomon, p. 205.)

EXTRACTION OF CATARACT.—*Mooren's plan.*—In cases where from the state of the patient's health, or the condition of the eyeball itself, the ordinary operation is almost sure to fail, it is better to make a prior iridectomy. The most common causes of failure are great age, complicated with debility, severe cough, asthma, persistent vomiting, or constitutional cachexia. The existence of pupillary adhesions, a diseased or deteriorated condition of the lens or eyeball itself, are also indications for the careful performance of iridectomy. The worse the patient's condition, the longer must the interval between the iridectomy and subsequent extraction be. It is found that this prior iridectomy almost entirely prevents any risk of subsequent inflammation and destruction of the eye. When the cornea is extremely thin, extraction alone is rarely successful; we cannot diagnose this with certainty prior to the completion of the section, but we may infer it when the skin is extremely fine and thin. When the portion of iris removed is from the upper part no deformity or inconvenience results. (Dr. C. Taylor, Dr. A. Mooren. p. 197.)

Jacobson's plan.—The cause of failure after extraction of cataract is inflammation of the eye, leading to suppuration and destruction of the organ. This inflammation always commences in that portion of the iris corresponding to the flap, and compressed by the lens in its exit. This to prevent all risk must be got rid of. In performing the section, Jacobson makes an inferior flap with the line of incision not in the transparent tissue, but in the vascular limbus cornæ, so as to get a larger opening for the lens, and to place the wound nearer than usual to the materials for repair. Nearly all the external wound is made indeed in the conjunctiva. The knife, which must be of a breadth corresponding to the semi-diameter of the cornea, must be withdrawn before making a complete division of the parts, a small central isthmus of conjunctiva alone being left. Von Gräfe's fleam-shaped cystotome is then introduced and the capsule freely opened; and then the isthmus is divided, after the escape of the lens the lower segment of iris is drawn out with proper forceps and cut off close to the ciliary margin. If the cornea be in right position, Arlt's compress is applied immediately; but if it be depressed, five to thirty minutes must be allowed for a re-secretion of aqueous humour. Jacobson is very particular not to operate until the patient is fully under the influence of chloroform, in order that the ocular muscles may be quite passive. (Dr. C. Taylor, M. Jacobson, p. 202.)

Grey Cataract.—Schufte's Method of Scooping out.—With one of Jäger's bent iridectomy knives incise the cornea close to the sclerotic, so as to form a curved slit from three to four lines in extent. With a hook a portion of

iris is then drawn out and removed, a slight belt at the ciliary margin being however left to afford support to the vitreous humour. The capsule of the lens must then be freely opened, and any superficial softened matter coaxed out with the scoop of the ordinary curette. Then Critchett's vectis-spoon must be glided between the posterior surface of the cataract and the hyaloid membrane, and the lens cautiously lifted into the anterior chamber and extracted. Any remaining portions must be carefully fished out with the spoon, while the globe is lightly rubbed with the lids from time to time, so as to wash any fragments concealed behind the iris into the pupillary area. This operation enables us to deal with all varieties of cataract, however dense. The chief risks attending it are, that the hyaloid may be ruptured and the vitreous humour allowed to escape. This operation is at present in great repute at Moorfields. (Dr. C. Taylor, M. Schufte, p. 204.)

Extreme Divergent Strabismus.—The indications to be fulfilled by any proceeding undertaken for the radical cure of *extreme* divergent strabismus, when a result of previous tenotomy of the inner rectus, are:—To give the eye a central position; to restore the length and the proper point of attachment to the adductor muscle, thereby insuring balance of the opposed muscular forces and *permitting normal convergence.* Anæsthesia having been induced, and a self-acting speculum applied, make a rather long perpendicular incision down to the sclerotic, and distant about half a line from the inner edge of the cornea. Dissect the conjunctiva from the eye by means of curved scissors, and make two short incisions through the extremities of the first incision towards the nose. A similar flap is now to be made on the outer side of the cornea, and the external rectus divided. The two flaps must now be united over the centre of the cornea by three fine sutures. The eye now occupies the axis of the orbit. The central suture must be removed on the second or third day, so as to obviate corneitis. At the end of three weeks the conjunctiva should be trimmed of any uneven processes. (Mr. J. V. Solomon, p. 208.)

Prolapse of the Iris.—In some cases the steady and continued contraction of the pupil produced by Calabar bean has succeeded in inducing withdrawal of the protruded iris from the wound. The patient is, however, seldom seen early enough to allow of this treatment. The prolapsed iris should not as a rule be removed, for the wound which has produced it being a punctured one, and inflicted usually with considerable violence, does not unite at once, and remains more or less patulous; hence, if the prolapsed iris is snipped off, the constant drain of the aqueous will often cause an additional protrusion to follow. As the portion of the iris which protrudes is usually not the edge, but a fold, bulged to its fullest extent by the aqueous behind, it is an excellent plan to prick it occasionally with a fine needle, that it may collapse, and permit the edges of the wound to close upon it. In the majority of cases of prolapse from injury, it is the best plan of treatment to close both eyes, and place a little cotton wool or charpie evenly over the injured one, so as to form a compress which is to be kept in its place by a turn of a roller. This often affords considerable comfort, and so long as this is the case it may be worn. (Mr. G. Lawson, p. 196.)

Strumous Ophthalmia with Irritable Vascular Ulcer.—The seton forms a most valuable and almost infallible method of treatment in cases of strumous ophthalmia with irritable vascular ulcer. The seton employed should consist simply of a thread of ligature silk passed double through the skin of the temple, that part under the hair, in order to avoid a visible scar (Mr. W. S. Watson, p. 195.)

Trichiasis.—By the following plan of operation this distressing complaint may be completely cured without leaving even a trace of deformity. Insert under the lid a horn spatula, and commencing at the inner angle of the eye dissect along the upper surface of the tarsal cartilage, separating the skin and eyelashes from the parts beneath. Then remove an elliptical piece of skin from the eyelid, leaving the tarsal margin and eyelashes untouched *and attached* at either end. This tarsal portion must then be approximated *to the upper part* of the eyelid by four or five fine stitches: union will rapidly *take place.* The steps of the operation will be found more in detail at p. *194.* (*Dr. E. P.* Wright, p. 193.)

MIDWIFERY, AND THE DISEASES OF WOMEN, ETC.

CHILD-CROWING.—Let the child be exposed as much as possible in the open air; if it be dry, the colder the better. For many hours daily it must be carried to the most exposed parts of the neighbourhood, of course being sufficiently clothed to prevent its taking cold. This, however, rarely happens. (Mr. J. Roberton, p. 269.)

DISPROPORTION.—*A more Extended Use of Version.*—In all head presentations in which the forceps will not succeed, instead of at once employing craniotomy it is better to try turning, even though there is little hope of extracting by means of it alone. In cases which, without turning, would require craniotomy, the application of forceps to the head, *after turning*, will often be sufficient to effect delivery, with a better result to the mother, and frequently without injury to the child. And even in cases where version does not enable us to avoid craniotomy, its previous employment seems to render that operation easier of performance, without adding in any degree to the risk. The occiput of the child may be perforated behind the ear with the greatest ease, and the hold afforded by the body renders the delivery of the head comparatively easy. (Dr. A. Inglis, p. 231.)

DYSMENORRHŒA.—In cases of painful dysmenorrhœa from contracted os and cervix uteri, it is necessary to enlarge the parts by some operation. Dr. Marion Sims accomplishes this in a somewhat different manner to that pursued by Dr. Simpson. Dr. Sims brings everything into plain view, and holding the uterus downwards by means of a tenaculum adapted for the purpose, divides the cervix by scissors. He tampons the vagina after the operation, for the double purpose of guarding against hemorrhage and ensuring an open os. The details, with explanatory woodcuts, will be found at pp. 262, 263. (Dr. J. Marion Sims, p. 261.)

EARLY PREGNANCY.—*An Additional Symptom of.*—It is in our power occasionally to foretell pregnancy as early as from the sixth week to the ninth. At this period, by auscultation *per vaginam*, a sound may be heard very like the sudden cessation for a moment of a general splenic murmur. It is synchronous with the pulse, and when very distinct, which it sometimes is, it has a very high pitch. It requires the closest attention to detect it, and a very practised ear. The instruments required are two, a glass stethoscope and a wooden vaginoscope. Glass is a beautiful conductor of sound, and does not make any noise when rubbed against the clothes. When made long and with a curve it is very easy of application. (Dr. C. H. F. Routh, p. 227.)

EXCORIATED NIPPLES.—Every time the child has left the nipple apply with a camel's-hair brush, gum arabic in an impalpable powder. This shields the nipple from atmospheric influences from the moment it is applied, and protects the nipple from the excoriating effects attending the act of sucking. (Dr. MacCormack, p. 246.)

FIBROUS TUMOURS OF THE UTERUS.—Fibrous tumours of the uterus are of such low vitality that the slightest interference with them stops their growth. Simple incision of the os and cervix is frequently sufficient to arrest their growth, and the gouging or breaking up of the tumour is unnecessary. Having placed the patient in the lithotomy position, let an assistant depress the perineum well by means of Bozeman's speculum. Let the operator seize the anterior lip of the os uteri by a pair of vulsellum forceps, and then, by means of the hysterotome, cut the os and cervix laterally, first towards one side and then the other. The cut surfaces are then plugged with oiled lint, and the vagina also thoroughly filled with lint or cotton wool, so as to ensure most perfect pressure and exclusion of atmosphere. When this last stage is well carried out there is never any serious hemorrhage. When it is considered advisable to perform the second step of the operation, viz., gouging the tumour, which should not be done *sooner* than a fortnight after the incision of the os and cervix, the patient is again to be placed in the lithotomy position. The index finger of the left hand must now be inserted within the inner os, and a pair of long-handled, blunt-pointed, curved scissors *passed up to the tumour*, and gently pressed through the capsule; the blades

are then to be so opened as to break up the tumour. It is not necessary or advisable to break up much. Oiled lint must then be thoroughly applied as before. (Mr. I. Baker Brown, p. 247.)

Fœtal Head.—*To deliver after Perforation.*—Remove the bones of the calvarium as far as possible, leaving the skin to protect the soft parts of the mother from injury. It is found that the base of the skull and face can then be removed with most ease when the face points anteriorly, and the chin is brought well down. This may be done with facility by using a small blunt hook, which may readily and without chance of injury be passed into the orbit. (Dr. J. Braxton Hicks, p. 236.)

Hemorrhage After Labour or Abortion.—Inject a solution of perchloride of iron into the uterus, after clearing out the cavity of placental remains and clots. The solution should be of the strength of one part of a saturated solution of the salt to eight of water. It not only arrests hemorrhage by coagulating the blood in the mouths of the uterine veins, but it is a splendid antiseptic, and hence reduces the liability to puerperal fever. (Dr. R. Barnes, p. 238.)

Hysterical Paralysis.—*Electricity.*—In cases of hysterical paralysis and anæsthesia the continuous galvanic current, such as is produced by a large number of cells of Bunsen's or Daniell's battery, feebly charged, generally proves curative. This current acts on the nervous centres by reflex action from the sentient nerves of the skin. This action is not possessed by the interrupted current. For local ailments invading the peripheral nerves the interrupted current is more beneficial. (Dr. Savage, p. 48.)

Labour Pains.—The pain attendant on labour is more neuralgic in its character than owing to the specific muscular act itself. The pain of labour may be relieved by the application of cold to the abdomen, by means of a freezing mixture, or pounded ice contained in a flat tin box. The pains are if anything increased in intensity, without any corresponding increase in the attendant suffering, the actual pain being sensibly diminished. (Dr. J. M. Granville, p. 233.)

Ovariotomy.—How to deal with the pedicle is the great difficulty. Dr. Tyler Smith transfixes it with a double silk ligature, cuts off the ends of the latter, and allows the whole to drop into the pelvis. Mr. Spencer Wells prefers the use of the clamp when the pedicle is long. Dr. J. Braxton Hicks believes it would be possible, by firmly compressing the pedicle for an inch or so by a serrated clamp or some such apparatus, to condense the tissues so firmly as to completely prevent bleeding, and so do away with the use of either a clamp or suture. (p. 251.)

There have hitherto been three methods of securing the pedicle in cases of ovariotomy: first, by ligature, allowing the ends to hang out; second, by cutting off the ligature short and closing the wound; and, third, by the clamp. Mr. Baker Brown has lately adopted a fourth plan, and with successful results in the case in which it was tried. The pedicle being secured by a clamp, it was thoroughly seared by the actual cautery and allowed to drop into the abdomen. The wound was healed in a week. The cautery should not be quite at a white heat. (Mr. I. Baker Brown, p. 251.)

Pruritus Pudendi.—One or two superficial applications of lunar caustic promptly allay the irritation of pruritus pudendi. (M. Dieudonné, *Glasgow Medical Journal, April* 1865, *p.* 126.)

Sickness of Pregnancy.—In the morning, an hour before rising, let the patient drink about three ounces of very strong, hot decoction of coffee, and during the day let liquid food be taken frequently in small quantities. If this fails, try in conjunction five drops of tincture of iodine in a teaspoonful of cold water every two hours. Should everything fail to give relief, the patient may be sustained by nutritive enemata for a time, that the irritable state of stomach may have time to subside. (Dr. J. P. Cassells, p. 229.)

Transfusion of Blood.—A description of a very perfect apparatus for the transfusion of blood is given at p. 237. (Dr. Graily Hewitt.)

Uterine Polypi.—*A modification of Chassaignac's Ecraseur.*—When *the polypus has* descended into the vagina, Dr. Braxton Hicks' cord of wire *answers very* well, but when the polypus is intra-uterine we cannot apply it.

nor can the chain of Chassaignac be applied on account of its want of rigidity. This difficulty may be overcome by adding to Chassaignac's instrument a porte-chaine, which may be described as a pair of dilating forceps with spring blades, which render the chain stiff, so that it may be passed straight into the vagina, or into the cavity of the uterus as easily as we would a sound or a sponge probang; after which the chain is expanded by the blades of the forceps or porte-chaine. (Dr. J. Marion Sims, pp. 260-1.)

VAGINAL INJECTIONS.—*Rasch's Instrument for.*—An efficient instrument for a vaginal douche may be made from a piece of strong but small india-rubber tubing, about six feet long. It is to be used as a syphon, and has, at the end to be immersed in the basin of water or lotion, a heavy leaden end, and at the vaginal extremity a piece of thick elastic bougie. (Dr. Adolph Rasch, p. 267.)

VESICO-VAGINAL FISTULA.—It is extremely difficult to see and bring within easy reach of the knife the edges of a very small fistula, particularly when situated very high up in the vagina. Dr. Banon, of Dublin, has contrived an instrument to obviate this difficulty. It is a curved steel sound or probe, provided with four elongated points projecting backwards. It may easily be forced into the smallest aperture, and on withdrawing it, will grapple the margin of the fistula on every side, and enable the operator to divide it with the greatest ease. (Dr. A. P. Banon, p. 268.)

MISCELLANEA.

ALKALOIDS OF OPIUM.—Of the alkaloids of opium three only possess the property of inducing sleep; these are, morphia, narceine, and codeia. The sleep produced by the sub-cutaneous injection of codeia differs essentially from that produced by morphia. Profound stupor cannot be produced by codeia, but the effect is more that of being calmed, and soothed to natural sleep. Animals do not awake after the administration of codeia as they do after that of morphia, in a state of terror, and their natural disposition altered. Narceine is the most somniferous substance contained in opium, and it acts more powerfully with equal doses than does codeia, the sensibility to noises which characterises the sleep produced by the latter being absent. A profound calm and absence of insensibility are alone produced by narceine. As a poison, thebaïne, although not a narcotic, is much more dangerous than is morphia. Codeia is also more poisonous than morphia, but infinitely less so than thebaïne. (M. Claude Bernard, *Edinburgh Med-Journal, Nov.* 1864, p. 454.)

Therapeutic Uses of the Alkaloids of Opium.—Narcotine has an action on the nervous system the opposite to that of morphia, being that of an excitant. Administered in doses of five or ten centigrammes in cases where the vital powers are failing, either after surgical operations, in paralytics, or in dying persons, it will cause the strength of the pulse and the animal heat to increase. It should be given in doses of one grain daily. Thebaïne is an excitant of the upper portion of the spinal cord. It has not hitherto been of any use therapeutically, the agitation and sleeplessness which follow its administration preventing any good result. Narceine is a valuable calmative, in ordinary doses of from one to two grains producing calm and comfort, but not sleep. It appears to have an especial action on the lumbar portion of the spinal cord, producing relief in cases of painful paraplegia. (Dr. Ozanane, *Edinburgh Medical Journal, Nov.* 1864, p. 459.)

DIET OF CHILDREN.—The diet of children of all ages should be, a substantial breakfast, with animal food in some shape; a substantial dinner of meat, vegetables, and cereal pudding; and a substantial supper, also consisting in part of animal food. The drink may be milk, tea, cocoa, and possibly beer. This is the diet of health; a diet capable of making a strong body and also a strong mind; and a diet capable of preventing disease. Rice and amylaceous food should have no place in the diet of health, but

should be reserved for the sick-room. (Mr. E. Wilson, *Med. Times and Gaz., Jan.* 7, 1865, p. 25.)

Baron Liebig's Soup for Children.—Cow's milk does not adequately represent the milk of a healthy woman, and when wheaten flour is added, although starch is not unfitting for the nourishment of the child, the change of it into sugar in the stomach during digestion imposes an unnecessary labour on the organism, which will be spared if the starch be beforehand transformed into the soluble forms of sugar and dextrine. This can be effected by adding to the wheaten flour a certain quantity of malt. The following is the best way of preparing this food:—Half an ounce of wheaten flour, and an equal quantity of malt flour, seven grains and a quarter of bicarbonate of potash, and one ounce of water, are to be well mixed; five ounces of cow's milk are then to be added, and the whole put on a gentle fire; when the mixture begins to thicken it is removed from the fire, stirred during five minutes, heated and stirred again till it becomes quite fluid, and finally made to boil. After the separation of the bran by a sieve, it is ready for use. By boiling it for a few minutes it loses all taste of the flour. (*Lancet, Jan.* 7, 1865, p. 17.)

Endoscope.—The endoscope is an instrument by means of which any viscus or part of the body into which a straight tube can be introduced, may be examined by the eye of the surgeon. The interior of the urethra, bladder, uterus, nasal passages, and rectum, may be examined minutely, and changes in their appearances determined with accuracy, and the treatment of many obscure cases rendered certain and successful. It may be obtained of Fannin and Co., Dublin. (Dr. Cruise, p. 178.)

Wire-Rope Ecraseur.—The wire-rope écraseur is not intended to compete with the chain ecraseur, although it is nearly as strong; but it is constructed to be used in places where the chain cannot be used, at the fundus of the uterus, or on the cervix, *in situ*, &c. The most common fault of makers is to leave the eye of the shaft sharp-edged instead of perfectly smooth; this causes the rope to snap over heavy work. Another fault is to twist the wires too evenly and firmly; this prevents its readily cutting through large firm growths. (Dr. J. Braxton Hicks, p. 249.)

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